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AMERICAN SOCIETY
FOR
TESTING MATERIALS.

AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROCEEDINGS

OF THE

FOURTEENTH ANNUAL MEETING

Held at Atlantic City, New Jersey,
June 27–July 1, 1911.

VOLUME XI.

EDITED BY THE SECRETARY,
UNDER THE DIRECTION OF THE COMMITTEE ON PUBLICATIONS.

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Atlantic City, N. J., June 27–July 1, 1911.

The Fourteenth Annual Meeting of the American Society for Testing Materials was held at the Hotel Traymore, Atlantic City, N. J., on June 27–July 1, 1911. The total registered attendance at the meeting, including 92 ladies and 61 guests, was 454.

The following members were present or represented at the meeting:

Abraham, Herbert.
Adams, H. C.
Aertsen, Guilliaem.
Aiken, W. A.
Ajax Metal Company,
  G. H. Clamer.
Akin, Thomas B.
Allan, Andrew, Jr.,
  William T. Allan.
American Asphaltum and Rubber Company, H. B. Pullar.
American Brass Company,
  William H. Bassett.
American Bridge Company,
  C. C. Schneider.
American Foundrymen's Association, Richard Moldenke.
American Locomotive Company,
  S. V. Hunnings.
American Steel and Wire Company,
  J. F. Tinsley.
Armstrong, T. P.
Ashby, E. B.
Atwood, J. A.
Aupperle, James A

Backert, A. O
Baldwin Locomotive Works,
  G. H. Woodroffe.
Barrett Manufacturing Company,
  William S. Babcock.
Bates, P. H.
Bausch and Lomb Optical Company, H. E. Howe.
Baxter, Florus R.
Beale, H. A., Jr.
Beall, Frank F.
Beck, Wesley J.
Belden, A. W.
Bent, H. B.
Berry, H. C.
Bethlehem Steel Company,
  L. H. Atkinson.
Blair, Will P.
Blanchard, Arthur H.
Bole, William A.
Bonzano, A.
Boston Elevated Railway Company, J. Walter Allen.
Bowman, L. H.
Boynton, C. W.
Boynton, Henry C.
Bridgeport Brass Company,
  William R. Webster.

Brown, John G.
Buck, D. M.
Buffalo, Rochester and Pittsburg
  Railway Company,
  H. G. Burnham.
Burgess, Charles F.
Burrows, Charles W.
Bush, B. F.,
  Floyd F. Chadwick.

Cambria Steel Company,
  E. F. Kenney.
Camp, J. M.
Campbell, William.
Capp, J. A.
Carnegie Steel Company,
  C. F. W. Rys.
Carney, F. D.
Carpenter, A. W.
Carpenter Steel Company,
  J. H. Parker.
Chamberlain, G. D.
Charls, G. H.
Christie, A. J.
Christie, James.
Church, Sumner R.
Churchill, Charles S.
Cobb, E. B.
Coggeshall, G. W.
Colby, Albert Ladd.
Colby, J. Allen.
Colorado Fuel and Iron Company,
  Jacob Cambier.
Condit, E. A., Jr.
Cook, Edgar S.
Cooper, James B.
Corse, W. M.
Corson, C. E.
Crocker Wheeler Company,
  P. S. Dellenbaugh, Jr.
Crosby, W. W.
Cutts, William C.
Cushing, W. C.
Cushman, Allerton S.

Davies, George C.
De Pierrefeu, A. D.
Derby, W. A.
Dewar, John.
Dewey, Bradley.
De Wyrall, Cyril.
Dixon Crucible Company,
  M. McNaughton.
Douty, D. E.
Dudley, P. H.

Electrical Testing Laboratories,
  F. M. Farmer.
Elliott, George K.
Engineering Record,
  John M. Goodell.

Farmer, F. M.
Fay, Henry.
Ferguson, Lewis R.
Fireman, Peter.
Fisher, Henry W.
Fleming, William R.
Force, H. J.
Forrest, C. N.
Fort, E. J.
Fowler, George A.
  (*Railway Age Gazette*).
Froehling and Robertson,
  Andrew Robertson.
Fuller, William B.
Fulweiler, W. Herbert.

Gardner, Henry A.
General Electric Company,
  J. A. Capp.
Gibboney, James H.
Glasgow Iron Company,
  James P. Roe.
Goldbeck, Albert T.
Goodspeed, G. M.
Gormully, A. R.
Greenman, Russell S.
Gregory, E. D.
Griffith, R. E.

Haas, Frank,
  R. E. Rightmire.
Hall, John H.

Hammond, George T.
Hancock, E. L.
Harris, J. R.
Hartford Steam Boiler Inspection
    and Insurance Company,
    S. B. Adams.
Hartranft Cement Company, W. G.,
    W. G. Hartranft.
Hastings, C. L.
Heckel, G. B.
Heidenreich, E. Lee.
Henry, J. A.
Hering, Rudolph.
Hibbard, Henry D.
Hilles, R. W.
Holst, J. L.
Hopkins, George A.
Howard, James E.
Howe, Henry M.
Hubbard, Prévost.
Hume, A. P.
Humphrey, Richard L.
Hunnings, S. V.
Hunt Company, Robert W.,
    Robert W. Hunt.

Ickes, E. T.
Illinois Central Railroad Company,
    J. S. Sheafe.
Illinois Steel Company,
    P. E. Carhart.
Ingalls, F. P.
Ingberg, S.
Iron Age, The,
    W. W. Macon.
Iron Trade Review, The,
    A. O. Backert.

Jarecki, Alexander.
Jeffers, John M.
Jennings, R. E., 2d.
Job, Robert.
Johnson, Charles W.
Jones and Laughlin Steel Company,
    Jesse J. Shuman.
Jones, C. R.
Jones, Jesse L.
Jones, Morgan T.

Kaufman, Gustave.
Kelsey, V. V.
Kenney, E. F.
Kershaw, William H.
Kinkead, J. A.
Kinney, William M.
Kirchoff, C.
Kirschbraun, L.
Koch, George B.
Kohr, Donald A.

Langenbach, E. O.,
    John N. Remsen.
Lanza, Gaetano.
Larned, E. S.
Lawson, T. R.
Lazell, E. W.
Leech, J. O.
Lesley, Robert W.
Lewis, A. W.
Lindemuth, L. B.
Linder, Oscar.
Little, Incorporated, Arthur D.
    Carl F. Woods.
Locke Insulator Manufacturing
    Company, The,
    Walter T. Goddard.
Lovell, Alfred.
Lowe Brothers Company, The,
    Donald A. Kohr.
Lucas and Company, John,
    Leo P. Nemzek.
Lukens, Alan N.
Lum, D. W.
Lynch, T. D.

Macgregor, James S.
Macnichol, Charles.
MacPherran, R. S.
Mahon, R. W.
Mannhardt, Hans.
Marburg, Edgar.
Marston, A.

Masury and Son, John W.,
William S. Baldwin.
Mathews, John A.
McCleary, E. T.
McDonnell, M. E.
McFarland, G. S.
McGrady, J. W.
McKibben, Frank P.
Leon D. Conkling.
McLeod, John.
McNaugher, D. W.
Mead, Charles A.
Meade, Richard K.
Meriwether, Coleman.
Merriman, Mansfield.
Metal Industry, The,
L. J. Krom.
Midvale Steel Company,
Radclyffe Furness.
Miller, Rudolph P.
Moissieff, Leon S.
Moldenke, Richard.
Morris, D. A.
Myers, J. E.

National Tube Company,
Frank N. Speller.
Neal, C. S.
New Jersey Zinc Company,
George C. Stone.
Norris, George L.

Olsen, Thorsten Y.
Olsen, Tinius,
Thorsten Y. Olsen.
Orton, Edward, Jr.
Outerbridge, Alexander E., Jr.

Page, L. W.
Page, W. Nelson,
Raleigh C. Taylor.
Pease, Burton H.
Penniman and Browne,
W. B. D. Penniman.
Pennsylvania Crusher Company,
G. W. Burton.
Pennsylvania Steel Company,
H. B. Bent.
Perley, George E.
Perry, R. S.
Pickard, Glenn H.
Polk, Anderson.
Porter, J. Madison.
Prentiss, George N.
Price, Charles P.
Provost, A. J., Jr.

Quimby, Henry H.

Railway and Engineering Review, The, Willard A. Smith.
Railway Steel Spring Company,
Alan N. Lukens.
Ramage, J. C.
Reading Iron Company,
George Schuhman.
Reinke, W. B.
Remington Arms and Ammunition Company, Nathan A. Chase.
Riehlé, Frederick A.,
William J. Tretch.
Rights, Lewis D.
Rinald, C. D.
Robinson, F. W.,
Roy W. Lindsay.
Rodgers, S. M.
Roebling's Sons Company, John A.,
Henry C. Boynton.
Rogers, Allen.
Rossi, James C.
Royal, Joseph.
Rys, C. F. W.

Sabin, A. H.
Sargent, George W.
Sauveur, Albert.
Schall, Frederick E.
Schmitt, F. E.
(*Engineering News*).
Schneider, C. C.
Scott, Arthur P.
Scott, R. G., Jr.

Sellers and Company, Incorporated, William, Alexander E. Outerbridge, Jr.
Sharples, Philip P.
Sheafe, J. S.
Sherwin-Williams Company, The, Edward C. Holton.
Shore, Albert F.
Shuman, Jesse J.
Skinner, C. E.
Smart, L. A.
Smith, Earl B
Smith, H. E.
Snow, J. P.
Spackman Engineering Company, H. S., Henry S. Spackman.
Spencer, Herbert.
Sperry, W. L.
Spengler, J. H.
Stafford, C. Edward.
Stafford, Samuel G.
Standard Underground Cable Company, Henry W. Fisher.
Standard Steel Works Company, A. A. Stevenson.
Starr, John J.
Stevenson, A. A.
Stone, George C.
Stoughton, Bradley.
Studebaker Library, The, J. A. White.
Swensson, Emil.

Talbot, A. N.
Taylor, Knox.
Taylor, W. Purves.
Tefft, G. H.
Testing Laboratory, City of St. Louis, Mont Schuyler.
Thomas, George, 3d.
Thomas, H. C.
Thompson, G. W.
Thompson, Hugh L.
Thompson, Sanford E.
Thorpe, J. B.
Tiemann, Hugh P.
Tretch, William J.
Tilt, Edwin B.
Toch, Maximilian.
Trautwine, John C., Jr.
Turneaure, F. E.

Unger, J. S.
United Gas Improvement Company, W. Herbert Fulweiler.

Van Gundy, C. P.
Voorhees, S. S.

Waldo, Leonard.
Walker, Percy H.
Walker, William H.
Wallace, John T.
Webster, George H.
Webster, William R.
Western Electric Company, Oscar Linder.
Westinghouse Air Brake Company, H. C. Loudenbeck.
Westinghouse, Church, Kerr and Company, C. M. Chapman.
Westinghouse Electric and Manufacturing Company, C. E. Skinner.
West Virginia University Library, C. R. Jones.
Wetherill, C. T.
Wetherill, Samuel.
Weymouth, F. A.
White, Alfred H.
White, G. A.
White, G. D.
Whittemore, H. L.
Wickhorst, M. H.
Wig, Rudolph J.
Wilhelm Company, The A., E. C. Street, Jr.
Wilson, Percy H.
Winchester Repeating Arms Company, Rupert L. Penney.
Wittman, N. B.
Wood and Company, R. D., Walter Wood.
Wood, Walter.
Woodroffe, G. H.
Worcester, J. R.

Worcester Polytechnic Institute,
E. L. Hancock.
Wyman and Gordon Company, The,
G. S. McFarland.
Young, J. Bertram.
Zehnder, C. H.
Zimmerschied, K. W.

Total number, 329; attendance, not including companies, etc., whose representatives also hold membership in their own name, 301.

First Session.—Tuesday, June 27, 3 p. m.

President Henry M. Howe in the chair.

The minutes of the Thirteenth Annual Meeting were approved as printed.

The annual report of the Executive Committee was adopted as printed.

In the absence of Mr. Walter Wood, Chairman of Committee A-3 on Standard Specifications for Cast Iron and Finished Castings, the report of that Committee was presented by Secretary Marburg. Mr. Marburg stated that he had been authorized by Mr. Wood to state that the Committee desired no action taken at this time on the proposed revised Standard Specifications for Locomotive Cylinders, and that the Committee recommended that these specifications be printed in the Proceedings with a view of their adoption in their present or modified form at the next annual meeting of the Society.

On motion, this recommendation of the Chairman of Committee A-3 was approved.

Mr. J. A. Capp, Chairman of Committee B-1 on Standard Specifications for Hard-Drawn Copper Wire, presented the report of that Committee including the proposed amendments to the Standard Specifications for Hard-Drawn Copper Wire.

On motion, these amendments were referred to letter ballot by the prescribed two-thirds vote of those voting.

Mr. Rudolph Hering, Chairman of Committee C-4 on Standard Specifications and Tests for Clay and Cement Sewer Pipes, presented the report of that Committee.

The tellers, Mr. Richard L. Humphrey and Mr. J. A. Capp, appointed by the Chair to canvass the vote on the election of members of the Executive Committee, reported that 131 ballots had been cast, and in accordance with their report the Chair declared the election of Mr. W. A. Bostwick, Mr. Robert W. Hunt, Mr. Richard Moldenke and Mr. William R. Webster to membership on the Executive Committee.

The meeting then adjourned till 8 P. M.

SECOND SESSION.—TUESDAY, JUNE 27, 8 P. M.

President Henry M. Howe in the chair.

The Chairman invited Mr. Robert W. Lesley, Vice-President of the Society, to the chair during the presentation of the Annual Presidential Address under the title of: "The American Society for Testing Materials."

The following papers were then read and discussed:

"The Manufacture of Pure Irons in Open-Hearth Furnaces." Allerton S. Cushman.

"Measured Strains on Engineering Structures." James E. Howard.

"A Study of the Heat Treatment of Some Low-Carbon Nickel Steels." Henry Fay and John M. Bierer.

"Flue-Sheet Cinder Formation in Locomotives." Robert Job.

The meeting then adjourned till the following morning.

THIRD SESSION.—WEDNESDAY, JUNE 28, 10 A. M.

On Steel.

President Henry M. Howe in the chair.

In the absence of Mr. William R. Webster, Chairman of Committee A-1 on Standard Specifications for Steel, the report of that Committee was presented by Mr. A. A. Stevenson, Vice-Chairman. Mr. Stevenson called attention to a typographical error in the designation of the upper limit of the carbon content in the proposed Standard Specifications for Forged and Rolled, Forged, or Rolled Solid Steel Wheels for Engine Truck, Tender and Passenger, Subway and Elevated Railway Service, which should be

0.85 instead of 0.80. Mr. Stevenson moved that the four proposed specifications appended to the report of Committee A-1 for:

"Forged and Rolled, Forged, or Rolled Solid Steel Wheels for Engine Truck, Tender and Passenger, Subway and Elevated Railway Service;"

"Forged and Rolled, Forged, or Rolled Solid Steel Wheels for Freight Car Service;"

"Heat-Treated Carbon Steel Axles, Shafts, and Similar Parts;"

"Steel Reinforcement Bars;"

be referred to letter ballot of the Society.

Mr. Charles S. Churchill moved that the proposed Specifications for Wheels for Freight Car Service and Wheels for Passenger and Other Service be referred back to Committee A-1 for further consideration. This motion was carried.

On motion, the proposed Standard Specifications for Heat-Treated Carbon Steel Axles, Shafts, and Similar Parts, were referred to letter ballot of the Society.

Mr. Albert Ladd Colby moved that the paragraph in the proposed Standard Specifications for Steel Reinforcement Bars relating to check analyses be changed to the following form in order to make this clause consistent with those in other standard specifications for steel: "A check analysis may be made by the purchaser or his representative if desired, in which case an excess of 25 per cent. above the required limits will be allowed."

On motion, the proposed Standard Specifications for Steel Reinforcement Bars as thus amended were referred to letter ballot of the Society.

Mr. Albert Ladd Colby called attention to the desirability of substituting the word "Steels" for "Steel" in the designation of the titles of Sub-Committees VIII and XI in the report of Committee A-1. The Secretary was instructed to make that change.

The President then invited Mr. Mansfield Merriman, Past President of the Society, to the chair.

The report of Committee A-4 on Heat Treatment of Iron and Steel, was presented by Mr. Henry M. Howe, Chairman. Mr. Robert W. Hunt moved that this report be printed both in the Proceedings and the Year Book for the information of the Society. Mr. Howe offered an amendment to this motion to the effect that the proposed Standard Specifications for Annealing

Miscellaneous Rolled and Forged Carbon-Steel Objects be referred to letter ballot of the Society as "Recommended Practice." This amendment was carried, as well as the original motion thus amended.

The following papers were read by title:

"Life History of Network and Ferrite Grains in Carbon Steel." Henry M. Howe.

"The Heat Treatment of a Steel containing 3.15 per cent. Nickel and 0.27 per cent. Carbon." William Campbell and H. B. Allen.

Mr. William Campbell, Chairman of Committee B-2 on Non-Ferrous Metals and Alloys, presented the report of that Committee embodying proposed Standard Specifications for:

"Copper Wire Bars, Cakes, Slabs, Billets, Ingots, and Ingot Bars."

"Spelter."

"Manganese Bronze Ingots."

On the recommendation of the Committee, as voiced by Mr. Campbell, these three proposed specifications were referred to letter ballot of the Society.

The following papers were then read and discussed:

"A Note upon the Heat Treatment of an Acid and a Basic Open-Hearth Steel of Similar Composition." Henry Fay.

"Studies on Steel Tires." Robert Job and Milton L. Hersey.

A paper on "Ductility in Rail Steel," by Mr. P. H. Dudley, was read by title.

The meeting then adjourned till 8 P. M.

FOURTH SESSION.—WEDNESDAY, JUNE 28, 8 P. M.

On Preservative Coatings.

President Henry M. Howe in the chair.

On behalf of the Executive Committee the Secretary introduced the following motions:

First: That in view of the Sixth Congress of the International Association for Testing Materials, which will be held in this country in September, 1912, the next annual meeting be held in June, 1912, and that it be limited to the annual reports of the standing technical committees and administrative business.

Second: That the Executive Committee be authorized to effect a financial arrangement with the Council of the International Association by which every Member of the American Society will receive, without extra charge, a copy of the English edition of the Proceedings of the Sixth Congress, and that every Junior Member of the American Society will receive a copy of these Proceedings on the extra payment of $2.50.

The Secretary explained that the American members of the International Association would by this arrangement receive also advance copies of the Congress Proceedings to which non-members of that Association would not be entitled.

These motions were carried by unanimous vote with the understanding that they will become effective only if confirmed by letter ballot of the Society at large.

On the invitation of the President, Mr. Allerton S. Cushman then assumed the chair.

Mr. S. S. Voorhees, Chairman of Committee D-1 on Preservative Coatings for Structural Materials, presented the annual report of that Committee.

Mr. Cushman then yielded the chair to Mr. Edgar Marburg during his presentation as Chairman of Committee A-5 on the Corrosion of Iron and Steel, of the annual report of that Committee.

The following papers were then read and discussed:

"Further Results of the Westinghouse, Church, Kerr and Company Paint Tests." C. M. Chapman.

"The Value of the Sulphuric Acid Corrosion Test." C. M. Chapman.

"The Marked Influence of Copper in Iron and Steel on the Acid Corrosion Test." William H. Walker.

A paper entitled "The Practical Testing of Drying and Semi-Drying Paint Oils," by Mr. Henry A. Gardner, was read by title.

Mr. Alexander E. Outerbridge, Jr., presented a paper on "A Novel Method of Detecting Mineral Oil and Resin Oil in Other Oils."

The meeting then adjourned till the following morning.

FIFTH SESSION.—THURSDAY, JUNE 29, 10 A. M.

On Cement and Concrete.

Vice-President Robert W. Lesley in the chair.

A paper entitled "Practical Tests of Sand and Gravel Proposed for Use in Concrete," was read by the author, Mr. Russell S. Greenman.

A paper on "Some Experiments on the Incrustation and Absorption of Concrete" was, in the absence of the author, Mr. A. O. Anderson, read by Mr. A. Marston.

The following papers were, in the absence of their authors, read by title:

"The Determination of Stresses in a Reinforced Concrete Member Subject to Axial Load and Flexure." S. Ingberg.

"The Properties of Magnesium Cement, Mortars and Concretes." C. Derleth, Jr., and A. C. Alvarez.

The following papers were then read and discussed:

"The Expansion and Contraction of Concrete while Hardening." Albert T. Goldbeck.

"Destruction of Cement Mortars and Concrete through Expansion and Contraction." Alfred H. White.

"The Effect of High-Pressure Steam on the Crushing Strength of Concrete." Rudolph J. Wig.

The report of Committee C-3 on Standard Specifications for Paving and Building Brick, was presented by Mr. D. E. Douty, Chairman.

A paper by Messrs. Edward Orton, Jr., and M. W. Blair on "A Study of the Rattler Test for Paving Brick," was read and discussed.

The meeting then adjourned till 3 P. M.

SIXTH SESSION.—THURSDAY, JUNE 29, 3 P. M.

On Steel.

Mr. Robert W. Hunt in the chair.

The report of Committee A-7 on the Tempering and Testing of Steel Springs and Standard Specifications for Spring Steel, was read by Secretary Marburg in the absence of the chairman of the Committee, Mr. Henry Souther.

The following papers on Hardness Tests were then read, and discussed jointly:

"Hardness Tests." Bradley Stoughton and J. S. Macgregor.

"A Comparison of Five Methods of Hardness Measurement." R. P. Devries.

"Hardness in its Relation to Other Physical Properties." R. P. Devries.

"The Property of Hardness in Metals and Materials." Albert F. Shore.

Mr. G. Lanza, Chairman of Committee E-1 on Standard Methods of Testing, presented the report of that Committee.

On motion, the recommendation of the Committee that the proposed Standard Methods for Transverse Tests be referred to letter ballot of the Society for adoption was approved by the prescribed two-thirds vote of those voting.

The meeting then adjourned till the following morning.

SEVENTH SESSION.—FRIDAY, JUNE 30, 10 A. M.

On Bitumens.

President Henry M. Howe in the chair.

The report of Committee D-4 on Standard Tests for Road Materials, was read by Mr. Prévost Hubbard, Secretary of the Committee. The recommendation of the Committee that the following methods of tests be referred to letter ballot of the Society as "Provisional Methods" was on motion approved:

"Provisional Method for the Determination of Soluble Bitumen."

"Provisional Method for the Determination of the Penetration of Bitumen."

"Provisional Method for the Determination of the Loss on Heating of Oil and Asphaltic Compounds."

"Provisional Method of Sizing and Separating the Aggregate in Asphalt Paving Mixtures."

The following papers were then read and discussed:

"A New Consistometer for Use in Testing Bituminous Road Materials." W. W. Crosby.

"Improved Instruments for the Physical Testing of Bituminous Materials." Herbert Abraham.

"Apparatus for Determining the Drop Point and Softening Point of Compounds." Henry W. Fisher.

A paper on "Organic Residues from Soluble Bitumen Determinations," by Messrs. Prévost Hubbard and C. S. Reeve, was read by title.

A paper on "The Behavior of Zinc under Compression" was read by the author, Mr. John C. Trautwine, Jr.

The following papers were then read by title:

"Some Tests on the Rate of Corrosion of Metals Exposed in a Gaseous Atmosphere." A. W. Carpenter.

"Some Causes of Failures in Metals." Henry Fay.

"Asphalt Cement for Use in Sheet Asphalt Pavement: Physical and Chemical Tests to Determine Quality." J. W. Howard.

The following amendment of the by-laws, proposed in the annual report of the Executive Committee, was on motion approved and referred to letter ballot of the Society:

ARTICLE V.

Strike out Sections 1 and 2, viz:

> SECTION 1. The fiscal year shall commence in 1910 on the first of August, and the dues from August 1, 1910, to December 31, 1910, shall be $5.00 for Members and $2.50 for Junior Members.
>
> SEC. 2. The fiscal year after December 31, 1910, shall begin on the first of January, and the annual dues from and after January 1, 1911, shall be $10.00 for Members and $5.00 for Junior Members, payable in advance.

Substitute the following new section:

> SECTION 1. The fiscal year shall commence on the first of January. The annual dues shall be $10.00 for Members and $5.00 for Junior Members, payable in advance.

Change the designation of existing Sections 3, 4, 5, 6 and 7, to Sections 2, 3, 4, 5 and 6, respectively.

The meeting then adjourned till 3 P. M.

EIGHTH SESSION.—FRIDAY, JUNE 30, 3 P. M.

On Testing Apparatus and Methods.

President Henry M. Howe in the chair.

The report of Committee A-6 on the Magnetic Testing of Iron and Steel, was presented by the Chairman, Mr. Charles W. Burrows.

On motion, the recommendation of the Committee that the proposed Standard Magnetic Tests of Iron and Steel be referred to letter ballot of the Society for adoption, was approved by the prescribed two-thirds vote of those voting.

The following papers were then read and discussed:

"A New Method of Testing the Endurance of Case-Hardened Gears and Pinions." J. S. Macgregor and Bradley Stoughton.

"New Types of Impact Testing Machines for Determining Fragility of Metals." T. Y. Olsen.

"A New Type of Autographic Transverse Testing Machine for Research Testing or Regular Foundry Practice." T. Y. Olsen.

"Standard Methods for Testing Sewer Pipe and Drain Tile." A. Marston.

The meeting then adjourned till the following morning.

NINTH SESSION.—SATURDAY, JULY 1, 10 A. M.

Miscellaneous.

Mr. A. N. Talbot in the chair.

A paper on "The Fritz Engineering Laboratory of Lehigh University" was, in the absence of the author, Mr. Frank P. McKibben, read by Mr. L. D. Conkling.

A paper on "A Study of the Elastic Properties of a Series of Iron-Carbon Alloys," by Messrs. C. R. Jones and C. W. Waggoner, was read by Mr. Jones.

In the absence of the authors the following papers were then read by title:

"Specifications and Grading Rules for Douglas Fir Timber: an Analysis of Forest Service Tests on Structural Timbers." McGarvey Cline.

"The Brinell Ball Test Applied to Wood." W. K. Hatt.

"Notes on Anti-Friction Alloys." William Campbell.

The following paper was read and discussed:

"Some Further Experiments upon the Absorption, Porosity, and Specific Gravity of Building Brick." D. E. Douty and L. L. Beebe.

On behalf of Mr. J. A. Holmes, Chairman of Committee D-5 on Standard Specifications for Coal, the report of that Committee was read by Mr. A. W. Belden. This was followed by the presentation of a paper on "Fuel Investigations, Bureau of Mines," by Mr. A. W. Belden.

By permission of the Chair, Mr. G. L. Fowler introduced a discussion of a paper read at the preceding meeting on "A New Method of Testing the Endurance of Case-Hardened Gears and Pinions," by Messrs. J. S. Macgregor and Bradley Stoughton.

In the absence of the Chairman, Mr. C. E. Skinner, the report of a committee appointed by the Executive Committee to report on the standardization of form of the standard specifications of the Society was presented by Mr. C. W. Burrows, who introduced the following motion:

> "That the Executive Committee be requested to appoint a Committee whose duty it shall be to formulate rules governing the form but not the substance of specifications."

The Secretary announced that the Executive Committee had decided to publish an index of Volumes I to X, inclusive, of the Proceedings.

The Chairman then declared the meeting adjourned *sine die.*

AMERICAN SOCIETY FOR TESTING MATERIALS.

AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROCEEDINGS.

This Society is not responsible, as a body, for the statements and opinions advanced in its publications.

THE AMERICAN SOCIETY FOR TESTING MATERIALS.

ANNUAL ADDRESS BY THE PRESIDENT, HENRY M. HOWE.

Our guild has lost an unusual figure by the death of Gustavus C. Henning. However much those to whom you have entrusted your administrative work may have differed with him in matters of policy and expediency, let us forget our differences at the coming of the awful messenger. Remembering that his untiring zeal contributed greatly to the movement which led to the formation of this Society, let us honor ourselves in honoring him by rising out of respect to his memory.

Were the story of Abou Ben Adhem to be written to-day it would probably tell us that the name which led all the rest was rather of one who served than of one who loved his fellow men. For to-day is the day of the gospel of service. Service is the fruit of love, and that love which bears no service is a barren tree. We may even ask whether such love deserves its name.

The justification for the existence of any society, indeed the justification for the existence of any being, is the service it renders to mankind. It is well for each society and for each individual to ask himself this question from time to time, "By what service to mankind am I justifying my existence, am I repaying the debt which was laid at birth, am I discharging the bond which was forged when I was given the supreme privilege of living on this beautiful earth and in this wonderful age?"

What then is our great service, and how may we increase its value to our fellows? Ours is a work, if I mistake not, akin in a way to lubrication and in another way to piloting. We reduce the friction of our fellow's work and thereby we make it more efficient, more fruitful; we guide it, and thereby again make it more fruitful. By improving methods of testing we teach the manufacturer how he may most wisely test his product so as to know most readily and most surely that he is truly serving the buyer and through the buyer the great world which the buyer in turn serves; that his product really has the qualities which fit it for the service of the user. By this same step we enable the user to learn most readily and surely that his purchase really has the qualities which will enable him, by its use, to serve the public efficiently.

By setting up standard specifications to serve as standards of reasonableness, of what is just and proper, of what safeguards the buyer with the least hardship to the maker, we do two things.

First, we teach both parties what specific qualities each given product needs in order that it may best fit its use. We help the maker to give surely and the buyer to require firmly those specific qualities which will fit the product for its future service, so that that product may the better serve that fraction of mankind which its final users form.

Second, we help maker and buyer to agree. We find them, human fashion, each glaring fixedly at that side of the gold and silver shield which faces him, one firm in his belief that it is gold, the other swearing that it is silver. Having looked the shield all over, we induce each to take a peep round the corner and see its other side. We do our share of the great work of the twentieth century, the substitution of cooperation for combat. By helping both parties to see both sides of the question, to see the true needs of the public which, in the end, we all serve, we help both parties to approach and at last to attain the attitude of cooperation in striving toward their common true interest, to serve that public the best.

It is a part of true service as of true love that it is never satisfied with itself, that it is always seeking to do more and better. Of the many ways in which our service may be made more fruitful, let me dwell on two, making firm our very shaky foundations, and paving the way for international specifications.

The Present Conditions of the Foundations of Testing.—The shakiness of our foundations is only too evident. In order to determine whether a given steel is fitted for a given use, for instance for rails, we prescribe certain reception tests, the conditions of which are very far removed from the conditions under which that steel is to do its work. The results of these tests are not quantitatively convertible into terms of service usefulness. The irrelevancy is not so great as if we determined the fitness of a prima donna by taking her specific gravity or testing her quickness at figures, but it is regrettably great.

A rail must needs resist severe dynamic stresses, severe friction which tends to wear away its upper surface, and severe peening, that is to say, distortion of the upper surface by the pressure of the wheels; and it must continue to endure these things even after the distortion both by friction and by peening have changed the properties of its upper surface radically.

The fitness of a rail for these conditions may be determined either scientifically or empirically, or by both means, using one to check the teaching of the other. To proceed scientifically we must determine, first, the exact nature of the stresses, their intensity, the mode of their application, and the nature, severity, and effects both of friction and of peening. All that is only the first step. As a second step we must next determine quantitatively the relative power of different steels and different sections to endure each of these several things; and forecasting thus the expectation of life which corresponds to each of the various different compositions, microstructures, and sections considered, we calculate by means of the higher mathematics what rail is the cheapest, the cheapest all things considered, including absolute safety within the proposed life, interest charges for relaying, etc. For such true scientific or philosophical solution of the problem not qualitative but quantitative knowledge is needed. That is to say, without awaiting the slow verdict of actual service test, we ought to have laboratory tests which will tell us trustworthily and quantitatively how any given contemplated material and section will behave in actual service.

To state the case is to confess that we are wholly unable to solve the problem scientifically. Though much has been done in the way of measuring the stresses, we have as yet little quantitative knowledge of the effects of friction and peening. But when we come to the second of the two steps, the quantitative forecasting of behavior in service by means of laboratory tests, matters are much worse. First we have most insufficient light on the relative power of different compositions, microstructures, and sections to endure severally the individual requirements of service; and beyond this we have no means of determining that power except by actual service trial. How little we know scientifically about the properties which our steel must have to endure these individual requirements is shown strikingly by the fact that our most learned, most competent, most broad-minded, and best informed authorities have, at least till lately, disagreed radically on so simple and fundamental a question as this, "Does hardness as such, or high ductility with the softness with which it now seems inseparably united, give the better resistance to abrasion?"

I invite your attention to our lack of means of determining by laboratory test the power of materials to endure the conditions of service.

There are certain properties which we need in service, and certain other properties which we can determine readily in our laboratories. For instance, in service we need resistance to abrasion under heavy pressure. This we cannot determine readily in our laboratories, but we can determine hardness by indentation and many like rapid methods. We attempt to use these rapid laboratory methods for determining fitness for service; but we cannot do this quantitatively because the results of the two are not convertible. That one of two steels resists indentation or scratching better than the other, or that it is harder under any other laboratory test, is or has till lately been no sure sign that it was the better fitted to resist abrasion in the track. The two properties are not convertible. Or, if they are convertible qualitatively, they are not quantitatively; and the scientific solution of such a problem requires that the indications of the laboratory reception test shall be convertible quantitatively into terms of power to endure the conditions of service.

A rail must endure the impact of an enormous load traveling extremely rapidly horizontally, but falling only a very short distance vertically. The convenience of our laboratories does not permit us to reproduce such an impact; but we can make a drop test, and we can determine how much the steel will elongate before rupture in a static tensile test. We say that the steel must elongate —say N per cent.—before rupture; not that it will ever have to elongate $N, N \div 2$ or even $N \div 5$ per cent. in service; indeed it hardly has to elongate at all; not because the test is fit, but because it is convenient to make. From the power to endure this distortion without rupture in a static tensile test we infer the power to endure the impact of the drivers. No doubt this test for elongation, and the drop test, both determine properties which are of the same general family with the power to endure service impact. Considerable power to endure these laboratory tests argues considerable power to endure this individual requirement of service, so that at first sight they seem to be convertible. But their convertibility is not quantitive, and hence not fitted for a scientific solution of the problem, as the least reflection shows. Were they convertible quantitatively, then we should know surely how much elongation would suffice as a guarantee that the rail will not break by impact; and knowing this, we should avoid specifying more, as we ought to, for elongation far in excess of that truly needed is probably harmful. For instance, however much you desire an elongation of 20 per cent., or whatever other number appeals to your judgment, you would probably admit that an elongation of 45 per cent. would, in a carbon steel, be accompanied by transverse weakness and by undue flow. What is true of elongation is true of the drop test, and I believe is true of almost every test that we use.

Here, then, is one matter to which I call your attention earnestly. Before we can solve scientifically such problems as these with which we have to do—and such a solution mankind has a right to demand from our guild in due time—we must devise laboratory tests the results of which shall be convertible into terms of power to endure the several conditions of actual service. This we are trying to do. Dissatisfied with the irrelevancy of our static tensile test, we are trying impact and endurance tests, with plain and notched bars, and under divers conditions. The results are to be awaited with the greatest interest; but we should recognize the great difficulties which lie before us, and scrutinize very searchingly the credentials of any new test. What kind of surprises await us may be inferred from the strange law which may be deduced from late results reached at the National Physical Laboratory,* namely, that, comparing wrought irons and steels differing in carbon content from 0.039 to 0.645 per cent., and in yield point from 32,390 to 71,460 lbs. per sq. in., the energy of impact of which 100,000 applications on notched bending bars is needed to cause rupture, is almost independent both of the carbon content and of the yield point.

If the fear of the Lord is the beginning of wisdom, a recognition of the profundity of our ignorance is the beginning of learning. If the remedy for the evils of democracy is more democracy, the remedy for the defects of science is more science. Let us make clear how little we know in order that the enormous value of the information which we await may be clear, and that the richness of this harvest may tempt the adventurous and the competent into our fields.

Tests to Determine Service Usefulness.—My chief purpose in referring to these facts, which are far more familiar to most of you than to me, is to call attention to the importance of the empirical method of solving our problem, and more particularly to a difficult but probably valuable way of making our empirical methods more instructive. Rankine refers, apparently with approval, to the empirical determination of the proper planning of chimneys "by the observation of the success and failure of actual chimneys, and especially of those which respectively stood and fell during the violent storms of 1856."† However confident we may be that we shall one day have laboratory tests, the results of which shall be directly convertible into the power to perform each of the several distinct duties which a rail, an eye-bar, an axle, or other object has to discharge, the difficulties in the way are so great, the distance we have traveled is so slight compared with that which lies beyond, that for a very long time we must rely to a great extent on empirical methods, remaining in the dark as to why it is that such or such specific test is needed and suffices.

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*Stanton and Bairstow, "Engineering," Vol. 86, pp. 733–734, Nov. 27, 1908. I do not find that the authors' enunciate this law in terms, but as regards carbon content it appears in the lower right hand diagram of their Figure 11, and as regards yield point it is readily deduced from a comparison of the energy plotted in that diagram with the yield point given in their Table II.

†"The Pressure of Wind on Chimneys." J. B. Francis, Lowell, 1873.

The method to which I wish to call your attention is the analysis of existing data, and the accumulation hereafter of data available for analysis, to show which, among the various qualities which we now or may later determine, are most truly indicative of service usefulness. Take, for instance, elongation and contraction of area; there are those who pin their faith on one, and those who swear by the other, as the only true measure of the ductility useful in service. Again, of the four properties, tensile strength, true elastic limit, proportionality limit, and yield point, we ought to know which is most truly indicative of power to undergo the stress of any given actual service. Discussions of the relative merits of these properties too often overlook the fact that their value depends solely on the trustworthiness of their indications of service utility, which to-day might be shown more conclusively by an analysis of existing data than by theoretical reasoning.

We have, moreover, the various forms of endurance and impact tests; and with moderate trouble and expense on the part of the great industrial concerns we could have enormous accumulations of data as to microstructure, hardness, and density, a property among the easiest to determine and among those most persistently ignored. My wish here is to call the attention of investigators to the promise of investigations of this class. If, for instance, it were definitely known that the yield point was a much less true measure than the elastic limit of the value of a material for a given service, the more intelligent would prepare to determine the elastic limit. If it were known that for a given service the indications of some form of impact test were more trustworthy, more relevant, than those of the static tensile test, such more relevant test could be adopted. At present we use tests which we know to be indeed irrelevant, but we hesitate to adopt others because we have no good ground for thinking these others more relevant, more indicative of fitness. A conclusive proof that one test was better than another as evidence of fitness for a given service would often be of incalculable value. Beyond this, similar investigations may show which of the indications of a given test are the true signs of fitness, as Mr. Job's[*] analysis of the microstructure of rails showed that the finer-celled rail had the longer life.

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[*] "The Metallographist," Vol. 5, 1902, p. 177.

From this analysis of existing data too many shrink. Indeed the superficial often value it below the creation of new data, forgetting that many of the most brilliant accomplishments of science have followed this path. The discovery of the periodic law called, not for new data, but for the interpretation of existing ones. The discovery of Neptune called in large part for the analysis of existing data; and Leverrier's glory would not have been a whit the less if he had never seen the planet with the sensuous eye, simply indicating to an assistant where it lay. It was not Columbus' simple feat of sailing on that we honor, for he was no more determined a voyager than many another seaman; but his conception, his conviction so deep that it swept away all obstacles, that the easiest way to the Indies lay westward. I dwell on this because a defect of our present conditions is that the proportion of those who mine out new data to those who smelt and refine them is far too large.

The Importance of International Specifications.—Turning to the subject of international specifications, let me first say that the arduous and devoted labors of the public-spirited men who have had this matter in charge have accomplished invaluable preliminary work. In reading their report we may not easily understand the amount of work, of discouraging work in face of tremendous obstacles, of exacting and most difficult work, that these self-sacrificing men have done. We, their debtors, cannot thank them too heartily. Whatever may be the tangible result of their efforts, the example of their untiring and unselfish zeal cannot be lost on us.

In retiring from this high office nine years ago I urged on you the great importance of this subject, saying,* "But we are firmly convinced that the existence of such standard specifications, stamped as standards of reasonableness by a properly constituted body such as we aim to be, will greatly facilitate agreement between buyer and seller, will help them to come to terms, to convince each other that this or that item specified by one of them and objected to by the other is or is not reasonable. Reasonableness should be the spirit in which every negotiation is carried on; standards of reasonableness such as we wish to erect should greatly aid reasonable men in deciding what is reasonable. While we believe this true of domestic negotiations, we believe that it applies with even greater force to international negotiations between buyer and seller who have never seen each other, who live under different conditions of society, with different traditions, conceptions and points of view."

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* *Proceedings*, Vol. II, 1902, p. 18.

Were I to change those words to-day, it would be in the way of making them stronger. Nine years have only strengthened the convictions which I then expressed. But they have impressed me with the difficulties, the very serious difficulties, in the way. Because the first step towards overcoming these is to understand them, let me devote the rest of this address to an attempt to make them clear.

Impediments to the Establishment of International Specifications.—They may be divided into the difficulties of inertia, those of friction, and those of principle. This division makes no pretense to being logical; it simply aims to help bring these difficulties clearly before you. The difficulties of friction and of inertia are like those of wave and fog which the steamer must needs master; the difficulties of principle are like the hurricane which the wise navigator avoids.

By the difficulties of inertia I refer to the general unwillingness of men as a whole to adopt any new thing. Some say "We are getting along very well now." Others have hard enough work to accommodate their ways to the changes which a seething century forces on them; the mediocre have hard enough work to keep their heads above water; the shiftless and self-indulgent to keep from sinking intolerably fast; the few to whom intelligence, self-denial, and industry have been given, to rise as much as their virtues seem to warrant. Every change as a change is unwelcome, because it sets before each of us yet another thing to which we must learn to adjust our ways. Each fears that some unforeseen result of the change may work him harm. Each shrinks, and not unnaturally, from it.

The maker of unusually pure material, whose established business and reputation are an asset of great value, fears that the erection of standards may lessen the value of these assets by tending to steer the buyer into relying more on his own knowledge in measuring the extra value of the better quality. But if he really believes in himself and his goods, he should welcome such standards as a measure by which their superiority may be the more readily verified. He is not really the one to dread the letting in of light, and the education of the public. The difficulty is essentially one of inertia; an unjustified dread of effects which he ought not to expect from the proposed change. His mental processes do not show him the benefits which will flow to him.

The difficulties of friction consist, in part, of the real harm which will be done to individual concerns, manufacturers, importers, brokers, testing engineers—yes, gentlemen, we too are the descendants of the old Adam—and others, by the erection of standards of reasonableness, by the process of re-adjustment to changed conditions, and by the letting in of light. The direct interests of these individual persons and concerns often seem opposed to the general welfare of mankind. The owner of the stage-coach opposes progress when it calls for the introduction of the railroad; the conveyancer when it calls for the creation of the title insurance company; the usurer when it calls for the creation of banks of discount; and the owner of the little retail store when it calls for the creation of the department store. The railroad, the title insurance company, the bank of discount, and the department store all serve the community, but each is a hardship to the few whose usefulness it stops by offering the community a greater usefulness, by lessening the difficulties and the cost of travel, of conveyancing, of borrowing, and of shopping.

There are those who, because of the misfortunes either of their character or of their environment, aim primarily to thrive, not through the fitness of their goods, not through their true service, but through cheapness reached through a sacrifice of quality which is not to the buyers' true interest. There are those so placed that even with the best intentions they cannot afford to give the quality which their customers' best interests need. Could our analysis pierce deep enough I cannot doubt that it would show that the true interest even of these is bound up with the true interests of the customer and thereby of the community; but the proof of this to most minds would be so difficult that, for our present purpose, we may grant that the setting up of standards of reasonableness is against the interest of these. He who would fain profit by the buyer's ignorance and prejudice, by the salesman's specious argument and half truth, is opposed to setting up standards of reasonableness and to letting in light in general.

These are against us. Who, then, are for us?

The Supporters of International Specifications.—First, the small number of manufacturers whose character and environment permit them to serve the consumers' best interests, by supplying the fittest goods; second, the far greater number, the consumers themselves; and third, the incalculably greater number, mankind as a whole, the public which we all serve directly or indirectly, the public whose true interest is that each of its servants shall be supplied with the fittest goods.

We should not be discouraged by the impression which a superficial glance gives us, that those whose immediate interests are against us are keenly touched and hence unite in active aggressive resistance, whereas the enormous mass of our beneficiaries, each of whom is affected but slightly, is apathetic. We should not say, "What profits it that millions bless us if they will not lift their fingers against the hundreds who would overwhelm us?" To say this is to be blind to the character of this century, and to the qualities of the manufacturers who belong on our side, and of the consumers.

The strength of the manufacturers whom, if we are fit for our task, we can bring into alliance, is enormous; the strength of the buyers whose support we should enlist is incalculable. If we but show ourselves worthy leaders the stars in their courses fight on our side.

Look at these manufacturers, and look at the trend of this century away from the old piratical idea that the buyer was the seller's reasonable prey, to the new idea that he is the seller's employer. Look at the growing recognition of the true relation between every employer and his employee, the recognition of the truth that it is not only the employee's moral duty but his true interest to serve his employer's true interests, be that employer his patient, his client, his student, his customer, his superintendent, his master, or the traveler on his trains. If we choose a high enough point of view, and realize that it is but a few centuries since our savage forefathers were cudgeling each other to death on the shores of the North Sea, while before us stretch untold millions of years in which the progress of civilization will surely be ever faster and faster—if we take this point of view, we see that this movement from piracy towards cooperation has in a broad sense only just begun. The power and position into which it has already put such men as Charles B. Dudley, John Fritz, Judge Gary, James Douglas, Charles Kirchhoff, and Edgar S. Cook, should show us the force of this movement even in this, its early stage, and how irresistible it will become. There will be setbacks, as there are in every great movement; but who can doubt that it is part of God's movement, that it is simply one phase of the coming of His Kingdom, and of the irresistible uplifting of mankind?

These are the manufacturers who should be with us. Look at the irresistible weapons with which the century is arming them. Enormous capital seeks their hands as the hands of the fittest. Because of the magnitude of their interests, scientific knowledge and scientific management come to them as surely as the ball comes to the good tennis player's racket. Who can suppose that, if properly appealed to, they would remain apathetic? Look at the consumers. Like conditions are massing enormous aggregations of capital into the hands of the fittest, the magnitude of whose interests justifies the greatest care, the closest scrutiny, and the employment of the best talent in selecting and testing their purchases. Let no man think that our great industrial and transportation corporations, whose true interests are with us, need remain either helpless or apathetic, if we have the skill to show them that we are fighting their fight. The growth of scientific knowledge and the spread of scientific training have begun putting into their hands trained specialists well fitted to test and to interpret the representations of the manufacturer, to see and to protect the consumer's true interests in a way inconceivable a century ago. Universal education has, in and by itself, further vastly increased the available supply of the most efficient servants, by throwing open every path of usefulness to every human being who brings to this earth intelligence and character, be he born in the hovel or in the manor. The creation of these great and intelligent consumers, to whom we can reasonably look for active and strong support, and the strengthening of their position, have only begun.

Look at our third great ally, the public. This same universal education is fast lifting up the great mass of mankind from the old position of the servants of the well-born and strong, the recipients of conditions imposed by their masters and of laws doled out by their lawmakers, to the position of the watchful masters of the lawmakers. This movement has spread beyond our Aryan race till to-day no important unlimited monarchy remains anywhere in the world. Think how lately the first important modern republic was founded.

See how, even in these relatively early days of the passage of the public from the position of recipients toward that of masters, they have enforced their demands for purity of food and of drugs; note their attitude when they thought that their demands for conservation were not duly regarded; note how they have enforced, through most of the world, their demand that the welfare of the masses shall be cared for, whether by the protection of the worker from needless bodily dangers, or by the protection of the young and helpless of either sex from needless perils to character, or by placing the risks of hazardous occupations on those who organize them and profit by them. This is the public, already enlightened and watchful, and fast increasing in intelligence and watchfulness, to which our appeal lies for help and support in our work of helping it to get the fittest goods, by erecting reasonable standards for judging the fitness of those goods.

As such manufacturers, such consumers, and such a public will be benefited by the erection of international specifications, and as they are therefore our natural allies, we should be weak-hearted indeed if we were dismayed by the friction and inertia which oppose us. Nevertheless the friction and inertia are there and must be reckoned with.

International Competition.—But beyond them lie more serious difficulties in certain directions, the present apparent conflict between the interests of certain important countries as regards certain products. Again let me take the case of rails. The ore conditions of Country A force it to use the acid Bessemer process for its rails, and to leave 0.08 per cent. of phosphorus in them; the ore conditions of Country B permit it to use the basic Bessemer process and to reduce the phosphorus content of its rails to 0.04 per cent. I use imaginary numbers to fix our ideas. How shall these two agree? How shall Country A be brought to admit that its apparent disadvantage is a true disadvantage, or Country B that its apparent advantage is illusory? I will not say that it cannot be done; but such conditions make the task of agreeing on international specifications far harder.

In view of this it would seem that the path of least resistance is to choose for the earliest international specifications, not objects the composition of which is determined by the ore conditions of individual countries, as that of rails is; but rather, objects which can readily be given one and the same composition in all the great producing countries.

For instance, among steel objects, for the first trials objects should be selected which are currently made by one of the basic processes, and therefore can have their phosphorus content lowered to any desired point in any country.

Let us look now at some of the steps proposed or taken towards the end of setting up international specifications.

Preparation of International Specifications.—In order that a set of specifications shall be international, they must be accepted by the great nations concerned, and hence must be acceptable to those nations. A first step towards the construction of specifications which should thus be acceptable is to learn precisely wherein and how the dominant specifications of the several nations differ, in order that methods of bridging those differences may be studied. To this end specifications representative of the practice of the chief industrial nations have been analyzed, and their elements have been set up in parallel columns, in order that their differences might be the more readily seen. This laborious work has been done with great care, and it is believed with great skill.

This done, our choice seems to lie between what may be called the direct and the stamping or indirect method of arriving at international specifications.

The direct method is to select as an entering wedge some object the existing specifications for which differ but little among the different producing nations, and try to draw up a single set of specifications which shall be acceptable to all these nations. Where the initial difference is the least, there should the work of bridging it be the least.

The indirect or stamping plan, as I understand it, is first to select, as before, some important object on which agreement should be most readily reached. Second, to create or select from among the several specifications for that object in use in each of the great producing countries, a single national specification which is fairly representative of the practice of that country, and therefore is acceptable to it; and to stamp on it an endorsement in the name of the International Association for Testing Materials, certifying that it is representative of the export practice of that country. This limitation to export practice is made because the chief purpose of international specifications is to facilitate export trade. Third, after these several national specifications shall have been in use long enough to become effective standards, to derive from them a single international specification. I call this the indirect method, because instead of proceeding straight to setting up international specifications, it first sets up single national ones, with the purpose of using them later as scaffolding for international ones. Not only is this plan attractive in itself, but it derives weight from being that selected by those very expert in the matter, one of the committees in charge. But, however great the weight of their authority, the matter is so vital that we may well look at it also from the point of view of those who criticize it. Their criticism bears on the step of stamping the endorsement of the International Association for Testing Materials on single national specifications. This step is scaffolding for the purpose of facilitating the later work of agreeing on international specifications; and it is to facilitate this agreement by simplifying the conditions under which it will later be attempted. The ulterior purpose of the step, then, is to facilitate; the immediate purpose is to simplify, because simplicity will facilitate. Let us look at it then under these two aspects, first, simplification; second, facilitation.

To select from among the many co-existing specifications of each country a single one, to stamp it with approval, to strengthen it thus in order that it may displace the other competing specifications of that country, and thereby to lessen the number of specifications which will later have to be reckoned with when the time finally comes for agreeing on a single international specification which is to replace the several national ones, is to simplify the environment in which this final agreement will be attempted. It simplifies the situation by eliminating most of the competing specifications, and bringing to a minimum the number of those which are later to be brought into harmony. I do not see how this inference can be disputed. So much for simplification. All grant that it would result.

Objections to the "Stamping" Plan.—But when we come to the ulterior purpose of the step, facilitation, the matter is far less clear. Many fear that the step will not facilitate but oppose final agreement; that it is based on a confusion of ideas, pointing out that the really difficult task is to harmonize not specifications but men; that you cannot strengthen one set of Nation A's specifications in such a manner that it expels and excludes the others of Nation A without at the same time strengthening it so that it will the more effectively resist the later attempt to displace it in favor of an international specification; that to induce each of the several little scattered bands of Country A to throw down its own little standard and unite with the others about a single national standard, is to weld them into a compact whole which will fight the more stubbornly and effectively for that standard, not only against the standards of Countries B, C and D, but against any standard, national or international, less favorable to their interests.

To this objection it may be replied that education, the broadening of the outlook of those to whom our appeal lies, is the thing most needed for the introduction of international specifications, and that the example of the good done by the adoption of a single standard export specification for each country would furnish a most powerful argument for carrying this same principle one step farther and uniting on a single international specification. To which in return the objectors may pertinently ask whether the benefits which the United States, Germany, and Italy have found to flow from the union of those whose interests are closely akin into a single vast nation, have actually proved a means of educating them toward taking the next and broader step of welding all these several nations into one international confederation under one constitution. Personally I believe that they have; but we can well understand that others should hold the opposite belief.

A second objection, a much more serious one, and one of a wholly different nature is urged against one of the necessary parts of the stamping plan, the endorsement by the International Association for Testing Materials of each of the several national specifications. It is urged that an endorsement, no matter how carefully it is worded and qualified, is after all an endorsement, a ready means of misleading importers in the countries to which we export, especially because their remoteness hinders their recognizing the intended limitations of the endorsement. In order to understand the position, we must have clearly in mind the distinction between the several national specifications first to be erected as scaffolding, and the single international specification later to be erected on that scaffolding. The criticisms to which I call your attention touch not on the ulterior work of erecting international specifications on the scaffolding of single national ones, but the erecting and endorsing of the single national ones.

In order to gain the assent of a given nation to such an endorsement of a single national specification as truly representing its own practice and product as distinguished from those of other countries, that endorsement must needs be identical in wording with the endorsements stamped on the specifications selected as representing the other nations, because any variation in the wording of the endorsement would be stoutly resisted as discriminating between these several countries in favor of one or against another. In short, identity of endorsement seems a necessary part of this stamping plan.

Many believe that any identical endorsement which can be framed must necessarily be open to this objection, that its identity must appear to endorse the products of the several countries as of equal merit. That is the essence of the objection. It is feared that the result of this identical endorsement of the laxer and of the more stringent specifications, of those which safeguard the buyer the most weakly alongside of those which safeguard him the most strongly, will be to give to the nation of the laxer specification an improper advantage, and to throw an additional and improper obstacle in the path of those whom we fain would aid, those who safeguard the buyer the most thoroughly.

The principle is that which underlies Gresham's law, that the attempt to keep in circulation two currencies of different intrinsic values results in the expulsion of the dearer. The history of finance has proved the truth of this law to those to whom it is not self-evident.

This objection to the plan of identical endorsement furnishes the most serious argument against the stamping plan. In my opinion it must be fully and fairly met before success can be expected. I do not say that it cannot be met. But it is not enough that you and I should be convinced that the wording of a given endorsement could not be used to give undue support to the laxer specification. The parties in interest in other countries must be convinced. Thus far they have not been. If wording can be devised which would, in the opinion of these parties, be free from this objection, it would remove this, the really serious obstacle to the adoption of the stamping plan. The best way to show that such an endorsement can be devised is to devise it; and as one who has the movement very close to his heart I urge on those who advocate the stamping plan to proceed immediately to remove this objection to it by devising such an endorsement.

These are real difficulties, needing our most serious thought. Their importance stands forth and does not need the endorsement of the high personal character of those who offer them, while hoping earnestly that their fears may be shown by appeals to reason to be groundless, and that the difficulties themselves may be overcome.

It is assumed that those who have the matter in charge, before formulating any definite steps in such difficult conditions, requiring as they do the warm cooperation of many classes in many lands, will ascertain that those steps seem wise to those on whose cooperation they must rely.

Whatever course is chosen, the task is going to be a very difficult one, calling for great patience, tact, breadth of view, and constructive statesmanship; for endless missionary work and endless labor in making our natural allies see that we are serving their true interests, and that we are fit for the task we have undertaken. Thus may we get their active cooperation. The problem is not an engineering but a psychological one.

The leaders in this movement must be willing to devote themselves to the patient labor of convincing the convincible, and to follow not the path which their own preferences may suggest, but that which offers the most efficient means of persuading those whom they have to persuade. When they fail to convince their natural allies, it is well to assume that the fault is either in their proposals or in their exposition of them. To question the good faith or intelligence of those allies is to close the only possible path, persuasion and diplomacy. The opportunity offers to serve the interests of mankind in a very broad way. The task is to be taken up reverently, with the consciousness that it must tax to the utmost the powers even of the best equipped.

REPORT OF COMMITTEE A-1 ON STANDARD SPECIFICATIONS FOR STEEL.

Committee A-1 has held two meetings during the past year, at which four proposed standard specifications prepared by sub-committees were thoroughly discussed, amended and referred to letter ballot of the general Committee. The proposed standard specifications referred to are as follows:

(*a*) Proposed Standard Specifications for Forged and Rolled, Forged, or Rolled Solid Steel Wheels for Engine Truck, Tender and Passenger Subway and Elevated Railway Service.

(*b*) Proposed Standard Specifications for Forged and Rolled, Forged, or Rolled Solid Steel Wheels for Freight Car Service.

(*c*) Proposed Standard Specifications for Heat-Treated Carbon-Steel Axles, Shafts, and Similar Parts.

(*d*) Proposed Standard Specifications for Steel Reinforcement Bars.

These specifications are appended to this report, and the vote of the Committee having resulted favorably, they are now recommended for adoption by the Society.

For the better systematization of the work of the Committee, permanent sub-committees have been appointed on the following subjects:

Sub-Committee	I.	Bessemer Steel Rails. Open-Hearth Steel Rails. Steel Girder Rails. Steel Splice Bars.
Sub-Committee	II.	Structural Steel for Bridges. Structural Steel for Buildings. Structural Steel for Ships.
Sub-Committee	III.	Steel Axles. Heat-Treated Steel Axles.

Sub-Committee IV.	Rolled Steel Wheels. Steel Tires.
Sub-Committee V.	Steel Forgings.
Sub-Committee VI.	Steel Castings.
Sub-Committee VII.	Steel Reinforcement Bars.
Sub-Committee VIII.	Open-Hearth Boiler Plate and Rivet Steels.
Sub-Committee IX.	Locomotive Materials in General.
Sub-Committee X.	Steel Wire.
Sub-Committee XI.	Automobile Steels.

For the proper distribution of the work among a sufficient number of well-qualified experts, the Committee has greatly enlarged its membership by authorization of the Executive Committee. The Committee has further recommended to the Executive Committee to consider the advisability of merging with Committee A-1 all other existing committees whose work relates partly or wholly to specifications for steel or steel products. As part of this general plan it is proposed to increase the number of sub-committees to such an extent as may be found necessary, but the work of these sub-committees would then be conducted under the auspices of a single general committee. This arrangement is not intended to affect the independent continuation of existing committees in so far as work not relating to specifications is concerned. No action has as yet been taken on this proposal, which has only recently been transmitted to the Executive Committee.

The report of the American representatives on International Sub-Committee Ia on International Specifications for Steel, appended to this report, was presented at a recent meeting of Committee A-1. It was decided at that meeting that "this report shall be embodied in the annual report of Committee A-1 for the information of the Society."

It is proposed during the coming year to consider the feasibility of reconciling the differences between the present Standard Specifications for Steel and the principal specifications of other societies, and to accomplish this, if possible, through the co-operation of these societies with the proper sub-committees of

Committee A-1. To facilitate this work, and as a matter of interesting information to the membership of the Society at large, the Committee has recommended to the Executive Committee that some of the leading specifications of other organizations be hereafter embodied in the Year-Book of the Society.

Respectfully submitted on behalf of the Committee,

WM. R. WEBSTER,
Chairman.

EDGAR MARBURG,
Secretary.

NOTE.—The Proposed Standard Specifications for Steel Wheels for (*a*) Engine and Passenger Service and (*b*) Freight Car Service were reported back to Committee A-1 for further consideration (see pages 55-62).

The Proposed Standard Specifications for Heat-Treated Carbon-Steel Axles, etc., and for Steel Reinforcement Bars, were referred to letter ballot of the Society. This ballot, canvassed on August 21, 1911, resulted favorably (see pages 63-68).—ED.

[For Discussion of this report, see pages 69–81.]

PROGRESS REPORT BY THE AMERICAN MEMBERS OF INTERNATIONAL SUB-COMMITTEE Ia ON THE INTRODUCTION OF INTERNATIONAL SPECIFICATIONS FOR STEEL.

During the past year summaries of the specifications for Ship Material of the German, English and American National Societies have been prepared and tabulated in parallel columns, which will soon be published in French, German and English. This is considered necessary so that they may be intelligently compared. Particular attention is called to this, since this procedure has been criticised as useless work and called parallelism, tending to fix the specifications rather than serving as a step toward unification. Exactly the same course was followed, however, in the early work of Committee A-1, by tabulating all the requirements of the various American specifications on a single sheet for each class of material. This greatly facilitated the work of that committee, and practical results were reached much sooner than would have been possible by any other method.

A description, with cuts, of the standard drop-testing machine for rails, adopted by The American Railway Engineering Association and this Society, will soon be published by The International Association in French, German and English, and widely distributed. This will tend towards the introduction of similar standard machines in other countries, or it will at least serve to call attention to the many varieties of drop-testing machines in these countries. The introduction of a standard drop-testing machine has been of great benefit to the manufacturers and consumers of this country, and the adoption of similar standard machines would no doubt be of like benefit to other countries. The seventh London Resolution was as follows:

> "That a standard drop-testing machine for rails, be adopted in each country, as has already been done in the United States, in order to make the tests comparative."

Under the London Resolutions, which were approved by the Copenhagen Congress, we are instructed that whenever our specifications were under consideration, we should refer to the English and German specifications for similar materials, and endeavor, if possible, to bring ours more nearly into line. In many respects our specifications are more complete, and it seems premature to make such a request at this time. For instance, neither of the other specifications contain chemical requirements for ship material, and the German specifications contain no chemical requirements for bridge material. Attention has been called to the desirability of chemical requirements in specifications, and it has been suggested that this be made a subject for discussion by Sub-Committee Ia, and at the next Congress.

The work referred to above is mostly preliminary to the introduction of international specifications. The instructions to the Sub-Committee are as follows:

> "Committee Ia shall, on the basis of specifications recognized by the National Testing Associations of Germany, England and the United States of North America, endeavor to establish uniform international specifications for the delivery of material to countries that do not produce iron in any considerable quantities."

that is, the international specifications are to be for the specific use of the countries that do not manufacture steel. It was with the view of familiarizing the consumers of such countries with the materials used by the railroads of Germany, England and America, that the London Resolutions were passed in July, 1909, recommending the specifications of the National Testing Societies of these countries for use on export orders. This was done in preference to recommending the specifications of any individual engineer or railway company as the previously mentioned specifications were in more general use and had been prepared by committees on which both consumers and manufacturers were represented.

It was not claimed that any one of these specifications was the equivalent of the others, but it was thought that if material was manufactured in any of the countries under its own specification it would probably be better than if it was manufactured under the specifications of the other countries, which would contain requirements with which the mills were not familiar. If this course is followed and the users of materials become familiar with the requirements and materials of each of the three countries, they will soon learn which is best suited to their needs, purchase in that country or ask for modifications from the other countries so as to get similar material. This would be of great assistance especially if the materials received from the three countries were equally satisfactory, Moreover, it was suggested that trial orders be manufactured under the specifications of the other countries so as to familiarize the manufacturers with the requirements of all three countries.

The use of a national specification for each country for export orders has been criticised as tending to "fix" such specifications, and thus to prevent international specifications. The recommendation of the first London Resolution covering this point has been criticised as being too strong, and a modification has been suggested by which these national specifications would be certified by the International Association as standard specifications of the National Societies for these countries. In other quarters objection has been made to any form of endorsement or certification whatever of these national specifications. The Sub-Committee has, however, received as yet no official instructions on these matters from its Chairman, nor has it been advised as to what modifications, if any, should be made in the proposed method of conducting this work.

Until a national specification has been definitely fixed and has come into general use, it would have little or no weight in arriving at the ultimate requirements for an international specification, when compared with national specifications of other countries which are definitely fixed and in general use. It is therefore of the utmost importance that between now and the International Congress in 1912, everything possible should be done to harmonize our specifications with the leading American specifications, in order that they may carry the proper weight at the Congress.

We can only expect that important requirements will be harmonized in an international specification, as minor requirements, especially those applying to methods of manufacture, will of necessity differ in different countries. It is not too much to expect that in some cases the national specifications will be all that is required since the engineers in each country have introduced special requirements into them to meet their needs, taking into account their own conditions of manufacture. Equally good ships, bridges, other structures, and materials have been manufactured in each of the three countries, and there is no reason why materials from any country, manufactured under their national specifications, should not give good results when used by countries that do not manufacture steel.

The summaries of the specifications, while well adapted to the work of the Sub-Committee, are of very little value to those desiring to use the national specifications. It would be much better, therefore, if all the specifications under consideration by the Sub-Committee were published in complete form in French, German, and English, in time for the Congress in 1912.

The report of the Sub-Committee, including the London Resolutions referred to above, was printed in three languages and, all the members having received copies, was presented for discussion at the last Congress. Professor A. Martens, First Vice-President of the International Association, offered the following resolution before Section A of the Congress:

"The Fifth International Congress welcomes the work of Sub-Committee Ia with pleasure and approves in general the principles laid down in the Congress Report VIII, and decides: Commission I is invited to continue their valuable labors in cooperation with the National Societies, and, if possible, to lay before the Sixth Congress definite proposals as to the basis of International Specifications for Iron and Steel."

As before reported, The International Railway Congress at Berne last year passed the following resolutions, showing the great interest that is taken in the work of Sub-Committee Ia:

1. The Congress express the wish to have reported at the next Congress, with the collaboration of steel manufacturers, on the state and progress of the work of The International Association for Testing Materials dealing with the comparison of the specifications for iron and steel.

2. The Congress further resolved in connection with the conclusions on the question of steel and special steel, that it is desirable to take into consideration the specifications drawn up by The International Association for Testing Materials for the inspection of steel materials."

This puts before members of Committee A-1 everything of importance that has taken place since the Congress. While Sub-Committee Ia is a sub-committee of an International Committee, that Committee has been instructed to consider the specifications prepared by Committee A-1. This report is therefore presented to Committee A-1 at this time for discussion, in order to obtain the views of that committee for our future guidance.

Respectfully submitted,

W. A. BOSTWICK,
WM. R. WEBSTER,
American Members of International Sub-Committee Ia.

SUPPLEMENTAL REPORT BY THE AMERICAN MEMBERS OF INTERNATIONAL SUB-COMMITTEE Ia ON INTERNATIONAL STEEL SPECIFICATIONS AND ON THE WORK OF THAT SUB-COMMITTEE.*

The report of International Sub-Committee Ia was made a prominent feature of the proceedings at the Copenhagen Congress held in 1909, inasmuch as this report recommended what was virtually a new departure of great significance and far-reaching importance in the policy of the International Association. The recommendations in this report were formally approved in principle at the Congress and the Sub-Committee was requested to continue its "valuable labors in co-operation with the National Societies, and if possible to lay before the Sixth Congress definite proposals as to the basis of International Specifications for Iron and Steel." A full account of this subject is contained in the report of Committee A-1 on Standard Specifications for Steel, presented at the Thirteenth Annual Meeting of the American Society for Testing Materials.†

> "Committee Ia shall, on the basis of specifications recognized by the National Testing Associations of Germany, England, and the United States of North America, endeavor to establish uniform international specifications for the delivery of materials to countries that do not produce iron in any considerable quantities."

It was decided to consider for each class of material only one standard specification from each country, prepared by representatives of the manufacturing interests and an equal number of representatives from the railroad companies or of consumers' interests. The specifications decided on were as follows:

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* This report was received from its authors on October 16, and is printed at their request in order to give the members of the Society information as to the developments in this field since the last annual meeting, and the recent recommendations of the American members of the International Sub-Committee Ia.—Ed.

† *Proceedings*, Vol. X, 1910, pp. 54–60.

German specifications recognized by the Deutscher Verband für die Materialprüfungen der Technik.
English specifications published by the Engineering Standards Committee, London.
American specifications adopted by the American Society for Testing Materials.

Since the Copenhagen Congress the plan proposed by Sub-Committee Ia has received much attention in interested circles, both in this country and abroad. Nevertheless, it appears that the real meaning and purpose of the plan is not yet fully understood, as evidenced, for example, by recent inquiries on the part of two prominent members of the Association, who were present at the Congress and participated in the discussion of the subject at that time. One of these, a prominent steel manufacturer of Belgium, asked what modifications would be required in the specifications of that country for home orders, in order that they might agree with the proposed international specifications. He was very much relieved to learn that there was no thought of changing the requirements of the specifications of any country for home orders, the idea being merely to simplify the specification for export orders, the individual countries being left entirely free in their own internal matters. The other asked, "Why does the International Association for Testing Materials want international specifications?" The reply was, "To increase international commerce on lines fair to all parties concerned, and to enable engineers to purchase materials on specifications that are consistent with the best practice to-day." He replied, "This opens up very important commercial questions that should be referred to the steel manufacturers." We thoroughly agree with this and steps have been taken to obtain the views of the English, German, Belgium, French and American steel manufacturers for the information of the Sub-Committee and the Association. It seems desirable, therefore, that a brief report of progress should be made to our members, both with a view of giving them a general idea of present conditions, and of enlisting their active cooperation. The time has passed when any specification or set of specifications with clauses calling for low phosphorus requirements based on favorable ore conditions, would tend to confine orders to any one locality or country, inasmuch as the basic and duplex processes have equalized matters in this respect so that there is now no commercial advantage to be gained by insisting on specifications of this kind. Such matters can now be settled by the Committee on their intrinsic merits, without reference to their commercial bearing.

The summaries of the specifications printed in the Proceedings of the Copenhagen Congress in French, German and English, have been very helpful to the Sub-Committee in the conduct of its work, but these summaries are of little or no value to an engineer who may desire to purchase rails or other materials, for he must have the specifications printed in full in his own language before he can decide intelligently upon their adoption. We therefore recommend that all of these tabulated specifications be printed in full in French, German, English and Spanish for the information of our members, and of the members of the International Railway Congress Association in view of the following resolutions which were passed at the International Railway Congress at Berne last year, showing the great interest that is taken in the work of Sub-Committee Ia:

1. The Congress express the wish to have reported at the next Congress, with the collaboration of steel manufacturers, on the state and progress of the work of the International Association for Testing Materials dealing with the comparison of the specifications for iron and steel.
2. The Congress further resolve, in connection with the conclusions on the question of steel and special steel, that it is desirable to take into consideration the specifications drawn up by the International Association for Testing Materials for the inspection of steel materials.

The Association in each country should attend to and pay for the translation, printing and distribution of its own specifications.

It is believed that real progress will be best insured by concentrating efforts on the following specifications which have already been summarized in parallel columns, and that it would be unwise to attempt to cover a wider field at the present time. The specifications referred to include the following products:

Structural Steel for Ships.—We would propose that the British Engineering Standards Committee's specifications for Structural Steel for Ship Building be adopted as international specifications. These specifications are the same as Lloyds specifications which are widely used.* We believe, however, that chemical requirements should be added and we would propose the adoption of the chemical requirements of the specifications of the American Society for Testing Materials for Structural Steel for Ships. It is not considered necessary at this date to enter upon an extended argument on the desirability of chemical requirements for an important material like ship steel, and it is to be expected that the above recommendations will be complied with.

Structural Steel for Bridges.—We also recommend that the specifications of the American Society for Testing Materials for Structural Steel for Bridges shall be adopted as international specifications. These specifications are practically the same as those of the American Railway Engineering Association, an organization of about one thousand members, affiliated with and operating under instructions, with regard to rail tests, rail specifications, etc., from the Committee on Maintenance of the American Railway Association representing over 260,000 miles of road. The latter is, in turn, affiliated with the International Railway Congress Association whose membership embraces all the leading railways of the world. This is an important consideration, since the railways are the chief users of specifications for bridge materials.

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* The Engineering Standards Committee in Report No. 13, revised in September, 1910, prescribes the following requirements for structural steel for ship building, which are exactly the same as those of Lloyds Registry, as given in July, 1909:

Plates: ultimate strength, 28 to 32 tons = 62,720 to 71,680 lbs. per sq. in.

Plates, for cold flanging: ultimate strength, 26 to 30 tons = 58,240 to 67,200 lbs. per sq. in.

Plates, $\frac{3}{8}$ in. thick and over: elongation, 20 per cent. in 8 ins.

Plates, under $\frac{3}{8}$ in.: elongation, 16 per cent. in 8 ins.

Shapes: ultimate strength, 28 to 33 tons = 62,720 to 73,920 lbs. per sq. in.; elongation, same as for plates.

Structural Steel for Buildings.—We further recommend for adoption, the specifications of the American Society for Testing Materials for Structural Steel for Buildings. These are in many respects the same as the specifications for Structural Steel for Bridges, but permit the use of Bessemer steel and a higher phosphorus limit.

Steel Rails and Splice Bars.—In the matter of rail and splice bar specifications, it would be premature to recommend any international specifications and it is doubtful if a single specification can be recommended owing to the different methods of manufacture and the different requirements of the railroads. It is therefore recommended that a discussion of rail specifications be made a matter of business at the next Congress and that a full session be devoted to this subject.

It is further recommended that until one or more international specifications for rails and splice bars are adopted, all those interested in the subject of rails should request the manufacturers of the different countries to work to the specifications of the National Societies of England, Germany and America indiscriminately, according to the individual preference of the engineer, as they are called upon by the purchasers of rails. This will give valuable information to the committees on the requirements of these different specifications, and is in accordance with the course outlined in one of the recommendations in the report of Sub-Committee Ia at the Copenhagen Congress. This recommendation is as follows:

> "The Sub-Committee recommends the Commission to confer with the three Associations referred to above and keep them advised of the progress made, or suggestions offered, on modifications to bring the specifications more into line; that they also confer with the manufacturers direct, or through the Association, to induce them to roll trial orders of material under the conditions of the specifications of other countries, in order to find out how far these conditions can be introduced for export orders."

We further recommend that a two days meeting of the Sub-Committee be held in England in May, 1912, immediately following the meeting of the Iron and Steel Institute, and that one session be conducted as a joint meeting with the members of the Standards Committee and other engineers or manufacturers, interested in the subject, who may be invited to attend.

To this report is added a list of subjects which we think should be discussed in connection with rail specifications. We would ask that additions to this list be made by the other members of the Committee.

Should this general plan meet with the approval of the Chairman and members of Sub-Committee Ia, we would ask that a bulletin be issued this fall giving information to all of our members as to the course that the Sub-Committee proposes to follow, with a view of creating more general interest in the subject, and securing cooperation in the widest possible circles.

As closely related to the foregoing we desire to direct attention to the great field of usefulness open to the International Association if it would concern itself not only with the establishment of international specifications for materials, but also with international standards affecting the form and size of important articles of manufacture. It is interesting to recall in that connection that the British Engineering Standards Committee was originally created to bring about the latter form of standardization rather than the standardization of materials. As the work of that committee progressed it was recognized, however, that the two questions were, to a large degree, inseparable, and the efforts of the Engineering Standards Committee, as now directed, include the standardization of materials as well as standardization of shape and form. It is to be hoped that the American Society for Testing Materials, which has thus far limited its activity mainly to the standardization of specifications and methods of tests, will also give attention in increasing measure to the standardization of shape; and that the influence of the International Association will make itself felt along these broad and inclusive lines, with a view to the ultimate establishment of International standards.

Respectfully submitted,

W. A. BOSTWICK,
WM. R. WEBSTER,

American Members of International Sub-Committee Ia.

Subjects for Discussion of Rail Specifications.

1. General points of difference in the three specifications under consideration.
2. What general conditions can be embodied in a specification irrespective of methods of manufacture:
    - (*a*) Physical tests;
    - (*b*) Chemical requirements.
3. How should the above requirements be modified for
    - (*a*) Basic Bessemer rails;
    - (*b*) Acid Bessemer rails;
    - (*c*) Basic Open-Hearth rails;
    - (*d*) Acid Open-Hearth rails;
    - (*e*) Duplex Process rails.
4. Should tension tests be required, and if so what limits in tensile strength and what elongation should be specified.
5. Dead load tests for different weights and sections of rails.
6. Height of drop for different weights and sections of rails in connection with carbon content and ultimate strength specified.
7. Discuss with 6, the variations in requirements in present specifications:
    - (*a*) Weight of tup (2,000 lbs. and 2,400 lbs.).
    - (*b*) Distance between supports (3 ft., 3 ft. 6 ins., 4 ft.).
    - (*c*) Length of rails tested (6 ft. and 30 ft.).
8. The standard drop testing machine in general use in the United States of America, adopted by The American Society for Testing Materials, and American Railway Engineering Association. A description of this machine has been published in Vol. II, No. 4, of the Proceedings of the International Association for Testing Materials, issued May 20, 1911. A full description of the machines and details of tests in use in other countries, is asked in order that we may arrive at a thorough understanding of the conditions under which tests are made, in order to decide on a standard requirement, or whether to make allowances, taking into account the varying conditions under which such tests are made.

Could not comparative tests be made between now and the Congress, to decide some of the above points?

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA, PA., U. S. A.
AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROPOSED STANDARD SPECIFICATIONS FOR FORGED AND ROLLED, FORGED, OR ROLLED SOLID STEEL WHEELS FOR ENGINE TRUCK, TENDER AND PASSENGER, SUBWAY AND ELEVATED RAILWAY SERVICE.*

Process. 1. Steel for wheels shall be made by the open-hearth process.

Discard. 2. The ingots from which the blanks are made must have sufficient discard to insure freedom from injurious pipe and segregation.

Chemical Composition. 3. The steel for wheels shall conform to the following limits in chemical composition:

Carbon	0.60 to 0.85 per cent.
Manganese	0.50 " 0.80 " "
Silicon	Not to exceed 0.35 " "
Phosphorus	" " " 0.06 " "
Sulphur	" " " 0.06 " "

Sample for Chemical Analysis. 4. Drillings from small test ingot cast with the heat shall be taken to determine whether the heat is within the limits of chemical composition specified in Paragraph 3. For check analysis the purchaser has the right to take drillings from any two points in the plate on radii at right angles to each other of any one wheel from each heat, but not at any point where the usefulness of the wheel will be impaired; drillings to be clean and free from scale, oil and dirt; and this check analysis may, at the option of the purchaser, be made from mixed drillings taken entirely through the plate at the two points.

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* The specifications were referred back to Committee A-1 for further consideration.—ED.

Analyses Furnished. 5. When required, the purchaser or his representative is to be furnished an analysis of each heat from which rolled wheels are made, such analysis to cover elements specified in Paragraph 3.

Finish. 6. All wheels must be free from injurious seams, cracks, laminations or other imperfections detrimental to strength or service.

Workmanship. 7. Wheels shall be furnished rough bored and with hubs faced. They may be furnished with contours as rolled and without additional machine work, provided they conform to the dimensions specified within the following tolerances:

(*a*) *Height of Flange.*—The height of flange shall not vary more than $\frac{1}{16}$ in. over nor more than $\frac{1}{32}$ in. under that specified.

(*b*) *Thickness of Flange.*—The thickness of flange shall not vary more than $\frac{1}{16}$ in. over or under that specified.

(*c*) *Throat Radius.*—The radius of the throat shall not vary more than $\frac{1}{16}$ in. over or under that specified.

(*d*) *Thickness of Rim.*—The rim may vary in thickness, but the variation less than the specified thickness shall not exceed $\frac{3}{16}$ in. The thickness of rim shall be measured at the center line of tread.

(*e*) *Width of Rim.*—The width of rim shall not vary more than $\frac{1}{8}$ in. over or under that specified.

(*f*) *Thickness of Plate.*—The plate may vary in thickness, but the variation less than the specified thickness shall not exceed $\frac{1}{32}$ in. for each $\frac{1}{8}$ in. in the thickness of the plate.

(*g*) *Limit Groove.*—Where limit groove is called for, the location of the center of limit of wear groove shall not vary more than $\frac{1}{16}$ in. from that specified and its distance from the inner edge of the rim at the thinnest point shall not be less than $\frac{11}{16}$ in.

(*h*) *Diameter of Bore.*—The diameter of rough bore shall not be more than $\frac{1}{16}$ in. greater nor more than $\frac{1}{8}$ in. less than specified. When not specified the rough bore shall be $\frac{1}{4}$ in. less in diameter than the finished bore, subject to the above limitations.

(*i*) *Hub Diameter.*—The hub diameter may vary, but the thickness of the wall of the finished bored hub shall not be less than 1$\frac{1}{8}$ ins. at any point unless otherwise specified, and shall not vary more than $\frac{3}{8}$ in. at any two points on the same wheel.

(*j*) *Hub Length.*—The length of hub shall not vary more than $\frac{1}{8}$ in. over or under that specified.

(*k*-1) *Depression of Hub.*—For passenger truck wheels and wheels of similar design, the depression of hub from front face of rim shall not be less but may be $\frac{1}{8}$ in. more than that specified.

(*k*-2) *Projection of Hub.*—For engine truck wheels and wheels of similar design the projection of hub from back face of rim shall not be less but may be $\frac{1}{8}$ in. more than that specified.

(*k*-3) *Depression and Projection of Hub.*—For subway and elevated railway motor wheels the depression of hub from front face of rim shall not be less but may be $\frac{1}{8}$ in. more than that specified. The projection of hub from back face of rim shall not be more than $\frac{1}{32}$ in. over nor more than $\frac{1}{16}$ in. under that specified.

(*l*) *Black Spots in Hub.*—Black spots in the rough bore shall not be longer than 2 ins. nor deeper than $\frac{1}{8}$ in. Black spots longer than 2 ins. or deeper than $\frac{1}{16}$ in. will not be permitted in rough bore within 2 ins. of either face.

(*m*) *Eccentricity of Bore.*—The eccentricity between the tread at its center line and the rough bore shall not exceed $\frac{1}{16}$ in.

(*n*) *Block Marks on Tread.*—The maximum height of block marks must not be greater than $\frac{1}{64}$ in.

(*o*) *Rotundity.*—All wheels shall be gauged with a ring gauge, and the opening between the ring gauge and tread at any one point shall not exceed $\frac{1}{32}$ in.

(*p*) *Plane.*—Wheels shall be gauged with a ring gauge placed concentric with and perpendicular to the axis of the wheel. All points on the back of the rim equidistant from the center shall be within a variation of $\frac{1}{16}$ in. from the plane of the gauge when so placed.

(*q*) *Tape Sizes.*—Wheels shall not vary more than five tapes under nor nine tapes over the size called for by the drawing.

(*r*) *Mating.*—All wheels shall be measured with a tape based on the standard M. C. B. tape, with tape divisions $\frac{1}{8}$ in. apart. The tape numbers shall be stenciled in plain letters on each wheel. Wheels must be mated to tape sizes and shipped in pairs. A variation of one tape will be allowed in the same pair.

Branding 8. Wheels shall be stamped with the maker's brand and number in such a way that each wheel may be readily identified.

Inspection. 9. The inspector representing the purchaser shall have free entry to the works of the manufacturer at all times while his contract is being executed. All reasonable facilities shall be afforded the inspector by the manufacturer to satisfy him that the wheels are being furnished in accordance with the contract. All tests and inspection shall be made at the place of manufacture prior to shipment and shall be so conducted as not unnecessarily to interfere with the operation of the mill.

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PROPOSED STANDARD SPECIFICATIONS FOR FORGED AND ROLLED, FORGED, OR ROLLED SOLID STEEL WHEELS FOR FREIGHT CAR SERVICE.*

Process. 1. Steel for wheels shall be made by the open-hearth process.

Discard. 2. The ingots from which the blanks are made must have sufficient discard to insure freedom from injurious pipe and segregation.

Chemical Composition. 3. The steel for wheels shall conform to the following limits in chemical composition:

Carbon	0.60 to 0.85	per	cent.
Manganese	0.50 " 0.80	"	"
Silicon	Not to exceed 0.35	"	"
Phosphorus	" " " 0.06	"	"
Sulphur	" " " 0.06	"	"

Sample for Chemical Analysis. 4. Drillings from small test ingot cast with the heat shall be taken to determine whether the heat is within the limits of chemical composition specified in Paragraph 3. For check analysis the purchaser has the right to take drillings from any two points in the plate on radii at right angles to each other of any one wheel from each heat, but not at any point where the usefulness of the wheel will be impaired; drillings to be clean and free from scale, oil and dirt; and this check analysis may, at the option of the purchaser, be made from mixed drillings taken entirely through the plate at the two points.

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* The specifications were referred back to Committee A-1 for further consideration.—ED.

Analyses Furnished. 5. When required, the purchaser or his representative is to be furnished an analysis of each heat from which rolled wheels are made, such analysis to cover elements specified in Paragraph 3.

Finish. 6. All wheels must be free from injurious seams, cracks, laminations or other imperfections detrimental to strength or service.

Workmanship. 7. Wheels may be furnished with contours as rolled and shall conform to dimensions specified within the following tolerances:

(*a*) *Height of Flange.*—The height of flange shall not be more than 3/32 in. over nor more than 1/32 in. under that specified.

(*b*) *Thickness of Flange.*—The thickness of flange shall not vary more than 1/16 in. over or under that specified.

(*c*) *Throat Radius.*—The throat radius shall not vary more than 1/16 in. over or under that specified.

(*d*) *Thickness of Rim.*—The rim may vary in thickness, but the variation less than the specified thickness shall not exceed 3/16 in. The thickness of rim shall be measured at the center line of tread.

(*e*) *Width of Rim.*—The width of rim shall not be more than 1/8 in. less nor more than 1/4 in. over that specified.

(*f*) *Thickness of Plate.*—The plate may vary in thickness, but the variation less than the specified thickness shall not exceed 1/32 in. for each 1/8 in. in the thickness of plate.

(*g*) *Limit Groove.*—Where limit groove is called for, the location of the center of limit of wear groove shall not vary more than 1/16 in. from that specified and its distance from the inner edge of the rim at the thinnest point shall not be less than 11/16 in.

(*h*) *Diameter of Bore.*—The diameter of rough bore shall not be more than 1/16 in. greater nor more than 1/8 in. less than specified. When not specified the rough bore shall be 1/4 in. less in diameter than the finished bore, subject to the above limitations.

(*i*) *Hub Diameter.*—The hub diameter may vary, but the thickness of wall of the finished bored hub shall not be less than $1\frac{1}{8}$ ins. at any point unless otherwise specified, and shall not vary more than $\frac{1}{2}$ in. at any two points on the same wheel.

(*j*) *Hub Length.*—The length of hub shall not vary more than $\frac{1}{8}$ in. over or under that specified.

(*k*) *Depression of Hub.*—The depression of hub from front face of rim shall not be less but may be $\frac{1}{8}$ in. more than that specified.

(*l*) *Black Spots in Hub.*—Black spots in the rough bore shall not be longer than 2 ins. nor deeper than $\frac{1}{8}$ in. Black spots longer than 2 ins. or deeper than $\frac{1}{16}$ in. will not be permitted in rough bore within 2 ins. of either face.

(*m*) *Eccentricity of Bore.*—The eccentricity between the tread at its center line and the rough bore shall not exceed $\frac{1}{16}$ in.

(*n*) *Block Marks on Tread.*—The maximum height of block marks must not be greater than $\frac{1}{64}$ in.

(*o*) *Rotundity.*—All wheels shall be gauged with a ring gauge, and the opening between the ring gauge and tread at any point shall not exceed $\frac{1}{16}$ in.

(*p*) *Plane.*—Wheels shall be gauged with a ring gauge placed concentric with and perpendicular to the axis of the wheel. All points on the back of the rim equidistant from the center shall be within a variation of $\frac{3}{32}$ in. from the plane of the gauge when so placed.

(*q*) *Tape Sizes.*—Wheels shall not vary more than five tapes under nor nine tapes over the size called for by the drawing.

(*r*) *Mating.*—All wheels shall be measured with a tape based on the standard M. C. B. tape, with tape divisions $\frac{1}{8}$ in. apart. The tape numbers shall be stenciled in plain letters on each wheel. Wheels must be mated to tape sizes and shipped in pairs. A variation of one tape will be allowed in the same pair.

8. Wheels shall be stamped with the maker's brand and number in such a way that each wheel may be readily identified. **Branding.**

Inspection. 9. The inspector representing the purchaser shall have free entry to the works of the manufacturer at all times while his contract is being executed. All reasonable facilities shall be afforded the inspector by the manufacturer to satisfy him that the wheels are being furnished in accordance with the contract. All tests and inspection shall be made at the place of manufacture prior to shipment and shall be so conducted as not unnecessarily to interfere with the operation of the mill.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD SPECIFICATIONS FOR HEAT-TREATED CARBON-STEEL AXLES, SHAFTS, AND SIMILAR PARTS.

ADOPTED AUGUST 21, 1911.

Process of Manufacture. 1. Steel under this specification shall be made by the open-hearth or other approved process.

Discard. 2. A sufficient amount of discard must be made from each ingot to insure freedom from piping and undue segregation.

Chemical Composition. 3. The steel shall conform to the following limits in chemical composition:

Carbon	Not over 0.60 per cent.
Manganese	0.40 to 0.80 " "
Phosphorus	Not over 0.05 " "
Sulphur	" " 0.05 " "

Samples for Chemical Analysis. 4. Drillings shall be taken from the crop end of one axle, shaft, or similar part from each melt represented, parallel to the axis on any radius one-half the distance from the center to circumference, to determine whether the chemical composition of the heat is within the limits specified in Paragraph 3.

In addition to the complete analysis, the purchaser has a right to call for a phosphorus determination, to be made from turnings from each tensile test specimen, and the phosphorus must show within the limits called for by Paragraph 3.

Tensile Test. 5. The steel shall conform to the following minimum physical properties:

Ultimate strength, lbs. per sq. in.	85,000
Elastic limit, " "	50,000
Elongation in 2 ins., per cent.	22
Reduction of area, "	45

The elastic limit shall be determined by extensometer. Above

40,000 lbs. per sq. in., each increment of load shall be not more than 1,000 lbs. per sq. in.

Specimen for Tensile Test.

6. The test specimen as shown by Fig. 1, 0.5-in. diameter and 2-in. gauge length, shall be used to determine the physical properties as specified in Paragraph 5. Tests specimens shall be taken from the crop end of one axle, shaft, or similar part, from each treating-plant heat; if more than one open-hearth heat is represented in a treating-plant heat, a test shall be taken from each open-hearth heat represented. A full-size prolongation shall be left on each axle, shaft, or similar part.

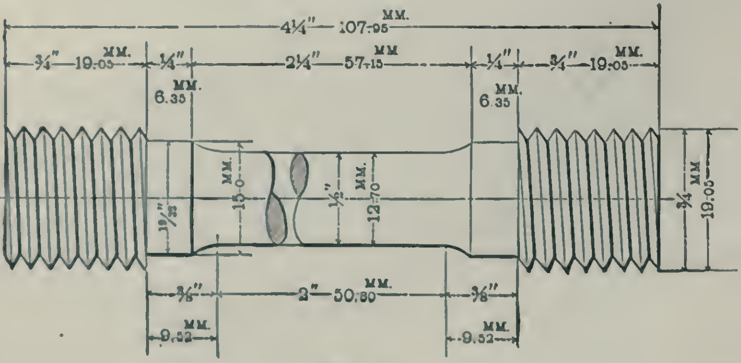

FIG. 1.

Cold Bend Test.

7. A cold bend test shall be made from the crop end of one axle, shaft, or similar part, from each treating-plant heat; if more than one open-hearth heat is represented in a treating-plant heat, a test shall be taken from each open-hearth heat represented. The test shall be made with a ½-in. square specimen, not exceeding 6 ins. in length, around a flat mandrel with edges of ½-in. radius, and the specimen shall bend, without fracture, 180° around the said mandrel.

Location of Specimens for Tensile Test and Cold Bend Test.

8. Specimens for tensile test and cold bend test shall be taken parallel to the axis of the axle or shaft and on any radius one-half the distance from the center to the circumference.

Re-testing.

9. In case the physical results obtained from any lot of axles, shafts, or similar parts, do not conform to those called for by Paragraphs 5 and 7, the manufacturer shall have the privilege of re-treating such parts, from which new tests shall be taken by the

purchaser, and these shall govern the acceptance or rejection of the lot.

10. Each axle, shaft, or similar part shall be allowed to cool after forging, shall then be re-heated to the proper temperature, quenched in some medium, allowed to cool, and then re-heated to the proper temperature for annealing. **Heat Treatment.**

11. Warped axles or shafts or similar parts must be straightened hot; that is, at a temperature above 900° F., and before offering the parts for test. **Warped Axles or Shafts.**

12. All axles, shafts, and similar parts shall be free from cracks, flaws, seams, or other injurious imperfections when finished. Those which show such defects while being finished by the purchaser will be rejected and returned to the manufacturer, who must pay return freight. **Quality.**

13. All axles, shafts, and similar parts must be rough-turned with an allowance of $\frac{1}{8}$ in. on surface for finishing, except on collar, which is to be left rough forged. Turning must be done on 60° centers with clearance drilled at point. **Finish.**

14. The heat number shall be stamped on the rough forged collar. After rough turning, the manufacturer's name, heat number, individual axle or shaft number, and inspector's mark shall be stamped at place indicated by the purchaser, except at any point between the rough collars. **Branding.**

15. The inspector representing the purchaser shall have free entry, at all times while his contract is being executed, to all portions of the manufacturer's shop which concerns the manufacture of material ordered. All reasonable facilities shall be afforded to the inspector by the manufacturer to satisfy him that the axles, shafts, and similar parts are being furnished in accordance with the specifications. All tests and inspection shall be made at the place of manufacture prior to shipment and free of cost to the purchaser. The purchaser shall have the right to make tests to govern the acceptance or rejection in their own test-room, or elsewhere, as may be decided by the purchaser, such test, however, to be made at the expense of the purchaser and to be made prior to the shipment of the material. Unless otherwise arranged, any protest based on such tests must be made within six days, to be valid. Tests and inspection shall be so conducted as not to interfere unnecessarily with the operation of the mill. **Inspection.**

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD SPECIFICATIONS FOR STEEL REINFORCEMENT BARS.

ADOPTED AUGUST 21, 1911.

Manufacture.

1. Steel may be made by either the open-hearth or Bessemer process. Bars shall be rolled from billets.

Chemical and Physical Properties.

2. The chemical and physical properties shall conform to the following limits:

Properties Considered.	Structural Steel Grade.		Hard Grade.*		Cold-Twisted Bars.
	Plain Bars.	Deformed Bars.	Plain Bars.	Deformed Bars.	
Phosphorus, max.					
Bessemer........	0.10	0.10	0.10	0.10	0.10
Open-Hearth.....	0.05	0.05	0.05	0.05	0.05
Ult. tensile strength, pounds per sq. in...	55,000-70,000	55,000-70,000	80,000 min.	80,000 min.	Recorded only
Yield point, min., pounds per sq. in...	33,000	33,000	50,000	50,000	55,000
Elongation, min. per cent. in 8 ins.......	$\frac{1,400,000}{\text{tens. str.}}$	$\frac{1,250,000}{\text{tens. str.}}$	$\frac{1,200,000}{\text{tens. str.}}$	$\frac{1,000,000}{\text{tens. str.}}$	5 per cent.
Cold bend without fracture:					
Bars under ¾ in. in diameter or thickness......	180°d. = 1t.	180°d. = 1t.	180°d. = 3t.	180°d. = 4t.	180°d. = 2t.
Bars ¾ in. in diameter or thickness and over..	180°d. = 1t.	180°d. = 2t.	90°d. = 3t.	90°d. = 4t.	180°d. = 3t.

* The hard grade will be used only when specified.

Chemical Determinations

3. In order to determine if the material conforms to the chemical limitations prescribed in Paragraph 2 herein, analysis shall be made by the manufacturer from a test ingot taken at the time of the pouring of each melt or blow of steel, and a correct copy of such analysis shall be furnished to the engineer or his inspector.

4. Chemical determinations of the percentage of phosphorus, shall be made by the manufacturer from a test ingot taken at the time of the pouring of each melt of steel and a correct copy of such analysis shall be furnished to the engineer or his inspector. A check analysis may be made by the purchaser or his representative if desired, in which case an excess of 25 per cent. above the required limits will be allowed.

Yield Point.

5. For the purposes of these specifications, the yield point shall be determined by careful observation of the drop of the beam of the testing machine, or by other equally accurate method.

Form of Specimens.

6. (*a*) Tensile and bending test specimens may be cut from the bars as rolled, but tensile and bending test specimens of deformed bars may be planed or turned for a length of at least 9 ins. if deemed necessary by the manufacturer in order to obtain uniform cross-section.

(*b*) Tensile and bending test specimens of cold-twisted bars shall be cut from the bars after twisting, and shall be tested in full size without further treatment, unless otherwise specified as in (*c*), in which case the conditions therein stipulated shall govern.

(*c*) If it is desired that the testing and acceptance for cold-twisted bars be made upon the hot-rolled bars before being twisted, the hot-rolled bars shall meet the requirements of the structural steel grade for plain bars shown in this specification.

Number of Tests.

7. At least one tensile and one bending test shall be made from each melt of open-hearth steel rolled, and from each blow or lot of ten tons of Bessemer steel rolled. In case bars differing ⅜ in. and more in diameter or thickness are rolled from one melt or blow, a test shall be made from the thickest and thinnest material rolled. Should either of these test specimens develop flaws, or should the tensile test specimen break outside of the middle third of its gauged length, it may be discarded and another test specimen substituted therefor. In case a tensile test specimen does not meet the specifications, an additional test may be made.

(*d*) The bending test may be made by pressure or by light blows.

Modifications in Elongation for Thin and Thick Material.

8. For bars less than $\frac{7}{16}$ in. and more than ¾ in. nominal diameter or thickness, the following modifications shall be made in the requirements for elongation:

(*e*) For each increase of ⅛ in. in diameter or thickness above

$\frac{3}{4}$ in., a deduction of 1 shall be made from the specified percentage of elongation.

(*f*) For each decrease of $\frac{1}{16}$ in. in diameter or thickness below $\frac{7}{16}$ in., a deduction of 1 shall be made from the specified percentage of elongation.

(*g*) The above modifications in elongation shall not apply to cold-twisted bars.

Number of Twists. 9. Cold-twisted bars shall be twisted cold with one complete twist in a length equal to not more than twelve (12) times the thickness of the bar.

Finish. 10. Material shall be free from injurious seams, flaws, or cracks, and have a workmanlike finish.

Variation in Weight. 11. Bars for reinforcement are subject to rejection if the actual weight of any lot varies more than 5 per cent. over or under the theoretical weight of that lot.

Inspection. 12. When an inspector is furnished by the purchaser to inspect material at the mills, he shall have full access, at all times, to all parts of mills where material to be inspected by him is being manufactured.

DISCUSSION.

Mr. C. S. Churchill.—It seems to me that this report on steel wheels is a step in the right direction in that it has brought very valuable information together in the form of specifications; but the limits of the chemical constituents are in some respects a little wider than would obtain in good practice. As we all know, when specifications are made a little too liberal it is rather difficult to get back again to close limits. I think that these specifications on steel wheels should be received as a report of progress, and that they should be referred back to the Committee for further study. The process of manufacture of steel wheels is not a business that has been long in existence, and therefore new items of progress are liable to come up from time to time, so that I think it would be a mistake to submit these specifications to letter ballot at the present time. **Mr. Churchill.**

The President.—Mr. Churchill, would you indicate specifically the changes you would like made, so that they may be discussed on their merits? Of course we do not want to send out anything that is really premature; on the other hand it does seem, generally speaking, that specifications which are very nearly perfect are better than none. They may be improved from time to time, and it is better that the public should have some guide that is nearly exact rather than to have no guide. **The President.**

Mr. Churchill.—To illustrate: for solid rolled steel wheels for freight-car service, the carbon limits are given in the proposed specifications as 0.60 to 0.85 per cent.; manganese 0.50 to 0.80 per cent.; silicon not to exceed 0.35 per cent.; phosphorus not to exceed 0.06 per cent.; sulphur not to exceed 0.06 per cent. I think the limits for carbon are too wide, and that they should be from 0.65 to 0.85 per cent. In the case of manganese the limiting values are too low; the limits should be from 0.60 to 0.85 per cent. The silicon should not exceed 0.25 instead of 0.35; and the upper limit of phosphorus and sulphur should be 0.05 per cent. Those are some of the main points. My criticisms are based on actual results from long service of steel wheels com- **Mr. Churchill.**

Mr. Churchill. pared with other wheels in which service test the steel wheels used were all within these limits. It seems to me wise, therefore, to go a little slow and not adopt now such wide limits as are suggested.

Mr. Stevenson. MR. A. A. STEVENSON.—In the absence of Mr. Webster, Chairman of the Committee, who is now abroad, I, as Vice-Chairman of the Committee, will venture to reply to Mr. Churchill's remarks. It seems to us that, considering the amount of work that has been put on these specifications, covering a period of two and a half years, it would be rather unfortunate to turn them back to the Committee and cause another year's delay in the adoption of a rolled wheel specification. In our opinion the changes suggested by Mr. Churchill are not radical enough to make such action necessary. We do not think Mr. Churchill has taken into consideration the fact that the chemical composition given covers both acid and basic steel. The carbon limits as called for by the proposed specification are 0.60 to 0.85; the suggestion is to make these 0.65 to 0.85. This may be all right for basic steel, but we are sure the manufacturers using acid steel would not be satisfied to accept the lower limit of 0.65, but would not object to cutting the upper limit the amount suggested. Manganese limits as called for by the specification are 0.50 to 0.80. Mr. Churchill suggests making these 0.60 to 0.85; we feel 0.50 to 0.80 represents the best practice, although the lower limit might be raised 0.05 to bring the variation in limits to that suggested.

The silicon limit, as called for by the proposed specification, is not to exceed 0.35. Mr. Churchill suggests that this limit be placed at 0.25. For basic steel this is all right, but not for acid steel. For acid steel the upper limit should not be less than 0.30.

Mr. Churchill suggests 0.05 as the upper limit for phosphorus and sulphur. To repeat what has been said before, the specification covers both acid and basic steel, and the Society has adopted other specifications with 0.06 as an upper limit for acid steel. Rather than to postpone the adoption of these specifications for another year, we feel safe in saying that the manufacturers would be willing to accept an upper limit of 0.05 in phosphorus and sulphur.

Mr. Chairman, we feel it would be a mistake to return the report to the Committee for further consideration; in other words, what we want now is a specification for rolled steel wheels. Some of the railroads and manufacturers have specifications of their own; but at the present time I know of no specification that has been adopted by any technical society. It was a very difficult problem your sub-committee had before them at the time they began work on these specifications; both the consumers and manufacturers were somewhat at sea. **Mr. Stevenson.**

For two years and a half there have been numerous meetings of the sub-committee on the question of specification in an endeavor to arrive at something that would be fair to both the consumer and the manufacturer. The action of Committee A-1 upon the report of the sub-committee would indicate that, in their opinion at least, these specifications represent the practice of to-day. If these specifications are adopted, no doubt, like other specifications, they can and will be improved. If we had not adopted a rail specification some years ago, we would not have the one we now have, which is a decided improvement over the original specification adopted. We are in favor of submitting the points brought up by Mr. Churchill to the action of the members present, and then submitting the specification with any modification that might be made at this meeting to the Society for letter ballot.

Mr. Gibboney. MR. J. H. GIBBONEY.—The question of steel wheels, Mr. Chairman, is one of vital interest to the railroads, no doubt more so than to any other class of users, and the Norfolk and Western Railway has given this question considerable study, both from the standpoint of composition and the service obtained from different compositions. Our study, covering a period of some four years, during which time the service of several hundred wheels was closely observed, has impressed us very forcibly with the thought that narrower, instead of wider, limits are desirable for the carbon and manganese contents. Steel wheels should be studied in pairs, and it is desirable to have wheel mates as near the same hardness as possible, since the service of the softer wheel will determine the time the pair of wheels is to be removed for re-turning. For instance, if we mate a low-carbon with a high-carbon wheel, say a 0.60-per cent. with a 0.80-per cent., after approximately 10,000 miles have been made,

Mr. Gibboney. the soft wheel will commence to show very appreciable wear in the throat of the tread. This wearing away in the throat will cause the harder wheel to continue to crowd the softer wheel to the rail, which process will ultimately cause the removal of the pair of wheels for re-turning before the harder wheel has shown sufficient wear to warrant removal.

I would infer from Mr. Stevenson's remarks that this subject has received very careful study from the manufacturer's standpoint. However, I do not think that we should adopt a specification without first studying the effects from the user's standpoint, and for this reason I shall support Mr. Churchill in his motion to refer these specifications back to the Committee, with the suggestion that they seek data from the railroads with reference to the service they are obtaining, and the range for carbon and manganese they consider most desirable for satisfactory service.

The specification submitted by the Committee we find to be more elastic in its limits than the specification used by our road for the purchase of some 60,000 or more wheels. The manufacturer of these particular wheels has not experienced trouble in making them conform to these limits; therefore it appears to us absolutely unnecessary to widen the limits, especially when the same will tend to make the proper mating of wheels, as to hardness, more difficult.

Mr. Hunt. MR. R. W. HUNT.—I think I cannot quite sympathize with the Chairman in his expressed feeling that it is better to have *a* specification than *no* specification even if that specification is not perfect. I will admit that practically and probably no specifications are absolutely perfect; but unless you have good grounds for thinking that they are as perfect as you know how to make them and that they represent the results of the best experience, observation and practice, I think it is better to have none.

The steel wheel is a new product, and it is better to let that product fight its own battles for a little while. The manufacturer undoubtedly is endeavoring to make the best wheels that he can, and I presume that a certain guarantee goes with them as to the service they will render. Let us gather together data of the best service before this Society hands specifications to the world with their endorsement, so that we may feel that we have a basis on which to recommend. I do not believe we have those data yet in regard to steel wheels. I should be very glad to listen to an expression from a large user of wheels. I believe we can afford to go a little slow before we say that any specifications are accurate. **Mr. Hunt.**

Mr. Job. Mr. Robert Job.—I wish to endorse the views of the gentleman who has just spoken in regard to obtaining further data covering actual service and wear of the steel wheel, in view of the fact which appears that the Committee has not looked into that subject particularly. It seems to me it would be an excellent plan, as Mr. Churchill, Mr. Gibboney and Mr. Hunt have suggested, that the specifications be referred to the Committee with the request that they take up the matter particularly with the various railroads, the large consumers of wheels, in order to get such additional data as possible regarding the actual service wear of these wheels. It seems to me that if that step were taken it might add a great deal to the value of the specifications which the Society finally adopts. If we adopt these specifications now without having these data, it seems to me that we will have specifications which may be of very little value; but with the other additional information, which can be quite readily obtained, the specifications will be of great importance.

Mr. Kenney. Mr. E. F. Kenney.—I am not a member of this Committee, but I would like to correct the impression that seems to be prevalent that the large users of steel wheels are not represented on the Committee and were not taken into account. A representative of probably the largest user of steel wheels in the country was a member of this Committee, namely, Mr. E. D. Nelson.

Mr. Mahon. Mr. R. W. Mahon.—Let me say a word for this Committee, with reference to the point that was brought up by one of the speakers concerning different hardness in a pair of wheels. It is customary on the New York Central Lines to have this feature regarded in the mating. I think that the difficulty arising from difference in hardness of the two members of a pair on the same axle—that is, the more rapid wear of one wheel—can be very largely overcome by the system of mating which takes account of the carbon percentage. On the New York Central Lines the

Mr. Mahon. two wheels mounted upon the same axle never differ more than by 0.05 per cent. carbon.

Another point that I want to speak of is in connection with the carbon limits that have been specified. The wheels that are being furnished to us are actually within those limits; they extend precisely from 0.60 to 0.85, all of them. I do not think we have ever had a carbon less than 0.60, nor over 0.85. We have had both of those carbons repeatedly.

Mr. Gibboney. MR. GIBBONEY.—I should like to add that the same system of mating was attempted on our road, not only with reference to agreement in carbon, but also, for possibly closer mating, with reference to hardness by using the scleroscope. This method however was found to be impracticable, due to the large number of wheels requiring mating in a short period of time.

I should like to ask what the difference in wear was found to be between the softer and harder wheels, and what the carbon limits were?

Mr. Mahon. MR. MAHON.—There has not been sufficient experience so far on the New York Central Lines to show that feature, that is, the wear of rolled steel wheels; at least, it has not reached me. The use of steel wheels with us, except as to a few hundred wheels, is comparatively recent. One thing that has been spoken of is the difficulty of mating old wheels as to carbon, on account of not knowing their carbons. As to this, the carbon is stamped on all our wheels. It is placed just to the left of the billet number which the manufacturer uses to identify his product; so that there is no trouble in that respect.

Mr. Stevenson. MR. STEVENSON.—For the information of the Society we would say that the sub-committee had the specification in readiness to submit to the Society at the meeting two years ago, but it was held up at that time because we felt that we should have more results from actual experience before submitting it to the Society. Since, as well as prior to that time, there have been thousands and tens of thousands of wheels furnished to some of the leading roads to a specification very similar to the one we are submitting. Some of the leading roads are represented on the sub-committee, and we have waited these two years until the members of the sub-committee, representing both con-

Mr. Stevenson. sumers and manufacturers, felt that they were ready to submit the specification to the Society as representing good practice.

From some of the remarks that have been made, it would seem that the Society is under the impression that these are manufacturers' specifications. They are not. If, as suggested by one or two of the speakers, it would be wise to hold up the specification until such a time as we can arrive at a specification that is nearer perfection, it may be three, four, or five years before the Society will be ready to take action. It will take a number of years to learn just what is the best. In the meanwhile, the majority of consumers will not have any recognized specification to work to. As stated before, a specification practically the same has been worked to by some of the largest consumers of rolled steel wheels in the United States, and is giving satisfaction as a general thing to-day.

Mr. Gibboney. MR. GIBBONEY.—May I ask if the specifications of these railroads require a closer limit for carbon than the specifications in this report?

Mr. Stevenson. MR. STEVENSON.—I think not. I have not the exact figures, but I am very sure that the limits are the same.

Mr. Gibboney. MR. GIBBONEY.—Our road has been buying on a specification which is not as elastic as that contained in the proposed specification, Mr. Chairman, and we have had no trouble in getting wheels on this closer range; therefore we must conclude that the limits of our specification are sufficiently elastic to cover all contingencies of proper manufacture.

The President. THE PRESIDENT.—It would seem, in order that we may vote on this question more intelligently, that we ought to understand what damage would be done by sending out these specifications in their present form. We can understand that a certain good would be done, that is, that a certain guide would be furnished. It has been intimated rather than explained, as I understood it, that there would be some damage done from the fact that these specifications are not as good as they might be. It seems to be admitted that they are very good, but are not the best possible; they are susceptible of improvement. I think it would be well that we should understand wherein this slight divergence from perfection is going to work a damage in the next year or so, what harm is going to be done. I think we ought to under-

The President. stand that a little more fully. I do not mean to prejudice the matter at all, or to assume that the objections are not well taken; but I think they ought to be more clearly before us than they now are. Perhaps Mr. Hunt will explain the specific nature of the objections and the damage that would be done by using such specifications now rather than waiting a year.

Mr. Hunt. MR. HUNT.—My feeling is that this Society should not issue specifications on any subject unless they are based upon sufficient observation to satisfy us as to the results of those specifications, and to make us feel that they are the best that we can issue with the present knowledge. Now I do not feel that that is so. When our Society issues specifications they are accepted by the world as representing the best opinion of very distinguished gentlemen; and we do not want to put ourselves in the position of issuing them simply as a matter of utility.

There are one or two other points in these specifications that I think are not as they should be. Personally I voted against them in the Committee; but that has no bearing now. Again, since the manufacturers themselves are not asking for the latitude we are giving them, as I believe has been stated, it is, I think, a rather unique position to get into. Should we not recognize that condition by giving more time to see if we cannot get as close to the ideal as the manufacturers?

Mr. Marburg. MR. EDGAR MARBURG.—Mr. Chairman, I should be among the last to advocate the adoption of what may fairly be called inferior specifications; but since these specifications were framed for what is concededly a new product, it seems to me a reasonable line of procedure that they should be a little too lenient rather than too exacting concerning a product on which knowledge is very incomplete. If the specifications are too exacting, they will be rejected and will serve no useful purpose; if, however, they are somewhat too lenient, they will be tried out on their merits, unless they are palpably inferior. The Society has not a specification in its Year-Book of which it may not be said that there are more exacting specifications in use somewhere. The Society does not purport to put out specifications which are the most exacting that can be found anywhere; but rather specifications fairly representative of good current practice. That observation applies to every specification that has been put out by the Society, and it seems to me it ought to apply especially to specifications concerning a product that is concededly new. **Mr. Marburg.**

If in the course of the year experience should show the desirability of making certain requirements more exacting, that can readily be done. The history of the Society shows that many of the specifications have been thus revised from year to year. The only question is, shall we accept a specification for one year—not necessarily for longer than a year—that is concededly not an inferior specification. If it is concededly an inferior specification it ought to be rejected; but if it is concededly not an inferior specification, but one that is not as exacting as certain specifications in use, why should it not be adopted tentatively for a year? Any members who are particularly interested in this product, and who are particularly desirous that the specification shall be as stringent as circumstances may warrant, can offer their suggestions by letter to the Committee with the certainty that they will be given very careful consideration on their merits.

In view of the fact that these specifications stand for a great deal of work, and since they have received the approval of a member of the sub-committee who represents also what is perhaps the largest consuming interest in this line, it seems to me evident they do not deserve to be regarded as manifestly inferior specifications. In fairness, therefore, to the Committee, and in line with what the Society has done in the past in matters of this kind, it seems to me desirable to discuss now, and on their merits, the specific points about these specifications which, in the judgment of members here present, ought to be amended, and then to refer the specifications, either in their present or amended form, to letter ballot of the Society.

Mr. Hunt. MR. HUNT.—Since there seems to be some difference of opinion as to the specifications, and there have been statements made here by the consumers with reference to some of the requirements, I think it would be the part of wisdom to put the matter back in the hands of Committee A-1. I am a member of Committee A-1, not a member of the sub-committee; but I think that if we should put it back in the hands of Committee A-1 and suggest to the gentlemen who have criticized these specifications some-

Mr. Hunt. what adversely that they give us the benefit of their experience as consumers, that we will be in better shape next year to present specifications which will represent our best effort and be more perfect. I do not believe in simply issuing specifications in a tentative way. Personally, I would prefer to have the matter referred back to the Committee of which I am a member, expecting when it is referred back to get the benefit of the suggestions and criticisms that have been offered this morning. I think it would be a step in the right direction and the part of wisdom to do so. You know the specification was acknowledged by the Committee as not being perfect. There were some features of the specifications criticized in the Committee, and some features which are not in the specifications were referred back to the sub-committee to take up, with the idea of possibly incorporating those features next year.

The President. THE PRESIDENT.—Are there any further remarks? If not, I shall call for a vote first on the amendment to refer these specifications back to the Committee. Those in favor of referring these specifications back to the Committee will manifest it in the usual way. The motion appears to prevail. Is a division called for?

Mr. Stevenson. MR. STEVENSON.—Yes sir.

[A rising vote resulted in ayes, 49; nays, 25.]

The President. THE PRESIDENT.—The vote is, that these specifications shall be referred back to the Committee.

Mr. Hunnings. MR. S. V. HUNNINGS.—I notice in the proposed specifications for heat-treated steel axles that only the maximum carbon is specified. I think that the maximum carbon is considerably above what is recognized as sufficiently high to get the same tensile strength in ordinary annealed axles. I should like to ask Mr. Stevenson what the idea was in permitting as high as 0.60 per cent. carbon in the heat-treated axles in view of the tensile strength stipulated in the specifications.

Mr. Stevenson. MR. STEVENSON.—Is Mr. Nelson present? He is Chairman of the sub-committee. [Mr. Nelson was not present.]

As far as carbon limit is concerned, that was put in at the Committee meeting as a rejection limit. I think a number of the members of the sub-committee would have preferred not to have mentioned carbon at all; and in fact there are a number of specifications to-day for heat-treated axles where the carbon limit is not given; but some of the members felt that in view of what had been done in other materials, it would be wise to put in some limit for carbon; consequently it was decided to put 0.60 as the upper limit. **Mr. Stevenson.**

MR. HUNNINGS.—During the past two years we have done considerable heat-treated work and have had several failures. We have found that the most critical point we have to watch is the carbon, especially where the sulphur and phosphorus also approach the upper limit of the specification. The phosphorus and sulphur in these specifications are limited to not over 0.05 in both cases. The fact must be recognized that where you get a ladle analysis, or even actual analyses, from a couple of billets—previous to going ahead with your forgings—showing carbon, phosphorus and sulphur around the upper limits of these specifications, it is quite possible that some of the billets in the same heat may show these elements to be present in quantities considerably higher than allowed by these specifications. The possibility of the carbon running to 0.60, and at the same time the possibility of the phosphorus and sulphur equaling or exceeding the stipulated limits, would be a very serious matter in the heat treatment of forgings. **Mr. Hunnings.**

I believe also there should be a minimum carbon limit specified. Without a minimum carbon limit, the manufacturer could take 0.30 carbon steel and quench it, and as he is not required to "draw back" (anneal) after quenching, he could overcome the lack of carbon, either by not annealing at all, or annealing at a temperature too low to relieve the strains set up by quenching. In that way you are apt to get forgings which are in a treacherous condition.

MR. KENNEY.—The points referred to by Mr. Hunnings are, I think, taken care of in the requirements for ductility. Mr. Hunnings would get no such results under the requirements in these specifications by the treatment he suggests with 0.30 carbon. **Mr. Kenney.**

MR. RADCLYFFE FURNESS.—Mr. Hunnings spoke of the danger of phosphorus and sulphur running up with the carbon. I think if he will read the specification over he will see that one axle in every ten can be tested, depending entirely upon the heat **Mr. Furness.**

Mr. Furness. treatment; also that one axle from each melting heat should be tested. I think the possibility of getting high phosphorus and sulphur is very small, because no manufacturer is going to put any more tests upon himself than he is compelled to. I think as you have two heats, and as you test one axle in every ten, the chance of getting carbon high is practically eliminated.

Mr. Hunnings. MR. HUNNINGS.—We have followed the practice of testing one axle in ten, yet we have had in our shops failures of axles tested and accepted under such conditions. Regarding the point Mr. Kenney raises, we have found that a driving axle showing approximately 0.30 carbon may be quenched and not "drawn back," and yet give 22 per cent. elongation.

Mr. Mahon. MR. MAHON.—Would it not be well to give a little further consideration to that matter? Does not that 0.60 carbon seem like a concession to the manufacturing side which they can avail themselves of if they wish, but which, as I think Mr. Hunnings has very clearly pointed out, if accompanied by the proper and customary heat treatment, will lead to results which we do not want? Would it not be better to consider the possibility of putting somewhat lower limits in connection with carbon? That seems to me like a point which the Committee can consider with advantage. I do not want all of the specifications referred back to the Committee for further consideration; but I do not see that that criticism has been answered. I should be glad to hear any answer that anyone has to make to it, but I have not heard an answer which seemed to imply that it was necessary to go as high as 0.60 in carbon on these steels in order to get the physical results which the specifications require.

Mr. Kenney. MR. KENNEY.—We have been working at Cambria under similar specifications to those here proposed, and they have always been considered very severe specifications. The carbon in the steel which we have to use to get the desired results amounts to about 0.50 to 0.55 per cent., although it frequently rises to a little above 0.55. I doubt very much if Mr. Hunnings would get the results which are required by these specifications, a certain tensile strength, a certain elastic limit, elongation, and reduction of area, and a certain cold-bend test—all of which are required—by quenching 0.30, or anywhere near 0.30 carbon steel, without any drawing at all. I think that if instead of making a few experimental tests, he had been making axles of this size commercially, he would know that those sporadic cases of his do not represent what can be done regularly in practice. This must be regarded as a commercial proposition. **Mr. Kenney.**

MR. HUNNINGS.—When we can obtain from 80,000 to 95,000 lbs. per sq. in. tensile strength from ordinary annealed 0.40 to 0.50 carbon-steel driving axles, why is it necessary, in view of the increased strength obtained by heat treatment, to raise the maximum carbon to 0.60 per cent.? That is my principal objection to these specifications. While the absence of a minimum carbon limit will permit a wider range of chemical composition than is desirable, I do not think this as serious as the higher carbon permitted by these specifications. **Mr. Hunnings.**

MR. C. F. W. RYS.—In view of the fact that these specifications not only cover steel axles but also shafts and similar parts, it is quite possible that material will be ordered under these specifications of much larger sizes than the standard axle sizes. The limit of 0.60 per cent. carbon was put into the specification simply to protect the consumer against the use of excessively high carbon heats for forgings of this kind. It should not be necessary to specify the carbon at all, because the hardening elements in steel used under these specifications are naturally limited by the actual physical properties, especially the elasticity and ductility, which have to be met in testing such material. An undue amount of harmful impurities in the steel is determined by the specified check analysis. In my opinion the amount of carbon in such steel should be left entirely to the manufacturer, as he is interested in producing the highest quality of material by his best practice, and this can be fully checked by the purchaser in making the specified physical tests. I have, however, agreed to the maximum limit of 0.60 per cent. carbon. **Mr. Rys.**

REPORT OF COMMITTEE A-3 ON STANDARD SPECIFICATIONS FOR CAST IRON AND FINISHED CASTINGS.

Committee A-3 on Standard Specifications for Cast Iron and Finished Castings, has assigned its work to various sub-committees as follows:

- Pig Iron.
- Cast-Iron Pipe.
- Locomotive Cylinders.
- Car Wheels.
- Malleable Cast Iron.
- General Castings.
- Testing Cast Iron.
- Micro-Structure of Cast Iron.
- Influence of the Addition of Special Metals to Cast Iron.

The chairman of each of these sub-committees has been requested to furnish any matter that he might desire to have brought before the annual meeting.

While there has been some discussion of the details of various subjects, yet the only work that the Committee desires to present to the meeting is the proposed revision of the Standard Specifications for Locomotive Cylinders that has been put forward by the sub-committee on that subject. These proposed specifications were discussed at a meeting of Committee A-3 on June 10, and have been approved by letter ballot of that Committee. They are appended to this report.

Respectfully submitted on behalf of the Committee,

Walter Wood,
Chairman.

Richard Moldenke,
Secretary.

Note.—On the recommendation of the Committee, no action was taken by the Society on these proposed revised specifications. —Ed.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROPOSED REVISED STANDARD SPECIFICATIONS FOR LOCOMOTIVE CYLINDERS.

Process of Manufacture.

1. Locomotive cylinders shall be made from good quality, close-grained gray iron, cast in a dry mold.

Chemical Properties.

2. Drillings taken from test pieces as hereafter mentioned shall conform to the following limits in chemical composition:

Phosphorus..............	Not over	0.90	per cent.
Sulphur..................	" "	0.10	" "

In case of rejection on this analysis, the manufacturer shall have the option of analyzing drillings from the bore of the cylinder, upon which analysis the acceptance or rejection of the cylinder shall be based.

Physical Properties.

3. At the option of the purchaser, two test bars, each $1\frac{1}{4}$ ins. in diameter and about 14 ins. long, may be cast for each cylinder. When placed horizontally upon supports 12 ins. apart and tested under a centrally applied load, these test bars shall show an average transverse strength of not less than 3,000 lbs., and an average deflection not less than 0.09 in.

The above test pieces shall be cast on end in dry sand, the metal being taken from the ladle before pouring into the cylinder mold.

Chill.

4. Before pouring, a sample of the iron shall be taken from the ladle and chilled in a cast-iron mold of the dimensions shown in Fig. 1.

The sample shall be allowed to cool in the mold until it is dark red, or almost black, when it shall be knocked out and quenched in water. The sample, on being broken, must show a close-grained, well-mottled gray iron, with a well-defined border of white iron at the bottom of the fracture.

Character of Castings. 5. Castings shall be smooth, well cleaned, free from blowholes, shrinkage cracks, or other defects sufficiently extensive to impair the value of the casting, and must finish to blue-print size.

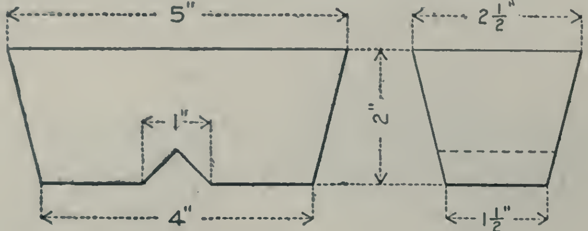

FIG. 1.

Each cylinder shall have cast on each side of the saddle the manufacturer's mark, serial number, date made, and mark showing order number.

The inspector representing the purchaser shall have all reasonable facilities afforded to him by the manufacturer to satisfy himself that the finished material is furnished in accordance with these specifications. All tests and inspections shall be made at the place of the manufacturer.

REPORT OF COMMITTEE A-4 ON HEAT TREATMENT OF IRON AND STEEL.

The Committee on Heat Treatment of Iron and Steel has been in existence a good many years, and has devoted itself principally to the work of research. A few years ago it offered a report which was in the nature of specifications, which were not pressed for adoption by the Society. This time the Committee appears before you, not with a set of specifications, but of practice recommended. We propose this practice recommended for annealing miscellaneous rolled and forged carbon-steel products.

A specification is something which is specific. These are generic; therefore we think they are not specifications, but are general rules covering heat treatment of rolled and forged carbon-steel products. It is thought well that the Society should stamp with its approval rules which may be used by steel makers and steel users who do not themselves possess great familiarity with the subject, and who need information. These rules may be useful also in the framing of specifications between buyer and seller. The Committee accordingly recommends that the "Practice Recommended for Annealing Miscellaneous Rolled and Forged Carbon-Steel Objects," appended to this report, be referred to letter ballot of the Society.

Respectfully submitted on behalf of the Committee,

H. M. Howe,
Chairman.

Note.—The Proposed Practice Recommended for Annealing Miscellaneous Rolled and Forged Carbon-Steel Objects was adopted by letter ballot of the Society on August 21, 1911, and follows this report.—Ed.

[For Discussion of this report, see pages 92–99.]

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PRACTICE RECOMMENDED FOR ANNEALING MISCELLANEOUS ROLLED AND FORGED CARBON-STEEL OBJECTS.

ADOPTED AUGUST 21, 1911.

1. *Purpose and General Procedure.*—The usual purpose in annealing miscellaneous forged or rolled carbon-steel objects is to remove existing coarseness of grain. This removal is brought about by heating the object to an annealing temperature, which varies with the carbon content of the metal. But the rate at which the object cools from this annealing temperature influences its properties very profoundly. Hence this rate of cooling should be suited to the duties which the object has to perform in service, and hence to the properties which we seek to give it.

Therefore these specifications first consider the annealing temperature to which the piece must be heated in order to remove existing coarse grain, and then the rate of cooling from that temperature.

Under certain special conditions an annealing is required in order to remove, not coarseness of grain, but the effects of rolling or otherwise working the metal at a temperature so low as to set up serious internal stress. Appropriate treatment for these conditions is given in Section 14.

As these specifications are intended to apply to a great variety of miscellaneous objects, they are purposely made suggestive rather than mandatory in many respects, because no single set of rules can be applied rigorously to such widely varying classes of objects and purposes.

ANNEALING FOR REMOVING EXISTING COARSE GRAIN.

2. *Method of Heating.*—In the case of large objects, the heating of the interior of which lags behind that of the outside, though the early part of the heating may if desired be rapid, the final approach to the annealing temperature aimed at should be slow, so that the interior may be brought fully up to it without carrying the exterior far beyond it, because in general any needlessly high temperature is injurious, and tends to re-coarsen the grain. The temperature should be held long enough at the annealing point to insure that the whole of the interior reaches that point, and that the refining of the grain may become complete. An exposure of one hour should be long enough for pieces twelve inches thick. Thicker pieces need a longer heating.

3. *Control of Temperature. Pyrometers.*—For all important work in careful hands the use of some trustworthy pyrometer is strongly recommended. But most pyrometers should be checked frequently against some standard. For those who are unwilling to take this trouble it is safer to rely on a trained eye than on an unchecked pyrometer. The operator should have clearly before him the fact that no pyrometer indicates the temperature of the interior of the object heated, and that the temperature which most pyrometers indicate is one between the temperature of the outside of the object heated and the temperature of the flame which is supplying the heat. Wherever practicable the part of the pyrometer which is supposed to reach the temperature of the object heated should be in immediate contact with that object, and should be protected from the flame or other source of heat by a suitable insulation, as for instance by covering it with sand. In important cases the gas or other source of heat should be shut off for at least ten minutes before taking the observation.

4. *Control Without the Use of a Pyrometer.*—In working without a pyrometer and relying on the eye, the light surrounding the furnace should be dull, and should be kept as nearly constant as practicable, in order that the eye may not be misled by the changing contrasts between the surrounding light and that of the object heated. In particular, direct sunlight should be excluded, and any arc or other strong lights should be so placed that neither they themselves nor any concentrated part of their light is in the field of the operator's sight when he is estimating by eye the temperature

of the objects to be annealed. Allowance should be made for the brighter surroundings by day than at night, and on sunny than on dark days.

5. *Magnetic Indications.*—The annealing temperature for steels containing between 0.50 and 0.90 per cent. of carbon is that at which the metal suddenly ceases to be magnetic. This fact may sometimes be used for the purpose of fixing or verifying the annealing temperature.

6. *Annealing Temperature.*—In general, the higher the carbon content the lower should be the annealing temperature. Hence different temperatures are given for different ranges of carbon content. Further, in order to bring the interior of large objects up to an effective annealing temperature, their outside may often be raised advantageously somewhat above that temperature. Hence for each range of carbon content a range of temperature is given. The upper limit of this range applied (1) to larger objects, and (2) to the lower part of the range of carbon content given.

The following ranges of temperature should be used for the several ranges of carbon content indicated. They refer to the usual moderate manganese content. For steels with a manganese content greater than 0.75 per cent. slightly lower temperatures suffice.

Range of Carbon Content.	*Range of Annealing Temperature.*
Less than 0.12 per cent.	875 to 925° C. (1607–1697° F.)
0.12 to 0.29 " "	840 " 870 " (1544–1598. ")
0.30 " 0.49 " "	815 " 840 " (1499–1544. ")
0.50 " 1.00 " "	790 " 815 " (1454–1499. ")

7. *Care in Heating.*—Care should be taken that all parts of the object reach the same temperature. To that end it may be necessary to mask from the heat, by means of bricks, the thinner part of objects of varying thickness. When the heating is nearly finished, these bricks may be removed. In particular the flame should never be allowed to touch any part of the object under treatment.

8. *Cooling.*—After the object has been held at the annealing temperature long enough to make its temperature nearly uniform throughout, and to complete the refining of the grain, it should be cooled in a way suited to its carbon content and to giving it the specific properties needed. The general principles are: first,

the higher the carbon the slower should be the cooling; and second, the slower the cooling the softer and more ductile the metal will be, and the lower will be its tensile strength, elastic limit, and yield point. The greatest softness and ductility are obtained at a certain sacrifice of strength and elasticity, and the greatest strength and elasticity at a certain sacrifice of softness and ductility.

For most purposes neither of these extremes is desired, and it is not only sufficient as regards quality but economical to remove the object from the furnace as soon as it has been thoroughly annealed, and to allow it to cool in air, always completely protected not only from rain and snow but from sharp drafts of air. Objects containing more than 0.50 per cent. of carbon should cool more slowly till the color dies out, say at 500° C. (932° F.), as for instance by leaving them in the furnace. They may then be removed and cooled in air. Further, thin objects containing between 0.25 and 0.50 per cent. of carbon should be treated like those of 0.50 per cent. of carbon, unless they can be so massed together that their collective bulk will retard their cooling, so that they will collectively cool even in air with moderate slowness, like single large objects.

SPECIAL ANNEALING METHODS.

9. The foregoing methods are economical because they release the furnaces early for further use. In case special qualities are desired the following methods may be used.

10. *To Give the Greatest Softness and Ductility* of which the metal is capable, even at a certain sacrifice of strength and elastic limit, for instance for ease of machining or to resist a small number of severe distortions, the metal should be cooled slowly, either within the furnace, or in the case of large objects, under a cover of lime, clay, or other slow conductor of heat. The slower the cooling and the lower the temperature to which slow cooling is carried, the softer and weaker will the steel be. But for most cases for which even unusual softness and ductility are required, it suffices to remove the object from the furnace when it has become dead black, and to cool it thenceforth in air.

11. *To Give Great Tensile Strength and High Elastic Limit* even at a certain sacrifice of ductility, the cooling should be more rapid, the rapidity to be governed by the thickness and carbon

content of the object. Thin objects and those with high carbon content cannot stand so rapid a cooling as thick and low-carbon ones, lest their ductility be too far sacrificed. For instance, thick objects with less than 0.50 per cent. of carbon may be cooled completely in air, of course protected from rain or snow. Objects with 0.50 per cent. of carbon or more, and thin objects with from 0.30 to 0.50 per cent. of carbon, may be cooled in air if their cooling is somewhat retarded, as for instance by massing them together, as happens in the case of rails.

12. *To Give an Unusually High Combination of Ductility with Tensile Strength and Elastic Limit. Water or Oil Quenching and Annealing.*—This process needs great care and intelligence, and should in general be used only by those familiar with high-grade steels. After holding at the annealing temperature suited to the particular steel, as indicated in Section 6, the object is quenched in oil, which should be kept in circulation. It may be quenched in water if its carbon content is so low and its shape so simple that it is not in danger of cracking or receiving permanently harmful stress. In any event the danger of such cracking and stress is lessened by removing the object from the oil or water before it has cooled completely, say when its temperature has fallen to 160° C. (320° F.). It should if possible not cool below 100° C. (212° F.) and certainly not below 20° C. (68° F.). The annealing should begin within a very few hours after the quenching and if possible without ever allowing the piece to cool below 100° C. (212° F.) and certainly not below 20° C. (68° F.).

The steel thus hardened should next be annealed by heating to a temperature suited for giving the properties needed. In general the higher this annealing goes, the more ductile will the steel become, and the lower will be its strength and elastic limit.

For very high elastic limit and tensile strength, anneal at 500° to 650° C. (932° to 1202° F.). In this case the ductility will be low. Some steels, such as watch-springs and shafting, are annealed at 350° C. Little commercial annealing is done below 500° C.

For intermediate tensile strength, elastic limit, and ductility, best suited to the majority of cases, anneal at 600° to 650° C. (1112° to 1202° F.).

For the greatest ductility, with good strength and elastic limit, anneal at from 725° to 750° C. (1337° to 1382° F.).

The object should be held at this second annealing temperature long enough not only to allow its interior to reach it, but also to relieve the stress given in the water or oil quenching. For pieces of moderate thickness a four-hour exposure should suffice.

13. *To Give a Moderate Increase of Strength and Elastic Limit above that Given by a Simple Slow Cooling, without the Full Sacrifice of Ductility which a Simple Air Cooling would Cause.*—After holding at the annealing temperature suited to the carbon content, as indicated in Section 6, hasten the cooling till the object is at a dull red, say 650° C. (1202° F.), and henceforth cool slowly. The hastening of the cooling may be brought about in the case of thin objects in relatively shallow furnaces by opening the furnace door till the temperature falls to dull redness; or by running the objects out into the air on a movable car-bottom, and returning them to the furnace when they have cooled to dull redness; or even, in the case of objects with not over 0.30 per cent. carbon and not too thin, by a temporary immersion in oil or even water, followed by a return to the furnace. In cases in which such operations are to be performed frequently, special unfired chambers may be used for the final slow cooling, thus leaving the annealing furnaces proper available for their regular work. The results obtained in this way are not as good as those obtained with the method described in Section 11.

14. *Special Annealing to Remove the Effects of Rolling or Otherwise Working the Object in the Cold or at any Unduly Low Temperature.*—The object should be heated to about 775° C. (1427° F.) and cooled with a slowness which should increase with the thickness, i. e., the least dimension of the piece. In the case of thin plates a mere heating to 725° C. (1337° F.), followed immediately by slow cooling, should suffice. In the case of thick forgings, in which much time may be needed to allow severe stress to relieve itself, the sojourn at 775° C. (1427° F.) may be prolonged for several hours, though always at the cost of superficial decarburizing.

Such annealing for steel containing less than 0.15 per cent. carbon should be at 900° C. (1652° F.). Great brittleness may be caused by annealing very low carbon steel in the neighborhood of 700° C. (1292° F.), after cold working.

DISCUSSION.

Mr. Howe. Mr. H. M. Howe (*Chairman, Committee A-4*).—The Committee has enlarged itself by adding representatives of the manufacturers, Mr. Acker, Mr. Unger, Mr. Furness and Mr. Kenney. A meeting of the Committee was held yesterday (June 27, 1911), at which these gentlemen were present, together with Mr. Christie, of the Executive Committee, Mr. Aertsen, and Mr. Zimmerschied; and this recommendation practice was gone over very carefully paragraph by paragraph. A few changes of form—they were all practically of form—were introduced, and the report was unanimously passed at this meeting, which was a meeting of the Committee itself plus a number of gentlemen invited for the purpose of aiding the Committee with their counsel. Through a misunderstanding which need not be gone into now, these recommendations have not been printed, and they are here only in typewritten form. It is a matter dependent on one's point of view as to whether it is advisable that such recommendations—such recommended practice—should bear the stamp of the Society, that it should even be referred to letter ballot, before the meeting as such has had a chance to examine it in detail. Of course those who believe in strict adherence to precedent and law have the law on their side. There are others who look to the question of utility rather than the law. This recommended practice was manifolded and sent to every member of Committee A-1, and the only recommendation that was made by that Committee was adopted as an admirable one. I do not want to go so far as to say that that Committee has considered it in such detail as to have brought out every recommendation which they had to make; but their recommendation was that this report should be presented and passed as a report.

I appreciate the legal weakness of our own position, yet nevertheless I make the motion that this recommended practice be submitted to the Society for letter ballot. [Motion seconded.]

I make that motion on the ground that the Society never **Mr. Howe.** has endorsed any directions for heat treatment, and that we believe that such directions will be of value to a great number of people. The Committee does not propose to stop here, but proposes to go right on to increase, to improve, and probably to enlarge these rules. I can say unofficially that the Committee will be glad to get from the Society, or from anybody else who is informed on these matters, all possible light, and that all such recommendations will be given careful consideration.

MR. MANSFIELD MERRIMAN (*in the Chair*).—Gentlemen, the **The Chairman.** motion is made and seconded that these general rules or recommendations for the heat treatment of iron and steel shall be received and sent out for letter ballot of the Society. Are there any remarks thereon?

MR. R. W. HUNT.—May I ask if there are any reasons why **Mr. Hunt.** the report cannot be read to us?

MR. HOWE.—No reasons, sir, except the matter of time. It **Mr. Howe.** can be read if called for.

MR. HUNT.—I think we will all accept with humility and **Mr. Hunt.** great confidence anything that this Committee does. At the same time we would vote a great deal more intelligently if the report were read. Knowing the condition of your voice I do not ask you to do so; but cannot you ask some one else to read it?

[At the invitation of Mr. Howe, the report was read by Mr. Albert Sauveur.]

MR. HUNT.—After listening to this most admirable report, **Mr. Hunt.** which shows so much labor and which certainly gives a great deal of information, it strikes me that it is not one which should be submitted to the Society for letter ballot. Some of us might not have sufficient knowledge to vote intelligently on many points. Again, it is not presented as a specific matter to govern the annealing process, but as a matter of information; and it strikes me that the better and certainly a respectful way to the Committee in recognition of their labors, would be to have the meeting receive their report, and have it spread upon our minutes, and that it go before our Society in our papers published to the world, as a valuable contribution, but not as specifications. I would therefore move that that action be taken. [Motion seconded.]

The Chairman. THE CHAIRMAN.—The Chair understands that the motions that were made previous to the reading of the paper have been withdrawn, and the motion now before you is that this report be received, spread upon the minutes, and printed for the information of the Society and the world. Are there any remarks on that motion?

Mr. Howe. MR. HOWE.—Mr. Chairman, I appreciate Mr. Hunt's remarks. My feeling is, however, that we want something a little more than that. I admit that it is a question whether that "more" should be given immediately, or should be postponed; but my feeling is that something more should be done with the report than simply to spread it upon the minutes, which does not carry very much weight.

The specifications of this Society as such carry weight because they have been adopted and certified by the Society. It may not be so pressing in cases of this kind as in the case of a specification; the conditions are not exactly alike; but I do not know that there is a very great difference. I should like to see those recommendations for heat treatment adopted by the Society. I assume that when these letter ballots go out for action every member who does not feel competent to vote will say to himself, "I do not know anything about the subject; I cannot vote intelligently, so I will not vote." I do not take it that members possessing the intelligence of the individuals comprising this body will vote "in the air" or cast their votes at all on questions which they feel themselves incompetent to answer. I do not think that is the state of the case. I think that would meet part at least of what Mr. Hunt has in mind.

On the other hand, there are a great many members of the Society who have considerable knowledge of one kind or another on all these points which are here brought out; and I think that these recommendations should go before these gentlemen quietly at their homes or at their offices—wherever they burn the midnight oil—so that they will have a chance to look them over in detail, and if they do not approve of them to say so frankly. But I do feel that the good which these rules would do would be greater if they were endorsed by the Society, and that thereby they would carry much more weight than by simply going out over the names of any commit-

Mr. Howe tee, no matter how illustrious that committee might be. In this particular case, with the pains that have been taken to consult with those who are best informed in all countries, and especially in this country, and with the agreement which has been reached without opposition or difference of opinion, the unanimous agreement that has been reached on all of these specific points has certainly raised such a presumption in favor of these rules being of value, that I think the Society can profitably send them out to the members to read at home and vote on, or not vote on, according to whether they feel competent to vote, or incompetent. I hope that action will be taken. Therefore, I move as an amendment to Mr. Hunt's motion, that this report be sent out to letter ballot of the Society as recommended practice. [Motion seconded.]

The Secretary. MR. EDGAR MARBURG (*Secretary*).—Mr. Chairman, the difference in effect of Mr. Hunt's motion and that of Mr. Howe, in so far as the members of the Society are concerned, lies essentially in this: if Mr. Hunt's motion should prevail this report would be printed once in our Proceedings, and afterwards lost sight of by most of our members; while the other motion would give the report a permanent place in our Year-Book, as "recommended practice." The conditions in this case, it seems to me, are analogous to, if not identical with, the conditions surrounding the now standard methods of testing which appear in the Year-Book. These standard methods of testing are not incorporated in any specifications. It might have been well in this case too to have used the words "Standard Methods of Testing" in the title, instead of the words "Recommended Practice," since the conditions are essentially alike. Personally I should regard it as unfortunate if a report of this kind should be printed once in the Proceedings and not given a place in the Year-Book.

Mr. Hunt. MR. HUNT.—I certainly do not wish to favor any action that will cause that valuable report to become pigeon-holed. At the same time I still feel as though the letter ballot proposition ought to be on a thing that has to be settled yes, or no, as the action of the Society. If all votes are cast with the consideration that Mr. Howe outlines, it will be a very valuable endorsement. Would it be parliamentary and consistent with our rules if my

Mr. Hunt. motion should be amended so as to read that the report be received and printed in our Year-Book? I agree that it is a live subject which ought to come before our attention; it is a treatise on heat treatment that is very valuable. There are definite points given. At the same time we all know that heat treatment is like tempering, that one man with the same instrument and with the same tools and the same facilities will produce an entirely different result from another man. We have to consider the value of personal skill in handling heat as to the effects we get from it. Now this is a treatise on the subject; in fact, information that goes beyond a mere treatise, and therefore it is very valuable, and we ought to have it where any of us can refer to it. If the ordinary course does put it in a place where the members cannot turn to it with convenience, I would amend my motion to that it be received and ordered printed in our Year-Book. May I do that?

The Secretary. THE SECRETARY.—Undoubtedly.

Mr. Hunt. MR. HUNT.—Then, with the permission of the Secretary, I will amend my motion to that extent.

Mr. Howe. MR. HOWE.—I do not think that is such an endorsement as the Committee will be quite willing to accept. Mr. Hunt thinks so much of us that he wants the meeting to pass this into the Year-Book. That is what we are after too; but he wants to pass it into the Year-Book without submitting it to the criticism through letter ballot of the members of the Society. I feel that the Committee would like to have this report perfected by careful study by the members at home; opportunity for which study would be given by the proposed letter ballot. If it should appear that we have erred, the letter ballot would decide that. Whereas if you pass the motion last made, and as now amended, you are accepting our report without giving the members of the Society any chance to study it in detail, as it should be studied in detail, by those who are familiar with the subject.

The Chairman. THE CHAIRMAN.—The Chair asks one moment to state his own personal belief in regard to this matter. This is a society for testing materials and for studying tests after they have been made. Thus our Committee on Methods of Testing has reported standard methods for conducting tests upon which letter ballots have been had. This has been cited as an argument in favor of dealing similarly with the report before us; but it seems to me that this report deals with methods of manufacture, a subject very different from anything that has come before this Society during its history. If we are to open the door to submit to letter ballot methods of manufacture, it is probable that we shall have plenty to do in future years. **The Chairman.**

MR. J. H. PARKER.—In view of the discussion which has arisen regarding the report of this Committee, it would seem best to have the report printed and sent to the members of the Society for consideration next year, in the meantime referring the report back to the Committee to be brought up again at the next annual meeting. That would give the members of the Society a chance to examine it and intelligently discuss it next year. **Mr. Parker.**

THE SECRETARY.—In the abstract such a procedure might have the effect indicated; but practically it does not work out that way. We have had experience in such matters time and time again. Attention for the moment is centered on this particular subject, but if the report is printed in the Proceedings, even though it should be stated that discussions for next year are particularly desired, busy men are not apt to respond to anything so indefinite. It is hardly to be expected, therefore, that the conditions next year would be found to differ essentially from the conditions now, unless the Committee should in the meantime find good reason to modify its present recommendations. **The Secretary.**

MR. HOWE.—I think the Secretary has commented wisely on this. We never put out any perfect documents in this Society. We want to do some work on this while we are alive. A year is a year. Now a great deal goes on in a year. There is a great deal of steel made in a year by a great many men. In this coming year there will be a great many men who will want these figures for their information. If we put this forth as being worth something, they will have confidence in this Society, and perhaps in the course of the year we shall be able to stir somebody up to think of some points in which these rules can be improved. If you lose a year what have you to show for it? Perhaps thirty or forty years will limit the life of most of us, and who wants to waste a year? What are you going to gain by post- **Mr. Howe.**

Mr. Howe. poning for a year? Nothing would ever have been accomplished by this Society if at every annual meeting when a report came up a remark had been made that this specification might be improved in some minutæ, and therefore ought to be referred back to the Committee. If you wait until you can achieve perfection you will never have any specifications. The thing is to get work done well and done thoroughly, but not wait for perfection. It is not a practicable scheme to wait for perfection. You will never get anything done that way. These matters are matters of expert judgment, and have been very carefully considered over a long period of time by men who are competent. While a large number of men know about this, that, or the other particular matter, there are a comparatively small number that know about the question of heat treatment as a whole in definite, precise figures; and I think that the gentlemen who prepared this, and those who have been consulted, constitute a body of great expert authority, sufficiently expert authority to warrant this Society in sending out their report for consideration by the members. I think Mr. Hunt's suggestion is a good one, that those who have any criticism of these figures to offer should stand up now, or else forever afterward hold their peace. Here are the figures. We are prepared to stand or fall on them. Any of you who think these figures are not right, stand up and say why they are not right and why you think so; if not right, we will change them.

The Secretary. THE SECRETARY.—I should like to say just a few words in answer to the comments of the Chairman, Mr. Merriman, that it might lead too far afield if the Society should attempt to endorse methods of manufacture. If that criticism is valid it should have been made at the time this Committee was appointed. We have had a Committee for years on the heat treatment of steel. It hardly seems consistent when a report on this subject is brought here in good faith by the Committee to say, "You have made a report that really does not lie within the province of the Society to the extent of approving your recommendations by letter ballot." If in time the consideration of matters bearing on manufacture should come to be an evil, suitable steps may then be taken to check it, but that contingency appears to me to be rather remote.

THE CHAIRMAN.—The motion before us is that of Mr. Hunt, namely, that this report be received, be printed in the Proceedings, and also in the Year-Book, for the information of the members. The motion as amended by Mr. Howe is that it first be sent out to letter ballot. A vote on the amendment will be taken first. **The Chairman.**

[The vote on the amendment, and then on the motion as amended, resulted affirmatively.]

REPORT OF COMMITTEE A-5 ON THE CORROSION OF IRON AND STEEL.

A considerable amount of work has been done by the Committee during the past year on various methods for determining the weight of zinc coatings on iron and steel. The tabulated results of these investigations are not yet ready for presentation. It is, however, the unanimous opinion of the Committee that the well-known Preece copper-sulphate test is unreliable and should be abandoned entirely as a basis of specification with respect to galvanized sheet and plate. In respect to wire, the Preece test has the advantage of being quick and simple, and if carried out in the proper manner yields comparative results of value. In the opinion of the Committee, the lead-acetate test* is preferable to the copper-sulphate test for determining or specifying the weight of zinc coatings.

Notwithstanding the fact that this Committee, in its annual report for 1909, pointed out as clearly as it could that the tentative suggestions made by it in 1907 as to the conditions for carrying out the so-called acid corrosion test were not to be considered a recommendation of the test, and that the results of such test are unreliable as truly measuring the tendency to natural corrosion, the name of the American Society for Testing Materials continues to be used as having recommended the acid corrosion test, and by inference as having endorsed the same. For this reason the Committee desires at this time to again disclaim any recommendation or endorsement of the acid test as a measure of natural corrosion, and to point out that any use of the name or authority of the American Society for Testing Materials in this connection is unwarranted.

An analysis of results of the official inspection of the fence-wire tests, Carnegie Technical Schools, Pittsburgh, Pa., November 30, 1910, is appended hereto.

A. S. Cushman,
Chairman.

W. H. Walker,
Secretary.

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* For description of this test see Walker and Patrick, *Journal, Ind. and Eng. Chem.*, April, 1911.

APPENDIX.

ANALYSIS OF RESULTS OF OFFICIAL INSPECTION OF FENCE-WIRE TESTS, CARNEGIE TECHNICAL SCHOOLS, PITTSBURGH, PA., NOVEMBER 30, 1910.

The tabulated results of the official inspection of the Pittsburgh fence-wire tests are given in Table I. These tests fall naturally into a number of different groups or headings, under which they may be considered. These groups or headings are as follows:

1. Standard galvanized Bessemer-steel woven wire fences, such as are in use throughout the United States.

2. Woven wire Bessemer-steel fences protected by special methods of zinc coating: (1) Sherardized, (2) electro-galvanized, (3) unwiped galvanized, (4) galvanized after fabrication.

3. Bessemer-steel bare- or black-wire woven fences protected by various paint coatings.

4. Standard hot-dip galvanized wire fences protected by various paint coatings.

5. Standard galvanized woven wire fences made of steel selected from different parts of Bessemer heats and from different portions of the ingots.

6. Straight-line wire galvanized by the hot-dip method but unwiped, the wires being made from steel of varying chemical compositions.

Studying the results under these various headings, the following conclusions are clearly justified:

1. The Standard galvanized woven wire fences, such as are supplied for common use under the trade conditions which now maintain in the United States, are made of hot-dip galvanized wire wiped through asbestos wipers, before the wire is fabricated into fencing. These test fences were erected between September 3 and September 26, 1908. All these fences have failed, as far as the protective zinc coating was concerned, in the Pittsburgh atmosphere in less than two years, as they had been entirely rusted for some time previous to the date of the official inspection. Up

TABLE I.—TABULATED RESULTS OF OFFICIAL INSPECTION OF FENCE-WIRE TESTS, CARNEGIE TECHNICAL SCHOOLS, PITTSBURGH, PA., NOVEMBER 30, 1910, BY SPECIAL SUB-COMMITTEE OF COMMITTEE A-5.

Field No.	Inspection No.	Description.	Condition.	Numerical Ratings.						Remarks.
				Aupperle.	Buck.	Cushman.	Rodgers.	Speller.	Average.	
1 N.	27	Sherardized woven wire fence.	Very good......	10	10	9	10	9	9.6	
1 S.	26	Electro-galvanized wire fence.	Good..........	10	8	9	9	9	9.0	
2 N.	25	Unwiped galvanized wire fence.	Rusted in a few spots only.	10	9	9	9	9	9.2	
2 S.	24	Standard galvanized woven wire fence.	Very bad failure.	0	2	1	1	0	0.8	
3 N.	23	Lead annealed bare-wire fence; one coat, No. 19 black paint.	Very bad failure.	0	0	0	0	0	0.0	Ungalvanized. Worst condition of entire series.
3 S.	22	Lead annealed bare-wire fence; first coat, zinc chromate, second coat, antoxide maroon.	Rusted in spots, otherwise fairly good.	8	8	7	8	7	7.6	Ungalvanized.
4 N.	21	Bare-wire fence, two coats green paint.	Both line and ties show rust.	5	3	2	2	2	2.8	Ungalvanized.
4 S.	20	Bare-wire fence, two coats graphite.	Very bad failure.	5	2	0	0	5	2.4	Ungalvanized.
5 N.	19	Standard galvanized woven fence made of Steel A-1.	Very bad failure.	0	1	0	0	3	0.8	
5 S.	18	Standard galvanized woven fence made of Steel A-2.	Failed badly.....	1	0	0	0	0	0.2	
6 N.	17	Standard galvanized woven fence made of Steel A-3.	Failed badly.....	0	0	0	0	4	0.8	
6 S.	16	Standard galvanized woven fence made of Steel B-1.	Failed badly....	0	0	0	0	0	0.0	
7 N.	15	Standard galvanized fence made of Steel B-2.	Failed badly.....	0	0	0	0	0	0.0	
7 S.	14	Standard galvanized fence made of Steel B-3.	Failed badly.....	0	0	0	0	0	0.0	
8 N.	..	Unwiped wires C-1..........	No failure......	10	10	10	10	10	10.0	High post.
8 S.	..	Unwiped wires C-2..........	No failure......	10	10	10	10	10	10.0	High post.

TABLE I. (*Continued*)

9 N.	13	Standard galvanized woven fence, painted one coat No. 39 yellow.	Good..........	10	10	10	9	9	9.6	Inhibitive paint.
9 S.	12	Standard galvanized woven fence, one coat dark slate, No. 17.	Rusted badly ...	2	2	0	2	2	1.6	Stimulative paint.
10 N.	11	Standard galvanized woven fence, dark brown, No. 1 paint.	Fairly good	9	8	8	7	10	8.4	
10 S.	10	Standard galvanized woven fence, one coat black paint.	Very bad; wires almost eaten through.	2	0	0	0	2	0.8	Stimulative action.
11 N.	9	Standard galvanized woven fence, one coat No. 3 white paint.	Bad; paint and wires failing.	2	2	1	1	5	2.2	Stimulative action.
11 S.	8	Standard galvanized woven fence, one coat No. 90 black paint.	Tie wires bad; line wires fair.	7	5	5	5	5	5.4	
12 N.	7	Standard galvanized woven fence, one coat galvanized iron primer, and one coat antoxide.	No corrosion; good condition; paint slightly brittle.	9	9	9	9	10	9.2	Inhibitive action plainly shown.
12 S.	6	Standard galvanized woven fence, one coat No. 5 black paint.	Badly corroded..	2	2	2	1	2	1.8	Stimulative action plain.
13 N.	5	Standard galvanized woven fence, one coat No. 4 green paint.	Badly corroded..	2	3	3	0	4	2.4	
13 S.	4	Standard galvanized woven fence, one coat No. 8 green paint.	Tie wires bad; line wires fair.	5	5	4	4	4	4.4	
14 N.	3	Woven wire fence, galvanized after weaving.	Condition perfect.	10	10	10	10	10	10.0	
14 S.	2	Woven wire fence, galvanized after weaving.	Condition perfect.	10	10	10	10	10	10.0	
15 N.	1	Sherardized woven wire fence.	Fair, except certain sections which show considerable rust.	7	5	6	7	7	6.4	First Sherardized panel erected.

Note: "N." indicates "north." "S. indicates "south."

to the present time the rusting of the wires which make up these fences has not gone to the point at which they have begun to rust through and break. The conclusion seems to be clearly justified, that the protection afforded by abestos-wiped hot-dip galvanized coatings as they were manufactured two years ago, at the time of the inception of this test, was inefficient, as it did not last for two years in the Pittsburgh district. If we assume that this type of wire will last about twice as long in the purer air of rural conditions, the results have come out in accord with the claims made by farmers and other consumers in regard to the lasting quality of the zinc-coated wire as it was manufactured at that time. It is, however, only fair to the larger manufacturers who are manufacturing fabricated fences out of galvanized steel wire which they themselves manufacture, to state that they have been making great efforts during the past two years to improve the quality of their wire and the weight of the galvanized coatings. It would follow from this that if these tests were to be duplicated, starting from the present time, a much better showing would be made on the galvanized wire panels representing the stock wire under present trade conditions.

2. The woven wire fences with special methods of zinc protection, as represented in these tests, have all done well. It will be noted that there are two Sherardized panels on the fences, experimentally made for this test, as represented by Inspections 27 and 1. The No. 1 Sherardized panel shows considerable rust in spots. The second panel of Sherardized fence, Inspection 27, has given very excellent results in the test. It should not, however, be understood as a result of this report that the Committee recommends Sherardizing as a method of protecting wire. It is not here shown that the Sherardizing of wire is feasible in large-scale operations. The electro-galvanized panel, also an experimental product, and the panel made from unwiped galvanized wire, have both done well, and it cannot be said that the test has shown any difference in excellence between these two panels. In the case of these two panels also the Committee desires to point out that the results recorded are not necessarily to be taken as a recommendation of these methods of protection. Some members of the Committee have strongly objected, on the ground that the single electro-galvanized panel included is not to be considered as thoroughly representative of

electro-galvanizing in general; and furthermore, that there are certain objections to the general use of unwiped galvanized wire in large-scale operations for the protection of fabricated fencing. While these objections are valid and must be carefully considered, it would seem that the general indication of the test is that so far as the practice in vogue two years ago is concerned the close wiping of the galvanized coating was carried too far on hot-dip wire. The question as to whether electro-galvanized wire could be used in large-scale manufacturing operations has not entered into this report.

The two fence panels which were galvanized after fabrication have shown perfect resistance to the Pittsburgh atmosphere to date, and exhibit every indication of lasting in the same way many years longer. The conclusion seems to be justified, that there are ways of manufacturing fairly rust-resistant woven wire fences, and that the problem is simply one of cost of production and the price that the public is willing to pay for efficiently protected fencing.

3. It will readily be understood that the effort to protect bare- or black-wire fencing by means of paint coatings puts a very severe test upon the protective qualities of the paint used. A very decided difference is exhibited in the protective effects shown by the different paint compounds, and the results obtained under Tests 3 and 4 constitute in themselves an accelerated paint test of very high value. A careful inspection of the report of the tests will show that the paints have fallen into the classes of inhibitors and stimulators, as indicated by the recently developed electrolytic explanation of the corrosion of iron. The panel under Inspection 23, painted with one coat of No. 19 black, a theoretical stimulator, exhibited a very bad failure, worse even than the standard galvanized woven wire fence under Inspection 24. The bare wire protected by a theoretical inhibitor, under Inspection 22, is in astonishingly good condition for a wire protected only by a paint coating. In every case on the bare-wire painted panels, the theoretical stimulators of corrosion have proved to be stimulators under the exposure conditions.

4. The standard galvanized wire fences all of which failed badly when unprotected by paint coatings, have corroborated the results on the paint tests as noted under Group 3. Panel 13, painted with

a specially designed inhibitive coating, is in almost perfect condition. The same thing is true of the panel under Inspection 7. All the standard galvanized fence painted with theoretical stimulators have failed badly, as instanced by panels under Inspections 6, 9, 10 and 12. Green Paint 4, on the panel under Inspection 5, presents an apparent exception to the rule, as it was designed to be a protective coating, but has failed badly. This may or may not be due to the fact that it consists of a combination of pigments not well adapted to the vehicle in which it was spread upon the wire. On the Atlantic City steel test panels this same formula was found to be ground with insufficient oil, which may possibly have accounted for the results obtained in this case. With this exception, the inhibitive theory of paint protection of iron finds a wonderful corroboration throughout these tests. In almost every case where stimulative action is shown on account of the paint, the rusting of the wire appears to have gone further than in the cases where no paint at all was used. The conclusion is clearly justified that the selection of a properly designed inhibitive paint coating is of the utmost importance where the protection of iron and steel from atmospheric corrosion is the object in view.

5. Up to the present time no difference has appeared in the extent of corrosion suffered by the different panels of standard galvanized fencing made of steel selected from different parts of a Bessemer heat or different parts of Bessemer ingots from a heat. It is, of course, possible that when corrosion progresses to the actual falling to pieces of the fence through breaking of the wires, some difference may manifest itself, but up to the present time the selection of the best part of the ingot for wire drawing does not appear to be justified. It is to be regretted that these test panels did not include a number of fabricated fences made entirely of open-hearth steels, but the Committee desires to emphatically state that there was no intention of excluding material of this kind from the tests on fabricated fences. The design and preparation of these test panels involved, as will readily be understood, a very great amount of work on the part of volunteer contributors to the Committee, and at the time of the inception of the test everything was done that seemed possible at that time.

6. All the straight-line wires of No. 9 gauge that were drawn in order to test the longevity of galvanized wire made of steels of

different chemical composition, were galvanized in the same way that telegraph wire is usually galvanized, and were not wiped in asbestos wipers. All the results on these test panels show that unwiped wires are capable of resisting the corrosive effects of the Pittsburgh atmosphere for two years without showing failure. It is therefore too early to draw any conclusion in regard to these particular wires, which are shown in the table under Field Nos. 8 North and 8 South. They received no Inspection Numbers, as the inspectors finding them all in good condition up to the present time, did not take the time to look them over carefully, reserving the inspection for some future time.

In tabulating the report of the various inspectors, the chairman has selected from the individual reports made to him the descriptions of the conditions of the various panels in language that presents the opinion of all the inspectors. The numerical ratings were assigned by the chairman to the remarks on condition, supplied by the various inspectors in charge, in much the same way that examination papers are marked in college work. The five inspectors made their inspections independently of each other and may fairly be said to represent the opinion and judgment of five men all especially equipped by experience for inspecting galvanized metal.

REPORT OF COMMITTEE A-6 ON THE MAGNETIC TESTING OF IRON AND STEEL.

This Committee has held two well-attended meetings during the year to consider the specifications on standard magnetic tests of iron and steel. At these meetings the details of methods of testing have been considered, and the accompanying specifications have been prepared. These specifications have been approved by letter ballot of the Committee, and are now submitted to the Society for adoption.

In these specifications the use of the metric system seems justified for the following reasons:

First: Nearly all electrical work in this country is done at least partly in the metric system, and in the European countries it is universally so.

Second: The sort of testing which is covered by these specifications involves, more than any other sort of testing, materials which are imported from countries using the metric system exclusively.

Third: These specifications, as far as is consistent with the best American practice, agree with the recommendations of the Hysterse-Kommission of the Verband Deutscher Elektrotechniker, which are the only specifications of methods of magnetic testing adopted by any national association. These latter specifications, which have received the approval of so capable a body as the Physikalische Technische Reichsanstalt, are in the metric system.

Fourth: The European technical press entirely and the American technical press in great part use the metric system in articles dealing with magnetic quantities.

Fifth: The following letter of the Council of the International Association for Testing Materials deserves consideration.

Dear Sir:

We beg to draw your special attention to the following resolution, passed at the last Council's Meeting of the International Association for Testing Materials, held at Brussels in August, 1910, as to the introduction of the Metric System of Weights and Measures, and beg you in consideration of the extreme importance of the matter to give all the assistance in your power to the carrying out of its suggested introduction.

"The importance of the unification of weights and measures for international trade has induced most countries of the world to adopt the French Metric System as obligatory, with the sole exception of the English speaking countries, which have unfortunately not seen their way up till now, to accepting its compulsory introduction.

"The International Association for Testing Materials has for its special object the unification of methods of testing and the unification of specifications for materials of construction. The Association also strongly realizes that by the universal adoption of the Metric System the general progress in these matters would be greatly facilitated.

"The Association, consisting as it does of a large number of members of high position in the ranks of science and industry, and having an organization extending throughout all countries, feels it incumbent upon itself to publicly advocate on its own account the general introduction of the Metric System.

"Accordingly, it was unanimously resolved by the Council of the Association at their meeting at Brussels in August, 1910, to address to the Government Authorities and the learned and industrial Bodies of all countries in which the Metric System is not yet obligatory, an urgent request that they promote by every possible means the early adoption of the Metric System."

Believe us, dear Sir, to be,

In the name of the Council of the International Association for Testing Materials,

Yours faithfully,

ERNST RIETLER,
General Secretary.

HENRY M. HOWE,
Acting President.

Those who for their own shop practice wish to express their results in watts per pound may do so by the simple relations:

1 watt per lb. = 2.20 watts per kg.
1 watt per kg. = 0.454 watts per lb.

Respectfully submitted on behalf of the Committee,

C. W. BURROWS,
Chairman.

NOTE.—The Proposed Standard Magnetic Tests of Iron and Steel were adopted by letter ballot of the Society on August 21, 1911, and follow this report.—ED.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A.

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD MAGNETIC TESTS OF IRON AND STEEL.

ADOPTED AUGUST 21, 1911.

CORE LOSS.

The power consumption in electrical sheet steel when subjected to an alternating magnetization is known as the core loss. The standard core loss is the total power in watts consumed in each kilogram of material at a temperature of 25° C., when subjected to a harmonically varying induction having a maximum of 10,000 gausses and a frequency of 60 cycles per second, when measured as specified below. It is represented by the symbol $W_{10/60}$.

The ageing coefficient is the percentage change in the standard core loss after continued heating at 100° C. for 600 hours.

The standard core loss shall be measured under the following conditions:

The magnetic circuit consists of 10 kg. (22 lbs.) of the test material, cut with a sharp shear into strips 50 cm. ($19\frac{11}{16}$ ins.) long and 3 cm. ($1\frac{3}{16}$ ins.) wide, half parallel and half at right angles to the direction of rolling, made up into four equal bundles, two containing material parallel and two containing material at right angles to the direction of rolling, and finally built into the four sides of a square with butt joints and opposite sides consisting of material cut in the same manner. No insulation other than the natural scale of the material (except in the case of scale-free material) shall be used between laminations, but the corner joints shall be separated by tough paper 0.01 cm. (0.004 in.) thick.

The magnetizing winding shall consist of four solenoids surrounding the four sides of the magnetic circuit and joined in series. A secondary coil shall be used for energizing the voltmeter and the potential coil of the wattmeter.

These solenoids shall be wound on a form of any non-magnetic non-conducting material of the following dimensions:

Inside cross-section..............4 by 4 cm.
Thickness of wall...............not over 0.3 cm.
Winding length.................42 cm.

The primary winding on each solenoid shall consist of 150 turns of copper wire uniformly wound over the 42-cm. length. The total resistance of the magnetizing winding shall be between 0.3 and 0.5 ohm. The secondary winding of 150 turns of copper wire on each solenoid shall be similarly wound beneath the primary winding. Its resistance shall not exceed 1 ohm.

A voltmeter and the voltage coil of a wattmeter shall be connected in parallel to the terminals of the secondary winding of the apparatus. The current coil of the wattmeter shall be connected in series with the primary winding.

A sine wave electromotive force shall be applied to the primary winding and adjusted until the voltage of the secondary circuit is given by the equation:

$$\mathrm{E} = \frac{4 f N n B M}{4 l D 10^8}$$

in which

f = form factor of primary E.M.F. = 1.11 for sine wave
N = number of secondary turns = 600
n = number of cycles per second = 60
B = maximum induction = 10,000
M = total mass in grams = 10,000
l = length of strips in centimeters = 50
D = specific gravity = 7.5 for high-resistance steel
= 7.7 for low-resistance steel
E = 106.6 volts for high-resistance steel for sine voltage
= 103.8 volts for low-resistance steel for sine voltage

A specific gravity of 7.5 is assumed for all steels having a resistance of over 2 ohms per meter-gram, and 7.7 for all steels having a resistance of less than 2 ohms per meter-gram. These steels are designated as high- and low-resistance steels, respectively.

The wattmeter gives the power consumed in the iron and the secondary circuit. The loss in the secondary circuit is given in terms of the total resistance and voltage. Subtracting this correction term from the total power gives the net power consumed in the steel as hysteresis and eddy current loss. Dividing this value by ten gives the core loss in watts per kilogram.

The Procedure.—1. Cut from the test material a number of strips 3 by 50 cm., half parallel and half at right angles to the direction of rolling.

2. Place on the balance a pile of strips weighing 2.5 kg. Add a second pile of the same kind, bringing the weight up to 5 kg. In each case the weight is taken to the nearest strip. Add in succession two piles of 2.5 kg. each, of the other kind of strips, bringing the weight up to 7.5 kg. and 10 kg. respectively.

3. Secure each bundle by string or tape (not wire) and insert in the apparatus as indicated.

4. Apply the alternating voltage to the primary coil and tap the joints together until the current has a minimum value, as shown by an ammeter in series. Then clamp the corners firmly by some suitable device.

5. Shunt the ammeter and adjust the primary current until the voltmeter indicates the proper value. This adjustment may be made by an auto-transformer, by varying the field of the alternator, or by both, but not by the insertion of resistance or inductance in the primary circuit. Simultaneously the frequency must be adjusted to 60 cycles.

6. Read the wattmeter.

7. Calculations. Subtract from the wattmeter reading the instrument losses, which will be constant for any set of instruments and voltage, and divide by 10. The result is the standard core loss.

NORMAL INDUCTION.

The normal magnetic induction is the induction produced by a magnetizing force in a given piece of magnetic material which has been previously demagnetized and then subjected to many reversals of the given magnetizing force.

Both the induction B and the magnetizing force H shall be expressed in terms of the C. G. S. electromagnetic unit (gauss).

Sheet Metal.—The standard normal induction data for sheet material shall consist of the magnetizing forces corresponding to inductions of 2,000, 4,000, 6,000, 8,000, 10,000, 12,000, 14,000, 16,000, 18,000, 20,000 gausses, or such as may be obtained without exceeding a magnetizing force of 200 gausses.

The following details are to be observed:

The test material shall consist of 5 kg. of the strips cut as indicated for the standard core loss test.

The magnetic circuit shall be a rectangle having the test material for one pair of opposite sides, and the same or different material for the other pair, which may be shorter. The joints at each corner are alternately butt and lap, or may be clamped on the edges.

The magnetomotive force is applied in two sections. The main magnetizing coils shall consist of two equal and uniformly wound solenoids surrounding the test material. The compensating coils shall consist of two solenoids surrounding the yoke strips.

The test coil surrounds the middle portion of each bundle of test material. Four other test coils of half the number of turns are placed over the test material, approximately midway between the yokes and the center. The two center test coils are joined in series and the four end test coils are joined in series. The corresponding ballistic deflections, due to these two test coils, are measures of the magnetic fluxes through the underlying portions of the magnetic circuit. By connecting the two test coils so that the induced electromotive forces oppose each other, and adjusting the current through the compensating magnetizing coils so that there is no resulting ballistic deflection, an approximate uniformity of flux is secured through the greater portion of the test material, and the induction may be measured ballistically in the regular manner. The magnetizing force when the flux is adjusted to uniformity is that calculated from the uniform winding of the main magnetizing solenoids.

The cross-section of the magnetic circuit is determined as in the standard core loss test.

Rods.—The standard test for rods for use in electromagnets shall consist of the magnetizing forces corresponding to inductions of 2,000, 4,000, 6,000, 8,000, 10,000, 12,000, 14,000, 16,000, 18,000, 20,000 gausses, or such as may be obtained without exceeding a magnetizing force of 200 gausses.

The standard test for rods intended for permanent magnets shall consist in the measurement of the magnetizing force, the residual induction, and the coercive force corresponding to a maximum induction of 14,000 gausses.

Standard tests shall be made by the Burrows compensated double yoke method (described in *Standard Electrical Engineer's Handbook*, and also in *Technical Paper No.* 117 *of the Bureau of Standards*).

REPORT OF COMMITTEE A-7 ON THE TEMPERING AND TESTING OF STEEL SPRINGS AND STANDARD SPECIFICATIONS FOR SPRING STEEL.

Since the meeting last summer, the Committee has held three meetings, with good attendance. The report of the Committee is one of progress only, but some testing work has been done that is of much interest. It is the intention of the Committee that this work shall be placed before the Society, as being of an instructive character. It is also the wish of the Committee to arouse as complete discussion in open meeting as possible, in order to draw out suggestions and criticism of the work, which will assist us in our work in the future.

During the first few meetings, the deliberations of the Committee centered about the chemical specifications for spring steel. The matter was fully discussed and the following composition agreed upon:

OPEN-HEARTH, CRUCIBLE, OR ELECTRIC STEEL.

Carbon	0.90 to	1.10	per cent. (1.00 desired).
Manganese	not to exceed	0.50	" "
Phosphorus	" " "	0.05	" "
Sulphur	" " "	0.05	" "

The next subject discussed was the determination of reasonable limits for the elastic limit of heat-treated spring leaves; also, the consideration of Brinell hardness tests, bending tests, drop tests, and endurance tests. It is understood that these tests should have reference to a leaf of a spring and not to the spring as a whole.

For the purpose of carrying out these tests, a special subcommittee was appointed, as follows:

F. D. Carney, Pennsylvania Steel Company.
J. E. Howard, Bureau of Standards.
A. N. Lukens, Railway Steel Spring Company.
A. A. Stevenson, Standard Steel Works Company.
E. D. Nelson, Pennsylvania Railroad Company (*Chairman*).

The work of the Sub-Committee has been progressing rapidly, and the results to date are appended to this report in the form of a report of the Sub-Committee to Committee A-7.

The specimens were furnished and placed in the hands of Mr. Nelson by Mr. Lukens. Analyses accompany the report, and it will be seen that the steel is as uniform as may be expected. It must be understood that it was the intention to conduct the tests on steel coming within the limit of the specifications drawn and previously referred to. It will be observed that these samples are slightly above the limit of the specification as to carbon, but well within the limit in all other respects.

It is the intention of the Committee to duplicate the experiments on steel with carbon at the opposite extreme of the specifications, that is, about 0.90 per cent.

It is also the plan of the Committee to have all tests repeated at the Bureau of Standards. The Bureau has offered to undertake this work and will do so as soon as their testing laboratory is fully equipped for the purpose. The Bureau will also make a number of tension tests, with a view of developing the situation most thoroughly.

The tests of repeated stress have not yet been completed, but are under way.

The Sub-Committee will also conduct transverse tests for the purpose of comparing the results obtained with 12-in. and 24-in. spans.

The Committee wishes to make it clear that its report is one of progress only, and that the matter should be recommitted for continuance, with suggestions and criticisms as to the work already done and that which may be done in the future.

Respectfully submitted on behalf of the Committee,

HENRY SOUTHER,
Chairman.

APPENDIX.

REPORT OF SUB-COMMITTEE ON TESTS.

The Sub-Committee appointed at a meeting of Committee A-7 in New York in February, 1911, has prepared data which it was decided upon at the time would form the subject of a report to be considered by the entire Committee.

At a previous meeting it had been decided to confine the question of heat treatment of steels for springs to what is commonly known as carbon steel of the following pattern analysis:

Carbon		0.90 to 1.10	per cent. (1.00 desired).
Manganese	not to exceed	0.50	" "
Phosphorus	" " "	0.05	" "
Sulphur	" " "	0.05	" "

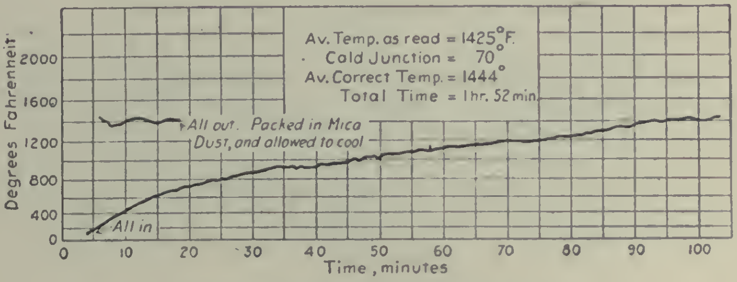


FIG. 1.—Heating Curve for Annealing.
Series A.—150–6–T; 150–10–T and D.

Furthermore, the Sub-Committee, following out the general instructions received, outlined a program, as follows:

Size of Test Specimens.—$1\frac{3}{4}$ by $\frac{3}{8}$ by 14 ins. long, with straight edges.

Sub-Division of Experiments.—(1) Heat treatment; (2) transverse tests; (3) extreme bending tests; (4) Brinell and Shore (scleroscope) tests for hardness; (5) drop tests.

Accordingly there was obtained from the Railway Steel Spring Company a sufficient quantity of steel taken from stock to make enough specimens to complete the program outlined above.

Heat Treatment.—All the material received was rolled 2 ins. wide, and when received was cut into the required lengths and the edges trued so as to make the width $1\frac{3}{4}$ ins. and the edges square. All specimens were thoroughly annealed before heat treatment to 1450° F., then packed in mica dust until cool. Fig. 1 is a sample heating curve showing regular temperatures for annealing.

The analysis made by the Pennsylvania Railroad Laboratory from five bars was as follows:

Sample	A	B	C	D	E
Carbon, per cent	1.11	1.20	1.12	1.15	1.12
Manganese, per cent	0.37	0.40	0.37	0.35	0.36
Phosphorus, per cent	0.018	0.018	0.016	0.016	0.015
Silicon, per cent	0.023	0.028	0.026	0.023	0.023
Sulphur, per cent	0.019	0.021	0.020	0.022	0.021

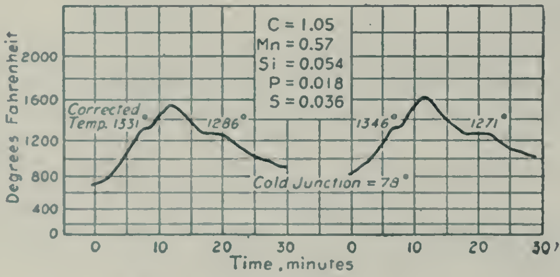


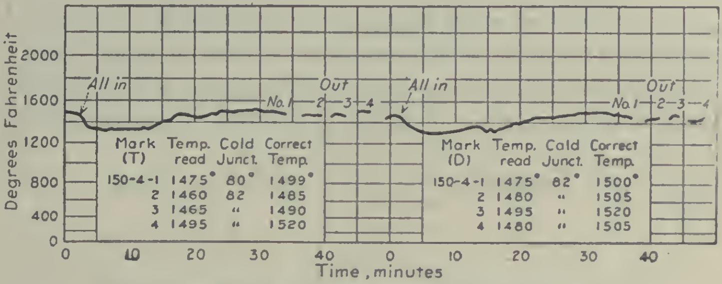
FIG. 2.—Recalescence Test.

Sample A represents test pieces 150–4 and 6.
Sample B represents test pieces 150–8 and 10.
Sample C represents test pieces 150–12, and 165–4.
Sample D represents test pieces 165–6 and 8.
Sample E represents test pieces 165–10 and 12.

The critical point for this steel was found to be about 1350° F. This was determined by means of an electric furnace in connection with a recording pyrometer of the Le Chatelier form. A hole was drilled in a small sample of the steel and the end of the thermo-couple inserted. The specimen was then slowly heated in the furnace to about 1600° F., after which it was again allowed to cool.

The heating and cooling curves are shown by the recording instrument. Fig. 2 is a sample of one of these.

There was no difficulty in keeping the furnace under control. A correction was made for the temperature of the cold junction of the thermo-couple. The time required for one determination was about 30 minutes.

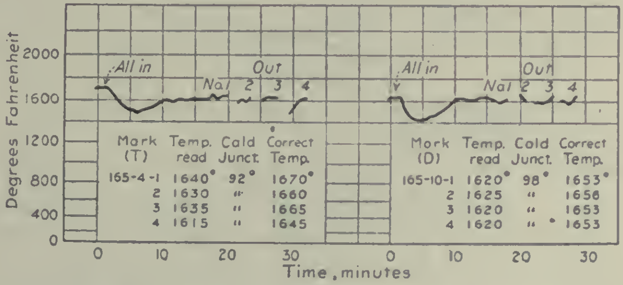


Mark (T)	Temp. read	Cold Junct.	Correct Temp.
150-4-1	1475°	80°	1499°
2	1460	82	1485
3	1465	"	1490
4	1495	"	1520

Mark (D)	Temp. read	Cold Junct.	Correct Temp.
150-4-1	1475°	82°	1500°
2	1480	"	1505
3	1495	"	1520
4	1480	"	1505

FIG. 3.—Heating Curves for Quenching, Series A.

The specimens were divided into two general groups known as Series "A" and Series "B". These two series differ in heat treatment only in respect to the quenching temperature. The temperature of the oil quenching bath was between 80° and 100° F.



Mark (T)	Temp. read	Cold Junct.	Correct Temp.
165-4-1	1640°	92°	1670°
2	1630	"	1660
3	1635	"	1665
4	1615	"	1645

Mark (D)	Temp. read	Cold Junct.	Correct Temp.
165-10-1	1620°	98°	1653°
2	1625	"	1656
3	1620	"	1653
4	1620	"	1653

FIG. 4.—Heating Curves for Quenching, Series B.

In heating the specimens for quenching they were pre-heated to about 1000° F., placed in the hardening furnace and soaked from 15 to 20 minutes.

Sample records of the quenching curves for the two series are given in Figs. 3 and 4. Fig. 5 shows a sample record for drawing temper.

Series A.—The specimens in this series were quenched at 1500° F. and drawn at temperatures approximately 400°, 600°, 800°, 1000° and 1200° F. The number of specimens used for the various tests and the method of marking are given in Tables I and III (pages 124 and 126).

Series B.—The specimens in this series were quenched in oil at 1650° F.; otherwise the same heat treatment as for Series A. The number of specimens (same as for Series A) and the corresponding marking are given in Tables II and IV (pages 125 and 127).

The total number of specimens needed for Series A and B was 80.

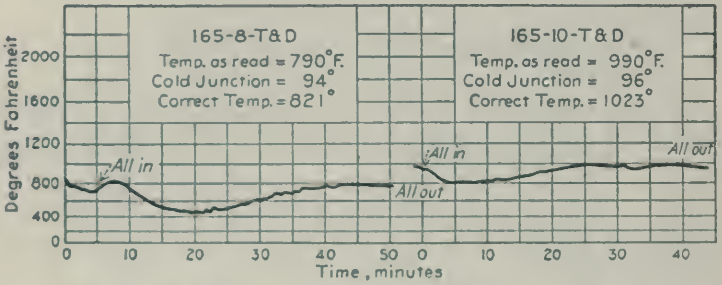


Fig. 5.—Heating Curves for Drawing the Temper, Series B.

Transverse Tests.—Specimens were tested transversely with 12-in. span in an apparatus specially designed for this purpose, which is shown in Fig. 6. The test piece was placed on the two steel supports *A* held by the forged steel base *B*. The load was applied in the center by means of another tool-steel block *C*. The top edge of each tool-steel block was rounded off enough to give a bearing, but notwithstanding this fact, it was found that the blocks would cut into the test pieces 0.001 or 0.002 in. In order to remove as far as possible any error in the measurement of the deflection due to this cause, or to a clearance in the testing machine, the specimens were subjected before the test was begun to three applications of a load which stressed them to about two-thirds of the elastic limit. These applications served to bring the specimen in close contact with the supporting points, and also to relieve any initial strain in the steel due to the hardening process.

The deflections were measured by means of the pointed steel pin *D*, which was pressed against the underside of the specimen by means of a light spring. Attached to this steel pin, through a slot in the base *B*, was a vernier *E* resting against the scale *F*. By means of this scale and vernier the deflections could be measured to 0.0005 in., although in these experiments no deflections smaller than 0.001 in. were read. The loads were increased by increments of 50 lbs. until a decisive permanent set had been obtained.

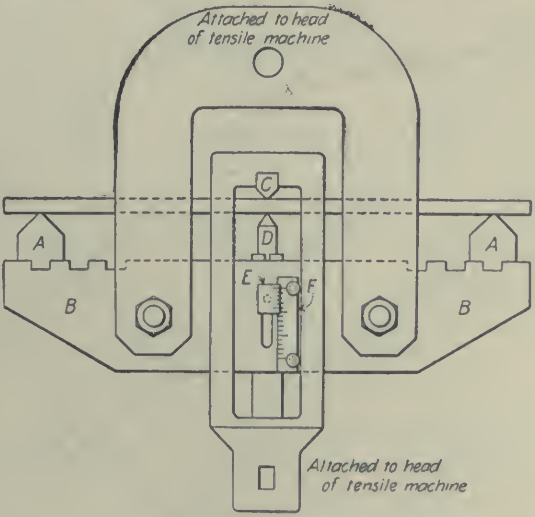

Attached to head of tensile machine
C
A
D
A
E
F
B
B
Attached to head of tensile machine

FIG. 6.—Apparatus for Transverse Tests.

Elastic Limit.—The elastic limit was determined by plotting the results of the transverse tests, and fixing upon a point where the ratio of deflection to stress ceases to be appreciably constant. In other words, after plotting, the point was determined at which the deflection curve began to depart from a straight line.

Figs. 7 and 8 show the plotted results and the point at which the limit of elasticity was selected.

The fiber stresses, elastic limit, modulus of elasticity, and resilience in inch pounds were calculated from the usual formulas for a simple beam supported at both ends and loaded at the center.

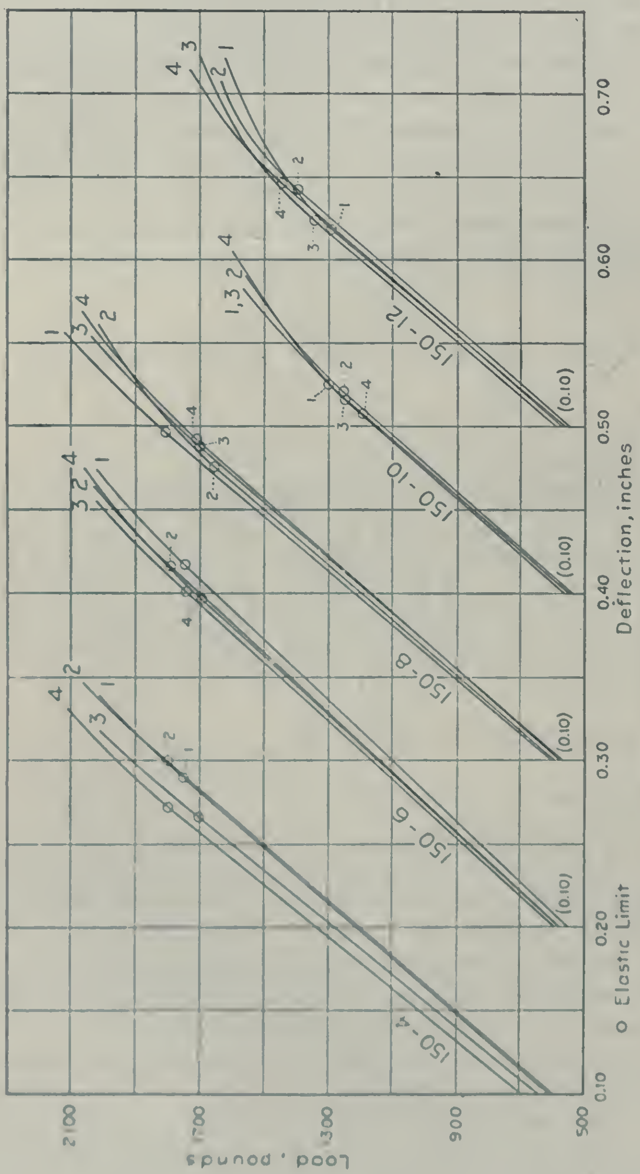


FIG. 7.—Transverse Tests, Series A.

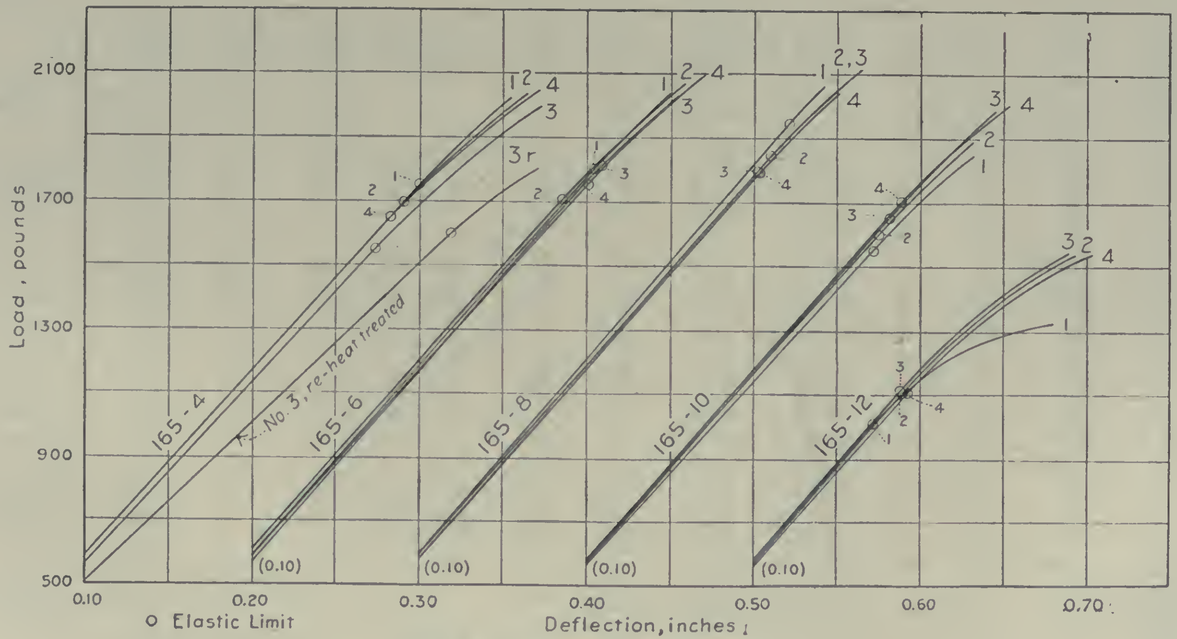


FIG. 8.—Transverse Tests, Series B.

TABLE I.—TRANSVERSE, HARDNESS AND BENDING TESTS OF SPRING STEEL, SERIES A.

Span, 12 ins.

Mark. T	Heat Treatment. (Temperature, deg. Fahr.)			Cross Section, ins.	Properties at Elastic Limit.				Hardness Number.			Bending Test, angle bent through at rupture, degrees
	Quenched at	Temperature of Oil.	Temper Drawn to		Load, lbs.	Deflection, ins.	Maximum Stress, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.	Scleroscope.		Brinell.*	
									On Flat.	On Edge.	On Flat.	
150-4-1	1,499	62	425	1.747 x 0.368	1,750	0.293	133,140	29,635,000	49	47	377	88
2	1,485	65	425	1.752 x 0.368	1,800	0.303	136,630	29,408,000	48	49	373	93
3	1,490	69	425	1.876 x 0.369	1,700	0.268	119,740	29,072,000	50	46	360	78
4	1,530	72	425	1.873 x 0.369	1,800	0.275	127,040	30,046,000	47	47	372	66
Average...					1,762	0.285	129,137	29,540,000	48.5	47	370.5	81.2
150-6-1	1,525	72	607	1.750 x 0.363	1,750	0.321	136,600	28,135,000	46	49	400	74
2	1,505	75	607	1.751 x 0.364	1,800	0.321	139,650	28,685,000	45	51	381	28
3	1,485	80	607	1.752 x 0.364	1,750	0.305	135,700	29,335,000	46	51	375	40
4	1,485	80	607	1.745 x 0.362	1,700	0.300	133,810	29,572,000	47	51	395	97
Average...					1,750	0.312	136,440	28,931,700	46	50.5	388	59.7
150-8-1	1,506	74	835	1.749 x 0.368	1,800	0.300	136,790	29,737,000	48	56	374	73
2	1,501	78	835	1.747 x 0.367	1,650	0.279	126,220	29,584,000	36	42	318	138
3	1,497	85	835	1.750 x 0.366	1,700	0.292	130,530	29,314,000	46	49	369	60
4	1,497	90	835	1.750 x 0.366	1,700	0.294	130,530	29,114,000	44	51	344	73
Average...					1,712	0.291	131,017	29,437,200	43.5	49.5	351	86
150-10-1	1,519	74	1,022	1.749 x 0.364	1,300	0.228	100,980	29,201,000	36	41	267	160
2	1,514	78	1,022	1.753 x 0.363	1,250	0.221	97,400	29,140,000	36	39	269	117
3	1,509	82	1,022	1.752 x 0.366	1,250	0.218	95,870	28,837,000	34	38	266	158
4	1,509	87	1,022	1.750 x 0.364	1,200	0.209	93,160	29,388,000	32	38	270	171
Average...					1,250	0.219	96,852	29,141,500	34.5	39	268	151.5
150-12-1	1,516	88	1,230	1.748 x 0.370	1,300	0.220	100,060	29,503,000	33	35	253	155
2	1,506	93	1,230	1.748 x 0.366	1,400	0.244	107,620	28,923,000	35	38	305	170
3	1,521	99	1,230	1.751 x 0.370	1,350	0.255	103,730	29,905,000	33	40	286	171
4	1,526	106	1,230	1.749 x 0.368	1,450	0.247	110,190	29,095,000	34	36	283	172
Average...					1,375	0.234	105,400	29,356,500	34	37	282	167

* Brinell Test.—Load, 3,000 kg.; Ball, 10 mm. diameter.

TABLE II.—TRANSVERSE, HARDNESS AND BENDING TESTS OF SPRING STEEL, SERIES B.

Span, 12 ins.

Mark. T	Heat Treatment. (Temperature, deg. Fahr.)			Cross Section, ins.	Properties at Elastic Limit.				Hardness Number.			Bending Test, angle bent through at rupture, degrees.
	Quenched at	Temperature of Oil.	Temper Drawn to		Load, lbs.	Deflection, ins.	Maximum Stress, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.	Scleroscope.		Brinell.*	
									On Flat.	On Edge.	On Flat.	
165-4-1	1,670	88	450	1.753 x 0.366	1,750	0.303	134,140	29,031,000	51	58	424	79
2	1,660	92	450	1.753 x 0.366	1,700	0.294	130,310	29,064,000	45	57	392	77
3	1,665	96	450	1.746 x 0.351	1,600	0.322	133,880	28,429,000	44	44	372	135
4	1,645	100	450	1.759 x 0.367	1,650	0.285	125,360	28,763,000	43	51	387	69
Average...	....	....	....	....	1,675	0.301	130,922	28,821,750	46	52.5	394	90
165-6-1	1,650	84	627	1.753 x 0.368	1,800	0.307	136,480	28,992,000	43	59	369	63
2	1,655	87	627	1.750 x 0.368	1,700	0.287	129,120	29,340,000	42	58	360	69
3	1,655	92	627	1.753 x 0.366	1,800	0.313	137,970	28,906,000	44	56	372	123
4	1,655	97	627	1.754 x 0.367	1,750	0.302	133,360	28,878,000	44	55	383	71
Average...	....	....	....	....	1,762	0.302	134,232	29,029,000	43	57	371	81.5
165-8-1	1,646	87	821	1.764 x 0.367	1,950	0.327	147,740	29,497,000	48	57	381	81
2	1,646	92	821	1.756 x 0.366	1,850	0.316	141,640	29,393,000	48	54	395	76
3	1,641	95	821	1.753 x 0.368	1,800	0.308	136,790	28,965,000	44	54	379	124
4	1,641	99	821	1.757 x 0.365	1,800	0.309	138,420	29,454,000	43	58	400	134
Average...	....	....	....	....	1,850	0.315	141,147	29,304,750	46	56	389	103.7
165-10-1	1,652	90	1,023	1.745 x 0.362	1,550	0.275	122,010	29,414,000	41	51	358	135
2	1,647	94	1,023	1.755 x 0.363	1,600	0.277	124,530	29,649,000	42	48	367	94
3	1,647	97	1,023	1.757 x 0.363	1,650	0.283	128,280	29,969,000	42	50	381	90
4	1,642	102	1,023	1.751 x 0.366	1,700	0.293	130,460	29,197,000	44	51	376	113
Average...	....	....	....	....	1,625	0.282	126,320	29,557,250	42	50	370.5	108
165-12-1	1,651	66	1,212	1.748 x 0.366	1,000	0.174	76,870	28,970,000	32	37	251	180
2	1,641	68	1,212	1.750 x 0.364	1,100	0.193	85,410	29,177,000	31	36	256	180
3	1,656	73	1,212	1.749 x 0.364	1,100	0.190	85,440	29,651,000	30	36	257	180
4	1,636	78	1,212	1.745 x 0.363	1,100	0.195	86,110	29,120,000	32	37	275	180
Average...	....	....	....	....	1,075	0.188	83,457	29,229,500	31	36.5	260	180

* Brinell Test.—Load 3,000 kg.; Ball, 10 mm. diameter.

TABLE III.—DROP TESTS OF SPRING STEEL, SERIES A.

Span, 12 ins. Weight, 50 lbs. Height of blow, 3 ft. Number of blows, 100.

Mark. D	Heat Treatment. (Temperature, deg. Fahr.)			Cross Section, ins.	Maximum Deflection at Elastic Limit from Transverse Tests, ins.	Total Permanent Set, ins.							
	Quenched at	Temper-ature of Oil.	Temper Drawn to			With 0.20 in. Deflection.		With 0.25 in. Deflection.		With 0.30 in. Deflection.		With 0.35 in. Deflection.	
150-4-1	1,500	70	425	1.765 x 0.368	0.293	0.01	0.01	0.02	0.03	0.04	0.04	0.07	0.08
2	1,505	74	425	1.872 x 0.369	0.303	0.00	0.00	0.01	0.01	0.03	0.04	0.06	0.06
3	1,520	80	425	1.745 x 0.368	0.268	0.00	0.01	0.02	0.02	0.04	0.05	0.05	0.06
4	1,505	86	425	1.873 x 0.369	0.275	0.01	0.01	0.03	0.03	0.05	0.06	0.08	0.09
Average...	....	....	....	....	0.285	0.005	0.008	0.02	0.023	0.04	0.048	0.065	0.073
150-6-1	1,517	77	607	1.864 x 0.366	0.321	0.00	0.01	0.02	0.03	0.04	0.04	0.04	0.04
2	1,502	72	607	1.871 x 0.370	0.321	0.01	0.02	0.03	0.04	0.04	0.04	0.05	0.06
3	1,507	78	607	1.878 x 0.369	0.305	0.01	0.01	0.01	0.02	0.06	0.06	0.07	0.07
4	1,502	70	607	1.877 x 0.371	0.300	0.01	0.01	0.01	0.01	0.05	0.05	0.07	0.08
Average....	....	....	....	....	0.312	0.008	0.013	0.018	0.025	0.048	0.048	0.058	0.063
150-8-1	1,515	80	835	1.874 x 0.367	0.300	0.00	0.00	0.00	0.00	0.00	0.01	0.02	0.02
2	1,505	85	835	1.872 x 0.367	0.279	0.00	0.00	0.01	0.01	0.02	0.02	0.03	0.04
3	1,515	90	835	1.875 x 0.368	0.292	0.02	0.02	0.02	0.03	0.06	0.06	0.07	0.07
4	1,520	97	835	1.879 x 0.366	0.294	0.01	0.01	0.01	0.01	0.04	0.04	0.05	0.05
Average....	....	....	....	....	0.291	0.008	0.008	0.01	0.013	0.03	0.033	0.043	0.045
150-10-1	1,529	82	1,022	1.873 x 0.368	0.228	0.00	0.01	0.03	0.05	0.05	0.05	0.09	0.09
2	1,519	84	1,022	1.875 x 0.368	0.221	0.01	0.02	0.03	0.06	0.07	0.07	0.10	0.10
3	1,524	89	1,022	1.873 x 0.368	0.218	0.01	0.01	0.03	0.03	0.09	0.11	0.13	0.13
4	1,519	96	1,022	1.879 x 0.366	0.209	0.01	0.01	0.02	0.03	0.09	0.10	0.13	0.13
Average....	....	....	....	....	0.219	0.008	0.013	0.028	0.043	0.075	0.083	0.113	0.113
150-12-1	1,513	94	1,230	1.874 x 0.370	0.220	0.01	0.03	0.10	0.11	0.13	0.13	0.16	0.16
2	1,518	99	1,230	1.859 x 0.368	0.244	0.01	0.04	0.10	0.11	0.12	0.12	0.15	0.16
3	1,518	105	1,230	1.871 x 0.367	0.225	0.00	0.01	0.02	0.05	0.09	0.10	0.12	0.13
4	1,523	109	1,230	1.877 x 0.367	0.247	0.01	0.01	0.02	0.05	0.09	0.10	0.12	0.13
Average....	....	....	....	....	0.234	0.008	0.023	0.06	0.08	0.108	0.113	0.138	0.145

TABLE IV.—DROP TESTS OF SPRING STEEL, SERIES B.

Span, 12 ins. Weight, 50 lbs. Height of Blow, 3 ft. Number of Blows, 100.

Mark. D	Heat Treatment. (Temperature, deg. Fahr.)			Cross Section, ins.	Maximum Deflection at Elastic Limit from Transverse Tests, ins.	Total Permanent Set, ins.							
	Quenched at	Temperature of Oil.	Temper Drawn to			With 0.20 in. Deflection.		With 0.25 in. Deflection.		With 0.30 in. Deflection.		With 0.35 in. Deflection.	
165–4–1	1,652	75	450	1.875 x 0.367	0.303	0.01	0.01	0.01	0.01	0.01	0.01	0.02	0.02
2	1,657	79	450	1.875 x 0.367	0.294	0.01	0.02	0.02	0.02	0.03	0.03	0.03	0.03
3	1,647	84	450	1.876 x 0.370	0.322	0.00	0.01	0.01	0.01	0.01	0.01	0.01	0.03
4	1,667	90	450	1.877 x 0.368	0.285	0.00	0.00	0.00	0.02	0.02	0.02	0.02	0.04
Average....	....	....	....	....	0.301	0.005	0.01	0.01	0.015	0.018	0.018	0.02	0.03
165–6–1	1,659	75	627	1.873 x 0.376	0.307	0.00	0.00	0.00	0.01	0.01	0.01	0.02	0.04
2	1,649	79	627	1.873 x 0.368	0.287	0.00	0.00	0.00	0.01	0.01	0.01	0.03	0.03
3	1,649	84	627	1.878 x 0.367	0.313	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.04
4	1,659	90	627	1.870 x 0.367	0.302	0.01	0.01	0.01	0.02	0.02	0.02	0.03	0.05
Average....	....	....	....	....	0.302	0.005	0.005	0.005	0.015	0.015	0.015	0.028	0.04
165–8–1	1,651	90	821	1.874 x 0.369	0.327	0.00	0.00	0.00	0.01	0.01	0.01	0.01	0.01
2	1,646	95	821	1.879 x 0.367	0.316	0.01	0.01	0.01	0.01	0.01	0.01	0.03	0.04
3	1,656	99	821	1.878 x 0.367	0.308	0.00	0.00	0.00	0.01	0.01	0.01	0.02	0.03
4	1,661	106	821	1.880 x 0.367	0.309	0.01	0.01	0.01	0.02	0.02	0.03	0.04	0.04
Average....	....	....	....	....	0.315	0.005	0.005	0.005	0.013	0.013	0.015	0.025	0.03
165–10–1	1,653	96	1,023	1.873 x 0.367	0.275	0.00	0.00	0.00	0.01	0.01	0.02	0.04	0.04
2	1,658	102	1,023	1.875 x 0.368	0.277	0.00	0.00	0.01	0.01	0.02	0.03	0.05	0.05
3	1,653	107	1,023	1.870 x 0.368	0.283	0.01	0.01	0.02	0.02	0.03	0.03	0.06	0.07
4	1,653	111	1,023	1.875 x 0.366	0.293	0.01	0.01	0.02	0.02	0.03	0.03	0.05	0.06
Average....	....	....	....	....	0.282	0.005	0.005	0.013	0.015	0.023	0.028	0.05	0.055
165–12–1	1,653	88	1,212	1.868 x 0.367	0.174	0.02	0.03	0.04	0.05	0.07	0.07	0.10	0.10
2	1,663	93	1,212	1.867 x 0.365	0.193	0.02	0.02	0.03	0.03	0.05	0.06	0.09	0.09
3	1,653	97	1,212	1.871 x 0.366	0.190	0.03	0.03	0.04	0.05	0.07	0.07	0.11	0.11
4	1,653	104	1,212	1.874 x 0.367	0.195	0.02	0.02	0.02	0.03	0.05	0.05	0.08	0.10
Average....	....	....	....	....	0.188	0.023	0.025	0.033	0.04	0.06	0.063	0.095	0.10

Brinell Tests.—For these tests the specimens already used for transverse tests were used, but before the extreme bending test was made. The load used was 3,000 kg. (6,600 lbs.); the diameter of ball was 10 mm. The impressions were made on one flat side, 2 ins. apart along the entire length of the specimens. The tests were made in a dead-weight testing machine.

Fig. 10 shows the Brinell hardness numbers as determined by these experiments, plotted against the elastic limits.

Extreme Bending Tests.—After a specimen had been bent far enough in the transverse testing machine to obtain the deflection at the elastic limit,—that is, until a marked permanent set had taken place,—it was further bent in a vise or similar machine around a 1-in. radius, until rupture occurred. Special note was made of the structure of the fracture.

The results of the transverse, hardness and extreme bending tests are given in Tables I and II.

Drop Tests.—The same number of drop tests were made as transverse tests. The apparatus used for these tests is shown in Fig. 9.

The method of testing was as follows: A specimen was placed in the machine and supported at both ends by the two anvils, *A*, the edges of which are 12 ins. apart. The two triggers, *B*, were then released by means of a special pair of tongs, allowing the plunger *C* to drop down on the specimen. The 50-lb. weight *D* was then raised 3 ft. and allowed to drop. On rebounding the plunger was caught by the trigger, thus giving the specimen only one blow at a time. The length of the stroke of the plunger is fixed by the interchangeable collar *E*, which allows any desired deflection to be used in bending the specimen.

The results of these tests are shown in Tables III and IV.

Summary.—A complete analysis of all the tests is shown on the following tables:

Table I covers the transverse tests, hardness tests, and bending tests of Series A; Table II gives the same information for Series B. Table III gives the results of the drop test for Series A; Table IV gives the same information for Series B.

Attention is called to the elastic limits as shown in Tables I and II. It is apparent that at a quenching temperature of 1500° F. (Table I), the maximum results are obtained with a drawing temperature of about 600° F., while where the quenching temperature was 1650° F. (Table II), the maximum elastic limit was found when the drawing temperature was about 800° F. In the former group—quenched at 1500° F. and drawn at 600° F.—it will be noticed that the angle of bend at rupture showed an average of slightly over 59°, there being considerable variation in the specimens; while with the latter group—quenched at 1650° F.

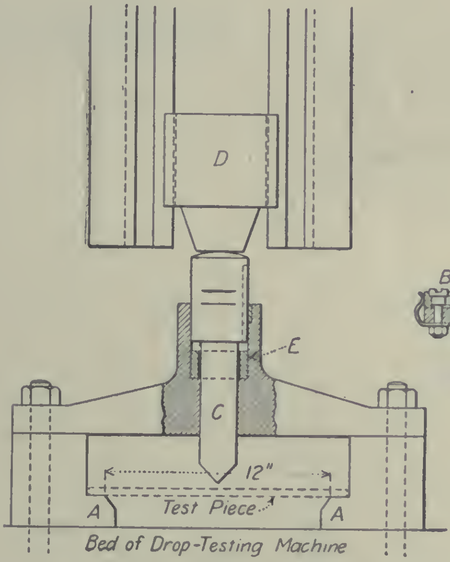


FIG. 9.—Apparatus Used for Drop Tests.

and drawn at 800° F.—the average angle is slightly over 103°, without any specimen going below 76°.

It had been hoped that there would be some definite relation between either the Scleroscope or Brinell hardness numbers and the elastic limits. Figs. 10 and 11 show graphically the results obtained. While it cannot be said that a definite law has been established showing the relation between hardness and elastic limit, as determined by the methods employed, there seems to be sufficient indication of the possibility of such a law, to warrant further investigation.

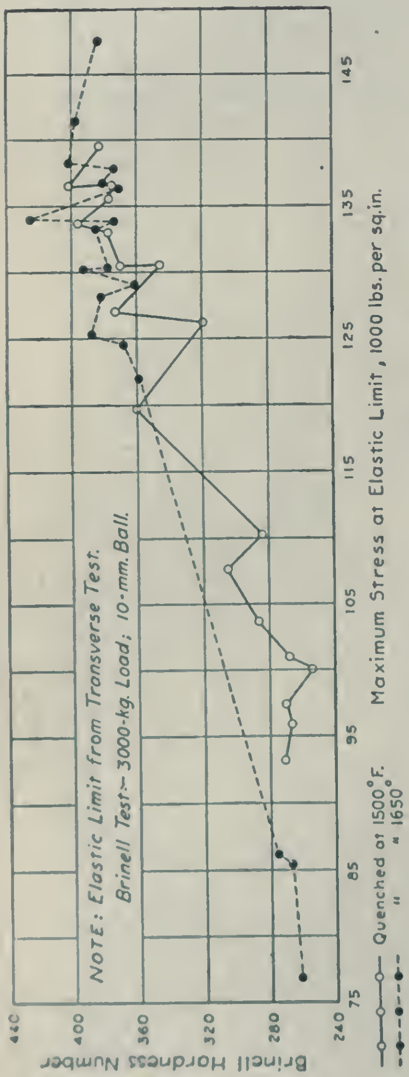


FIG. 10.—Comparison of Stress at Elastic Limit and Brinell Hardness Number.

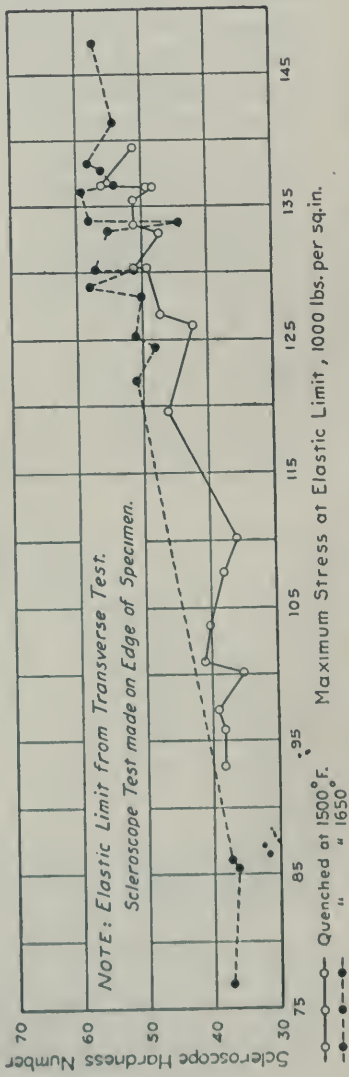


FIG. 11.—Comparison of Stress at Elastic Limit and Scleroscope Hardness Number.

It is thought by the Sub-Committee that if a relation can be found between the hardness number and elastic limit, or some other physical characteristic of heat-treated steel, it will be a long step towards placing the consumer in position to test, in a simple way, the product offered by the manufacturer before acceptance, and further, towards giving to the manufacturer a means of ascertaining in a simple way whether he has obtained the results desired.

Respectfully submitted,

F. D. Carney,
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E. D. Nelson, *Chairman.*

REPORT OF COMMITTEE B-1 ON STANDARD SPECIFICATIONS FOR HARD-DRAWN COPPER WIRE.

Last year a verbal report for Committee B-1 was made briefly to the effect that although experience, gained while operating under the specifications adopted in 1909, indicated the desirability of amending certain requirements, the Committee was not yet prepared to recommend the desired changes, but instead found it necessary, in effect, to resolve itself into a Research Committee to obtain the data upon which to base amendments.

The principal change found necessary is a revision of the table of required tensile strengths and corresponding percentages of elongation. The data available as a basis of the table values were the accumulation of results of commercial testing of wire offered to consumers by producers. In substantially all of these tests, the elongation reported was determined by measuring the distance between the jaws of the testing machine before and after breaking the wire, the zero measurements being made when 10 per cent. of the required breaking load had been applied and the initial length being as close to 60 ins. as possible. This elongation includes the actual permanent stretch of the wire, its live elongation at the breaking load, any lost motion in the machine, and slippage and elongation in the grips of the machine. At the earlier meetings of the Committee, the question of the method of measuring elongation was discussed at great length and when proposing specifications at the annual meeting in 1909, the Committee agreed that they would recommend that the elongation specified be the actual permanent elongation; the same as in nearly all other similar sorts of testing. However, when it is considered that it is not practical to measure the elongation of a broken sample to smaller fractions than 0.01 in., and that the actual elongation in 10 ins. after breaking a wire of 0.081 in. diameter is only 0.10 in., it is seen that an error of only 0.01 in. amounts to 10 per cent. In testing wire smaller than 0.081 in., the probable error is increased because of the less elongation, while in testing larger wire, the probable error decreases until at 0.204 in. it drops to about 6 per cent. It was agreed that a test method in which unavoidable errors would probably be at least as great as the figures mentioned, is undesirable because of the difficulty of settling inevitable disputes. The commercial testing of wire had generally been done on a 60-in. length. The elongation of a wire 60 ins. long is considerably less than six times that of a 10-in. length; hence the quantity actually to be measured, namely, the difference between original length and length after fracture, while greater than in the case of a 10-in. specimen, is still relatively small. The actual measurement of this difference on a 60-in. length is a difficult matter, partly because of the long length itself and partly because of the fact that hard wire is never straight, but is in a springy coil. For both these reasons, special devices would have to be designed and two men would probably be required to make the measurement. There are obvious objections to a test method in which there may be unknown variables, such as slippage or creeping of elongation into the jaws of the machine, yet it must be remembered that practically all experience in testing hard wire is based on such a test method. The Committee has felt that if satisfactory tests could be made under practical shop conditions, it would be preferable to retain in the specifications methods which are more nearly theoretically correct than those which are open to obvious criticism. To provide data upon which a final decision might be made, a series of tests was arranged in which there were to be determined:

1. Elongation in 10 ins. between bench marks.
2. Elongation in 60 ins. between bench marks.
3. Elongation in 60 ins. between jaws of the testing machine.

The three measurements were to be made on the same sample of wire. At the very outset, it was found impracticable to make measurements on a 10-in. length that were in any wise concordant, because of the obviously great opportunity for errors in reading. The tests were, therefore, continued only with measurements on 60-in. lengths. The tests were made in duplicate by the producers and by one of the consumers and some 1,500 samples of three different diameters were tested. There has, therefore, been opportunity to compare the results of this large number of tests made in six different laboratories. This comparison shows clearly that the measurement of elongation between bench marks is certainly not practicable in the case of wire of small diameter, because: first, it materially increases cost of testing by reason of the longer time required to make the measurements; and second, there are greater discrepancies between results obtained on similar samples tested in the same and different laboratories by this method than are found in results obtained by the theoretically less accurate method of measurement between the jaws of the testing machine. In other words, because of inherent difficulties in making the measurements with reasonable precision, the theoretically more nearly correct method is actually found to be the less accurate when both methods are carried out on a practical scale under practical commercial conditions. The evidence obtained is so strong that the Committee feels constrained to recommend the continuance of the now long established commercial method of testing the finer sizes of wire by measuring elongation between the jaws of the testing machine. The Committee recommends that the coarser sizes of wire be tested by measuring elongation as the permanent increase in length after breakage, measurement being made on an original length of 10 ins.

Certain other amendments to the specifications are proposed for the sake of broadening their scope to include figure-eight trolley wire, and certain additions are also made to the explanatory notes accompanying the specifications.

Specifically, your Committee unanimously recommends the following amendments to the Standard Specifications for Hard-Drawn Copper Wire, adopted August 16, 1909:

PROPOSED AMENDMENTS TO THE STANDARD SPECIFICATIONS FOR HARD-DRAWN COPPER WIRE, ADOPTED AUGUST 16, 1909.

Amendment 1.—In Paragraph 2, insert the words "figure-eight trolley wire" after the words "grooved trolley wire."

Amendment 2.—Present Paragraph 3 to become 3(*a*), and insert Paragraph 3(*b*) as follows:

3(*b*). Necessary brazes in hard-drawn wire must be made in accordance with best commercial practice, and tests upon a section of wire containing a braze must show at least 95 per cent. of the tensile strength of the unbrazed wire. Elongation tests are not to be made upon test sections including brazes.

Amendment 3.—For present Paragraph 8 substitute the following:

8. Wire shall be so drawn that its tensile strength and elongation shall be at least equal to the value stated in Table I. Tensile tests shall be made upon fair samples, and the elongation of wire larger in diameter than 0.204 in. shall be determined as the permanent increase in length, due to the breaking of the wire in tension, measured between bench marks placed upon the wire originally 10 ins. apart. The elongation of wire 0.204 in. in diam-

TABLE I.

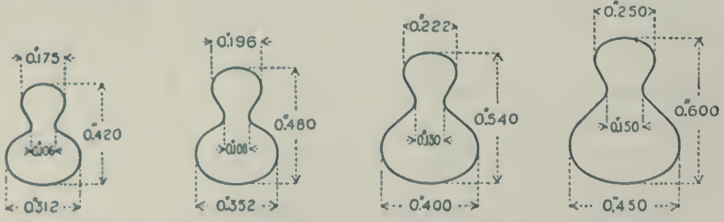
Diameter, ins.	Area, circular mils.	Tensile Strength, lbs. per sq. in.	Elongation in 10 ins., per cent.
0.460	211,600	49,000	3.75
0.410	168,100	51,000	3.25
0.365	133,225	52,800	2.80
0.325	105,625	54,500	2.40
0.289	83,520	56,100	2.17
0.258	66,565	57,600	1.98
0.229	52,440	59,000	1.79
			in 60 ins.
0.204	41,615	60,100	1.24
0.182	33,125	61,200	1.18
0.165	27,225	62,000	1.14
0.162	26,245	62,100	1.14
0.144	20,735	63,000	1.09
0.134	17,956	63,400	1.07
0.128	16,385	63,700	1.06
0.114	12,995	64,300	1.02
0.104	10,815	64,800	1.00
0.102	10,404	64,900	1.00
0.092	8,464	65,400	0.97
0.091	8,281	65,400	0.97
0.081	6,561	65,700	0.95
0.080	6,400	65,700	0.94
0.072	5,184	65,900	0.92
0.065	4,225	66,200	0.91
0.064	4,096	66,200	0.90
0.057	3,249	66,400	0.89
0.051	2,601	66,600	0.87
0.045	2,025	66,800	0.86
0.040	1,600	67,000	0.85

eter and smaller shall be determined by measurements made between the jaws of the testing machine. The zero length shall be the distance between the jaws when a load equal to ten per cent. of the required ultimate breaking strength shall have been applied, and the final length shall be the distance between the jaws at the time of rupture. The zero length shall be as near 60 ins. as possible. The fracture shall be between the bench marks in the case of wire larger than 0.204 in. in diameter and between the jaws in the case of smaller wire, and not closer than 1 in. to either bench mark or jaw. If, upon testing a sample from any coil of wire, the results are found to be below the values stated in the table, tests upon two additional samples shall be made, and the average of the three tests shall determine acceptance or rejection of the coil. For wire whose nominal diameter is between listed sizes, the requirements shall be those of the next larger size included in the table.

Amendment 4.—Insert after Paragraph 13 the following heading and Paragraphs 14 and 15:

FIGURE-EIGHT TROLLEY WIRE.

Sections. 14. Standard sections of figure-eight trolley wire shall be as shown in Fig. 2.



'5,600 C.M. 133,200 C.M. 168,100 C.M. 211,600 C.M.

FIG. 2.

Requirements. 15. The requirements for weight, physical properties, and electric conductivity of figure-eight trolley wire shall be the same as for the same sizes of grooved trolley wire.

Amendment 5.—Re-number Paragraphs 14, 15, 16, 17, 18, so as to become respectively 16, 17, 18, 19, 20.

In conclusion, the Committee offers the following addition to the explanatory notes accompanying the specifications:

8. *Wrap and Twist Tests.*—Since the adoption of the Stand-

Table II.—Conductivity and Resistivity of Copper.

Temperature 20° C. Density taken as 8.90.

Conductivity, per cent.	Ohms per meter-gram.	Lbs. per mile-ohm.	Microhms per cc.	Microhms per cu. in.	Ohms per mil-foot.
102.0	0.15002	856.62	1.6856	0.66363	10.140
101.9	0.15017	857.46	1.6873	0.66428	10.149
101.8	0.15032	858.30	1.6889	0.66494	10.159
101.7	0.15046	859.14	1.6906	0.66559	10.169
101.6	0.15061	859.99	1.6923	0.66625	10.179
101.5	0.15076	860.84	1.6939	0.66690	10.189
101.4	0.15091	861.68	1.6956	0.66756	10.200
101.3	0.15106	862.54	1.6973	0.66822	10.210
101.2	0.15121	863.39	1.6990	0.66888	10.220
101.1	0.15136	864.24	1.7006	0.66954	10.230
101.0	0.15151	865.10	1.7023	0.67020	10.240
100.9	0.15166	865.95	1.7040	0.67087	10.250
100.8	0.15181	866.81	1.7057	0.67153	10.260
100.7	0.15196	867.67	1.7074	0.67220	10.270
100.6	0.15211	868.54	1.7091	0.67287	10.281
100.5	0.15226	869.40	1.7108	0.67354	10.291
100.4	0.15241	870.27	1.7125	0.67421	10.301
100.3	0.15256	871.13	1.7142	0.67488	10.311
100.2	0.15272	872.00	1.7159	0.67555	10.322
100.1	0.15287	872.88	1.7176	0.67623	10.332
100.0	0.15302	873.75	1.7193	0.67691	10.342
99.9	0.15317	874.62	1.7211	0.67758	10.353
99.8	0.15333	875.50	1.7228	0.67826	10.363
99.7	0.15348	876.38	1.7245	0.67894	10.373
99.6	0.15364	877.26	1.7262	0.67962	10.384
99.5	0.15379	878.14	1.7280	0.68031	10.394
99.4	0.15394	879.02	1.7297	0.68099	10.405
99.3	0.15410	879.91	1.7315	0.68168	10.415
99.2	0.15426	880.79	1.7332	0.68236	10.426
99.1	0.15441	881.68	1.7350	0.68305	10.436
99.0	0.15457	882.57	1.7367	0.68374	10.447
98.9	0.15472	883.47	1.7385	0.68443	10.457
98.8	0.15488	884.36	1.7402	0.68513	10.468
98.7	0.15504	885.26	1.7420	0.68582	10.479
98.6	0.15519	886.15	1.7438	0.68652	10.489
98.5	0.15535	887.05	1.7455	0.68721	10.500
98.4	0.15551	887.96	1.7473	0.68791	10.510
98.3	0.15567	888.86	1.7491	0.68861	10.521
98.2	0.15583	889.76	1.7509	0.68931	10.532
98.1	0.15598	890.67	1.7526	0.69002	10.543
98.0	0.15614	891.58	1.7544	0.69072	10.553
97.9	0.15630	892.49	1.7562	0.69142	10.564
97.8	0.15646	893.40	1.7580	0.69213	10.575
97.7	0.15662	894.32	1.7598	0.69284	10.586
97.6	0.15678	895.23	1.7616	0.69355	10.597
97.5	0.15694	896.15	1.7634	0.69426	10.607
97.4	0.15711	897.07	1.7652	0.69497	10.618
97.3	0.15727	897.99	1.7671	0.69569	10.629
97.2	0.15743	898.92	1.7689	0.69640	10.640
97.1	0.15759	899.84	1.7707	0.69712	10.651
97.0	0.15775	900.77	1.7725	0.69784	10.662
96.9	0.15792	901.70	1.7743	0.69856	10.673
96.8	0.15808	902.63	1.7762	0.69928	10.684
96.7	0.15824	903.57	1.7780	0.70001	10.695
96.6	0.15841	904.50	1.7799	0.70073	10.706
96.5	0.15857	905.44	1.7817	0.70146	10.717
96.4	0.15874	906.38	1.7835	0.70218	10.729
96.3	0.15890	907.32	1.7854	0.70291	10.740
96.2	0.15907	908.26	1.7873	0.70364	10.751
96.1	0.15923	909.21	1.7891	0.70438	10.762
96.0	0.15940	910.15	1.7910	0.70511	10.773

ard Specifications for Hard-Drawn Copper Wire, proposed in 1909, the Committee has very carefully considered the matter of twist and wrap tests and it is their final opinion that while there might be some possible reason for requiring that wire shall stand wrapping around a wire of equal diameter, there can be no good reason for including in specifications the requirement that it shall stand unwrapping and re-wrapping, because such a test is indefinite and cannot be made otherwise. It is almost physically impossible to unwrap and re-wrap hard-drawn wire about a wire of its own diameter. With respect to twist tests, the Committee has nothing to add to the statement already on record, condemning this character of test.

8. *Elastic Limit.*—Tests carefully made by members of the Committee show that the elastic limit of hard-drawn copper wire from sizes 0.460 to 0.325 in., inclusive, averages 55 per cent. of the ultimate tensile strength required in these specifications, with a minimum value of 50 per cent.; for sizes 0.324 to 0.040 in., inclusive, it averages 60 per cent. of the ultimate tensile strength required in these specifications, with a minimum value of 55 per cent. This statement of experience is based on the definition of elastic limit as "that point on the elastic curve beyond which the ratio of stress to strain ceases to be constant."

9. *Conductivity.*—For the convenience of those who are accustomed to expressing resistivity or conductivity in any one of several more or less common units, Table II has been prepared giving the conductivity and resistivity of copper at 20° C., expressed in the several common units and covering a range of resistivity that would be covered by copper meeting the terms of the specifications.

Respectfully submitted on behalf of the Committee,

J. A. CAPP,
Chairman.

NOTE.—The above amendments were adopted by letter ballot of the Society on August 21, 1911. The Standard Specifications for Hard-Drawn Copper Wire as amended appear in the Year-Book for 1911, pages 117–126.—ED.

REPORT OF COMMITTEE B-2 ON NON-FERROUS METALS AND ALLOYS.

At the first meeting of the Committee the work was divided into the following sections, and sub-committees for each section were appointed:

1. On Pure Metals in Ingot Form: W. H. Bassett, *Chairman.*
2. On Wrought Metals and Alloys: W. R. Webster, *Chairman.*
3. On Sand Cast Metals and Alloys: T. D. Lynch, *Chairman.*
4. On White Metals—Tin, Lead or Zinc Base: G. H. Clamer, *Chairman.*

Meetings of the sub-committees were held during the year, and on June 19, 1911, a meeting of the Committee was held at Columbia University.

(1) The Proposed Standard Specifications for Copper-Wire Bars, Cakes, Slabs, Billets, Ingots, and Ingot Bars, were presented and accepted. On letter ballot there were 22 votes for adoption and 1 for amendment. The proposed amendment read "that the first sentence in Paragraph 4 shall apply to ingots and ingot bars as well as other varieties of copper." The reason given was that the appearance of the top of ingots was important, as indicating the condition of the metal. The condition of the metal, however, is fully controlled by Paragraph 3.

These specifications are therefore recommended for adoption by the Society.

(2) The Proposed Standard Specifications for Spelter were presented and accepted. On letter ballot there were 19 votes for and 1 against adoption. The following suggestions were offered: First, that full analytical methods be given for lead and iron as well as cadmium; and second, that aluminum be kept out of the "D" grade, Prime Western, or else a further grade be permitted allowing a high lead percentage and eliminating the aluminum. The vote in the negative was accompanied by the amendment that "for 'Brass Special' spelter, the average of each slab shall not exceed:

Lead	0.60 per cent.
Iron	0.03 "
Cadmium	0.60 "

and that the aggregate of all three of these impurities shall not exceed 1 per cent." Furthermore, as it is possible for each single slab of spelter to be utilized in the production of a single pot of metal, if a single slab contains a surplus of lead it might cause the rejection of the metal resulting from it, though the average analysis of ten slabs in a car might be below the specifications. Therefore, the further amendment was offered that "each single slab of spelter shall show upon analysis no greater degree of impurity than that described."

(3) The Proposed Standard Specifications for Manganese-Bronze Ingots were accepted. On letter ballot there were 17 votes for and 3 against adoption. One member states: "I am not in favor of having this specification adopted unless it is made a part of a general specification for copper-zinc alloys as distinct from copper-tin alloys. The proper name for the above alloy is 'manganese-brass ingots.' It seems to me that it would be better for the Committee to consider a general specification for brass under which could be included specifications for minor alloys, such as Tobin bronze, Muntz metal, managnese bronze, and common yellow brass."

A second objection raised was that chemical composition was not one of the absolute requirements determining acceptance or rejection. It was the intention of the Committee to cover this in the first paragraph. The following amendments to these specifications are therefore offered:

In Paragraph 1, General, omit the first paragraph; after Paragraph 1 insert Paragraph 2, Chemical Composition.

2. The chemical composition shall be as follows:

Copper	55 to	60	per cent.
Zinc	39 to	45	" "
Iron	not over	2	" "
Tin	" "	2	" "
Aluminum	" "	0.5	" "
Manganese	" "	0.5	" "

In Paragraph 6, Rejections, omit the words "accepted or," and add the words "or chemical composition" after the words "the physical tests;" thus making the paragraph read:

"All ingots in each lot will be rejected upon the physical tests or chemical composition, irrespective of the heat or heats from which the test ingots are selected."

Change the designations of existing Paragraphs 2, 3, 4, 5, 6, to Paragraphs 3, 4, 5, 6, 7.

Insert new Paragraph 8, Investigation of Claims, as follows:

8. "In case the buyer's tests show that the material does not meet these specifications, the seller shall have an opportunity to inspect the material and each party shall select a sample for re-test. If the results do not agree, each shall select a sample to be sent to a mutually agreeable umpire, whose decision shall be final. The costs of such re-tests shall be paid by the loser."

(4) The Sub-Committee on Wrought Metals reports progress. At the present time the representatives of the producing interests are engaged in drawing up tentative specifications for submission to the sub-committee. It had been hoped that these would be ready in time to secure action to be reported at this meeting, but after much consideration and discussion it was found impossible to present the matter in such a degree of completeness as was felt desirable before calling the sub-committee together.

The problem of preparing specifications for wrought non-ferrous metals presents difficulties not encountered in connection with materials not capable of so wide a range of adaptability to varying requirements. Little in the way of data connecting quantitative measurement with specific application is found on record. It has been and is the custom of the trade to determine suitability mainly by the test of use. For certain purposes very narrow limits both of mixture and heat treatment may be necessary, while for other and equally important ones various combinations of mixture, heat treatment and temper may be permissible, the limits in any particular case requiring to be determined tentatively. An added complication arises from the fact that the methods practiced by consumers are not standard, oftentimes leading to the necessity of supplying different qualities of material for identical purposes. Moreover, brass is made in lots individually so small as to preclude the possibility of an analysis or test of each lot.

The few specifications which exist are usually defective in that they are drawn to meet the extremely narrow requirements of some specific usage, and frequently under limitations which, while necessary for the case intended to be covered, are nevertheless insufficiently comprehensive.

(5) The Sub-Committee on White Metals reports progress.

They are collecting data and are making bearing tests on babbitt metals.

(6) *The Future Work of the Committee.*—It is the unanimous feeling of the Committee that the non-ferrous branch should be represented on the Executive Committee.

On account of the complexity, volume, and widely differing aspects of the work in non-ferrous metals and alloys, it is almost impossible for a single committee of normal size to work efficiently and intelligently, even with numerous sub-committees. The Committee therefore strongly recommends that the non-ferrous work of the Society be handled by definite subjects, as for example in the form of questions, similar to the method used by the International Association.

Respectfully submitted on behalf of the Committee,

WILLIAM CAMPBELL,
Chairman.

NOTE.—The Proposed Standard Specifications for Copper-Wire Bars, Cakes, Slabs, Billets, Ingots, and Ingot Bars; for Spelter; and for Manganese-Bronze Ingots, were adopted by letter ballot of the Society on August 21, 1911, and follow this report.—ED.

AMERICAN SOCIETY FOR TESTING MATERIALS

PHILADELPHIA, PA., U. S. A

AFFILIATED WITH THE

INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD SPECIFICATIONS FOR COPPER-WIRE BARS, CAKES, SLABS, BILLETS, INGOTS, AND INGOT BARS.

ADOPTED AUGUST 21, 1911.

1. All wire bars, cakes, slabs, and billets shall be stamped with the maker's brand and furnace charge mark. Ingots and ingot bars shall have a brand stamped or cast in, but need have no furnace charge mark. **Marks.**

2. The refiner shall arrange carloads or lots so that as far as possible each shall contain pieces from but one furnace charge, in order to facilitate testing by the user. **Lots.**

3. (*a*) *Metal Contents.*—The copper in all shapes shall have a purity of at least 99.880 per cent. as determined by electrolytic assay, silver being counted as copper. **Quality**

(*b*) *Conductivity.*—All wire bars shall have a conductivity of at least 98.5 per cent. (annealed); all ingots and ingot bars shall have a conductivity of at least 97.5 per cent. (annealed), excepting only arsenical copper, which shall have a conductivity of not less than 90 per cent. (annealed).

Cakes, slabs, and billets shall come under the ingot classification, except when specified for electrical use at time of purchase, in which case wire-bar classification shall apply.

The "Annealed Copper Standard," or resistance of a meter-gram of standard annealed copper at 20° C., shall be considered as 0.15302 international ohm. The per cent. conductivity for the purposes of this specification shall be calculated by dividing the resistivity of the Annealed Copper Standard by the resistivity of the sample at 20° C.

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4. Wire bars, cakes, slabs, and billets shall be substantially free from shrink holes, cold sets, pits, sloppy edges, concave tops and similar defects in set or casting. This clause shall not apply to ingots or ingot bars, in which case physical defects are of no consequence.

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dividual Pieces.

5. Five per cent. variation in weight or ¼ in. variation in any dimension from the refiner's published list or purchaser's specified size shall be considered good delivery; provided, however, that wire bars may vary in length 1 per cent. from the listed or specified length, and cakes 3 per cent. from the listed or specified size in any dimension greater than 8 ins. The weight of ingot and ingot-bar copper shall not exceed that specified by more than 10 per cent., but otherwise its variation is not important.

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6. Claims must be made in writing within thirty days of receipt of copper at the customer's mill, and the results of the customer's tests shall accompany such claims. The refiner shall be given one week from date of receipt of complaint to investigate his records, and shall then either agree to replace the defective copper or send a representative to the mill. No claims will be considered unless made as above stated, and if the copper in question, unused, cannot be shown to the refiner's representative.

Claims against quality will be considered as follows:

(*a*) Conductivity by furnace charges, ingot lots, or ingot-bar lots.

(*b*) Metal contents by furnace charges, ingot lots, or ingot-bar lots.

(*c*) Physical defects by individual pieces.

(*d*) Variation in weights or dimensions by individual pieces.

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Claims.

7. The refiner's representative shall inspect all pieces where physical defects or weight or dimension variation is claimed. If agreement is not reached the question of fact shall be submitted to a mutually agreeable umpire, whose decision shall be final.

In a question of metal contents each party shall select a sample of two pieces. These shall be drilled in the presence of both parties, several holes approximately ½ in. in diameter being drilled completely through each piece, scale from set to be rejected. No lubricant shall be used and drilling shall not be forced sufficiently to cause oxidation of chips. The resulting samples shall be cut up, mixed, and separated into three parts, each of which shall be placed in a sealed package, one for each party and one for the umpire if necessary. Each party shall make an analysis, and if the results do not establish or dismiss the claim to the satisfaction of both parties the third sample shall be submitted to a mutually agreeable umpire, who shall determine the question of fact, and whose determination shall be final.

In a question of conductivity each party shall select two samples, and in the presence of both parties these shall be rolled hot and drawn cold into wire of 0.080 in. diameter, which shall be annealed at approximately 500° C. Three samples shall be cut from each coil and the same procedure followed as described in the previous paragraph.

Settlement of Claims.

8. The expenses of the shipper's representative and of the umpire shall be paid by the loser, or divided in proportion to the concession made in case of compromise. In case of rejection being established, the damage shall be limited to payment of freight both ways by the refiner for substitution of an equivalent weight of copper meeting these specifications.

EXPLANATORY NOTE.

These specifications are intended to allow for the fact that the refiner produces copper and gauges its quality in furnace charge lots, while the user purchases copper in carload lots, necessarily obtaining a different basis for sampling.

It is intended to cover in these specifications an average grade of copper suitable for all mechanical uses and for making alloys to be used in various wrought forms.

The specifications also recognize the fact that certain shapes are largely put to electrical uses where high electrical conductivity is important.

They do not take into consideration the so-called casting copper used for the purpose of alloying with other metals to produce cast shapes.

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA, PA., U. S. A.
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STANDARD SPECIFICATIONS FOR SPELTER.

ADOPTED AUGUST 21, 1911.

1. Under these specifications Virgin Spelter, that is, spelter made from ore or similar raw material by a process of reduction and distillation and not produced from re-worked metal, is considered in four grades, as follows:

A	High Grade.
B	Intermediate.
C	Brass Special.
D	Prime Western.

Marks. 2. A brand shall be cast in each slab by which the maker and grade can be identified.

Lots. 3. The maker shall use care to have each carload of as uniform quality as possible.

Composition. 4. *A. High Grade.*—The spelter shall not contain over

0.07 per cent. lead.
0.03 " " iron.
0.05 " " cadmium.

It shall be free from aluminum.

The sum of the lead, iron, and cadmium shall not exceed 0.10 per cent.

B. Intermediate.—The spelter shall not contain over

0.20 per cent. lead.
0.03 " " iron.
0.50 " " cadmium.

It shall be free from aluminum.

The sum of the lead, iron, and cadmium shall not exceed 0.50 per cent.

C. Brass Special.—The spelter shall not contain over

0.75 per cent. lead.
0.04 " " iron.
0.75 " " cadmium.

It shall be free from aluminum.

The sum of the lead, iron, and cadmium shall not exceed 1.20 per cent.

D. Prime Western.—The spelter shall not contain over

1.50 per cent. lead.
0.08 " " iron.

Physical. 5. The slabs shall be reasonably free from surface corrosion or adhering foreign matter.

Sampling. 6. Not less than ten slabs shall be taken as a sample from each car; for smaller lots, in the same proportion to the total number, but in no case less than three slabs. In case of dispute half of the sample is to be taken by the maker and half by the purchaser; and the whole shall be mixed.

The slabs selected as samples are to be sawed completely across and the sawdust used as a sample. In case no saw is available for this purpose, the slabs should be drilled completely through and the drillings cut up into short lengths. The saw or drill used must be thoroughly cleaned. No lubricant shall be used in either sawing or drilling, and the sawdust or drilling must be carefully treated with a magnet to remove any particles of iron derived from the tools.

Analysis. 7. *Lead.*—For the determination of lead in High Grade not less than 25 grams, in Intermediate not less than 15, in Brass Special not less than 10, and in Prime Western not less than 5 grams, shall be taken; that is, the sample used for analysis should not contain less than 0.01 gram lead.

Iron.—The sample for iron should contain not less than 25 grams for the three higher grades and not less than 10 grams for Prime Western. The entire sample must be dissolved, the iron precipitated as ferric-hydroxide, then re-dissolved, reduced, and the iron determined by titration.

Cadmium.—Dissolve 25 grams in 330 cc. of a solution of one part of hydrochloric acid (specific gravity 1.2) and five parts of water. Let it stand over night; filter and wash; reject filtrate and dissolve the residue, which should be about 5 per cent. of the zinc, in nitric acid. Add 10 cc. of sulphuric acid; evaporate to fumes; dilute and filter out and wash the lead sulphate. Dilute the solution to 500 cc.; add 5 grams of ammonium chloride; pass a slow stream of hydrogen sulphide for one hour and let stand for about five hours; filter, wash with hot water; dissolve in 10 cc. of sulphuric acid and 50 cc. of water; filter and wash. Dilute to 400 cc.; precipitate with hydrogen sulphide as before. Weigh as cadmium sulphide or dissolve in hydrochloric acid and titrate with potassium ferrocyanide.

Claims.

8. Claims to be considered shall be in writing within thirty days of receipt of material at customer's mill and the results of customer's test shall be given. The shipper shall be given one week from date of receipt of such claim to investigate his records and then shall either agree to satisfy the claim or send a representative to the mill.

(*a*) *Analysis by Car Lots.*—No claims shall be considered unless the minimum samples as specified for the grade in question can be shown to such representative.

(*b*) *Physical Defects of Individual Pieces.*—No claims shall be considered unless the spelter in question, unused, can be shown to such representative.

Mill Treatment.

9. Where the spelter satisfies the chemical and physical requirements of these specifications, it shall not be condemned for defects of alloys in which it is used or for defects in the coating of galvanized products.

Investigation of Claims.

10. The maker's representative shall inspect all pieces where physical defects are claimed. If agreement is not reached the question of fact shall be submitted to a mutually agreeable umpire, whose decision shall be final.

On a question of metal contents an adequate sample shall be drawn by the representatives of both parties; the sample shall be prepared from the slabs so selected as described under "Sampling." The sample shall be mixed and separated into three parts, each of which shall be placed in a sealed package, one for each party and one for the umpire if necessary. Each party shall make an analysis and if the results do not establish or dismiss the claim to the satisfaction of both parties, the third sample shall be submitted to a mutually agreeable umpire, who shall determine the question of quality and whose determination shall be final.

Settlement of Claims.

11. The expenses of the maker's representative and of the umpire shall be paid by the loser or divided in proportion to concession made in case of compromise.

In case of rejection being established, damages shall be limited to the payment of freight both ways by the maker for substitution of an equivalent weight of spelter meeting these specifications.

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA, PA., U. S. A.
AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

STANDARD SPECIFICATIONS FOR MANGANESE BRONZE INGOTS.

ADOPTED AUGUST 21, 1911.

General.

1. This specification is intended to cover manganese-bronze ingots, having notched flat bottoms, approximately 3 by $2\frac{3}{4}$ ins. wide by 12 ins. long, properly tapered to strip easily from an iron mold.

Chemical Composition.

2. The chemical composition shall be as follows:

Copper	55	to	60	per cent.
Zinc	39	to	45	" "
Iron		not over	2	" "
Tin		not over	2	" "
Aluminum		not over	0.5	" "
Manganese		not over	0.5	" "

Physical Properties.

3. The ultimate tensile strength shall be not less than 70,000 lbs. per sq. in.

The elongation in 2 ins. shall be not less than 20 per cent.

Test Specimens.

4. The standard turned test specimen, as shown by Fig. 1, 0.5 in. diameter and 2 ins. gauge length, shall be used to determine the physical properties as specified above.

Number and Location of Test of Specimens.

5. One test ingot shall be selected by the inspector to represent 10.000 lbs. of ingots or fraction thereof. The test specimen shall be cut from one corner near the bottom of the ingot. In case the test specimen shows a flaw, two additional tests may be selected by the inspector from the same lot, and tested to represent the lot in question.

6. Each furnace charge shall be kept separate until the lot is sampled by the inspector, and each ingot thereof stamped with its proper heat number. When the ingot is sampled at destination, various heats can be mixed in shipment, but must be stamped with their proper heat number. **Marking.**

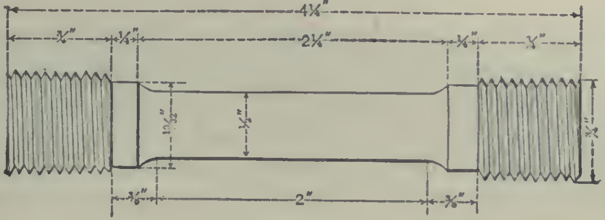

FIG. 1.

7. All ingots in each lot will be rejected upon the physical tests or chemical composition, irrespective of the heat or heats from which the test ingots are selected. **Rejections.**

8. In case the buyer's tests show that the material does not meet these specifications, the seller shall have an opportunity to inspect the material and each party shall select a sample for re-test. If the results do not agree, each shall select a sample to be sent to a mutually agreeable umpire, whose decision shall be final. The costs of such re-tests shall be paid by the loser. **Investigation of Claims.**

REPORT OF COMMITTEE C-3 ON STANDARD SPECIFICATIONS FOR PAVING AND BUILDING BRICK.

The Committee announces with sincere regret the resignation from the chairmanship and withdrawal from the Committee of Mr. L. W. Page, whose duties as chairman of the very large and important committee on road materials are so exacting that he feels he can no longer serve on Committee C-3. Mr. Page has been a member of the Committee since its formation and has for a number of years served as its chairman. His resignation was accepted with reluctance, protest, and deep regret. Upon nomination by him an acting chairman was selected to fill the unexpired term.

The program announced last year has been only partially executed on account of difficulties which have arisen in securing and transporting materials to all of the laboratories that agreed to cooperate in the work. Additional information has been secured, but it is not sufficient to justify the Committee in attempting to submit specifications to the Society for consideration.

The present scope of the subjects before the Committee, and the widely differing considerations that arise in studying the various elements which must enter into the preparation of standard specifications for paving and building brick, have led the Committee to form its membership into two Sub-Committees; one on Paving Brick and the other on Building Brick.

The membership of the Committee has been increased so that all interests involved, including manufacturers, testing and investigating laboratories, and consumers, are represented, and it is hoped that with the re-organization, the promises which have been made for several years may within the next year begin to bring results which will reward the Society for its patience and indulgence.

Respectfully submitted on behalf of the Committee,

D. E. Douty,
Acting Chairman.

E. W. Lazell,
Secretary.

REPORT OF COMMITTEE C-4 ON STANDARD SPECIFICATIONS AND TESTS FOR CLAY AND CEMENT SEWER PIPES.

The Committee begs to submit the following progress report on the work it has done during the past year:

The plan along which the work has been carried out was outlined in previous reports. The work was divided into three classes: first, the demands made upon sewer pipes; second, the practical examination and tests to secure materials and manufacture of pipe answering the foregoing demands; and third, the specifications for the work of laying pipes under the usual conditions occurring in practice.

The Committee has as yet been studying only the first and second parts, since the third part will be based to some extent upon the conclusions reached in the others. It will be seen that the matter relates equally to clay and cement-concrete pipes and that the demands upon the pipes are considered with reference to the conditions affecting their strength, durability, and serviceability. With the demands thus fixed for all practicable conditions, it will be possible to consider the properties of the several materials and the tests which can be practicably applied thereto to determine their compliance with the demands.

The classified analytical data accompanying this report may be considered a complete preliminary digest of the subject for the research and consideration of the entire membership of the Committee. It will be observed that the various phases of all the subjects have been reduced to interrogatory form for the purpose of assisting discussion of the main subjects and their co-related matters. It is also believed that by this procedure the opinions of the several members can be more readily reduced to a common basis of agreement or dissension. Some of the subjects have already been assigned to sub-committees.

Regarding the second class of data, "Examinations and Tests," it should be observed that the nature and extent of many of these must depend largely upon the determinations to be arrived at as to the demands upon the pipes and their properties. This part of the subject has, however, been treated in the same manner as the first class of data in order that such as can may be promptly disposed of, and that discussion of the remainder may proceed as rapidly as agreement upon demands and properties and materials can be reached.

It is believed the work, as laid out, exceeds in its scope previous treatment which this subject has received.

Since the last report the officers of the Committee have held sixteen meetings, in addition to which correspondence has been held with the several members in various parts of the country.

It is the purpose to hold at least one meeting during the convention for discussion of the subject by all members of the Committee who may attend.

Respectfully submitted on behalf of the Committee,

RUDOLPH HERING,
Chairman.

E. J. FORT,
Secretary.

A. J. PROVOST, JR.,
Vice-Chairman.

SCHEDULE OF ANALYTICAL DATA FOR SEWER PIPE: DEMANDS AND PROPERTIES; MILL, FIELD AND LABORATORY TESTS.

FIRST CLASS OF DATA: THE DEMANDS MADE UPON SEWER PIPES.

I. CONDITIONS AFFECTING STRENGTH.

(*a*) *Resistance to External Forces.*—What external forces are imposed upon sewer pipes under conditions of use? (Load: lbs. per hor. sq. ft. for several classes of depth; Impact: kinetic energy, $mv^2/2$, for several classes of depth.)

(*b*) *Resistance to Internal Forces.*—What internal forces are imposed upon sewer pipes under conditions of use? (Hydrostatic pressure: lbs. per sq .in.)

A. Clay Pipe.

1. (*a*) *Physical Properties of Raw Materials.*—What physical properties should a clay or mixture of clays have to satisfy the above demands for strength in the finished product?

(*b*) *Chemical Properties of Raw Materials.*—What chemical properties should a clay or mixture of clays have to satisfy the above demands for strength in the finished product?

2. *Porosity or Compactness.*—What is the allowable limit of porosity and uniformity of porosity to satisfy the above demands for strength?

3. *Toughness.*—How shall the toughness required to satisfy the above demands for strength be expressed?

4. *Vitrification.*—How far should vitrification be considered as affecting strength to satisfy the above demands for strength?

5. *Fire Cracks, Blisters and Laminations.*—What are the permissible limits to satisfy the above demands for strength?

6. *Thickness of Shell.*—(*a*) In what manner should the relation between the thickness of shell and its strength be expressed for various loads?

(*b*) Should the thickness of pipe shell be expressed as a function of the diameter for constant load? If so, by what algebraic formula should the thickness be expressed?

(*c*) How many classes of pipe should be provided? (See external stress load.)

7. *Ends of Pipe and Jointing Materials.*—What details of joints are most important in affecting strength of pipe laid?

(*a*) Shape and dimensions.

(*b*) Making of joints.

  1. Materials: their strength and rigidity.
  2. Methods of application of the materials.

8. *Branches, Spurs, etc.*—(*a*) Method of attachment to pipe.

(*b*) Angle of junction with pipe.

(*c*) Dimensions.

B. CEMENT-CONCRETE PIPE.

1 *a. Physical Properties of Ingredients.*

(*a*) *Cement.*—What physical properties should the cement have to satisfy the above demands for strength in the finished product? (Uniformity, fineness, weight, water, absorption, etc.) Are the standard specifications heretofore adopted (American Society of Civil Engineers and American Society for Testing Materials) sufficient?

(*b*) *Sand.*—What physical properties should the sand have to satisfy the above demands for strength in the finished product? (Fineness, cleanness, sharpness, etc.)

(*c*) *Stone.*—What physical properties should the stone have to satisfy the above demands for strength in the finished product? (Uniformity of size, average diameters, hardness, cleanness, structural character, etc.)

(*d*) *Water.*—What degrees of cleanness should the water have and what limits of temperature are allowable to satisfy the above demands for strength in the finished product?

(*e*) *Waterproofing Compounds.*—What physical characteristics should a waterproofing compound possess to satisfy the above demands for strength in the finished product?

1 *b. Chemical Properties of Ingredients.*

(*a*) *Cement.*—What chemical properties should the cement have to satisfy the above demands for strength in the finished product? Are the standard specifications heretofore adopted (American Society of Civil Engineers and American Society for Testing Materials) sufficient?

(*b*) *Sand.*—What chemical properties should the sand have to satisfy the above demands for strength in the finished product? (Silicious, calcareous, aluminaceous, micaceous, etc.)

(*c*) *Stone.*—What chemical properties should the stone have to satisfy the above demands for strength in the finished product? (Beneficial and deleterious.)

(*d*) *Water.*—What chemical properties should the water have to satisfy the above demands for strength in the finished product? (Salinity, acidity, alkalinity, etc.)

(*e*) *Waterproofing Compounds.*—What chemical properties should a waterproofing compound possess to satisfy the above demands for strength in the finished product?

2. *Proportions of Ingredients.*—What considerations should govern the determination of the best proportions of the above ingredients to satisfy the above demands for strength in the finished product? How can these proportions be varied to correspond with the varying physical characteristics of stone and sand available in different localities?

3. *Porosity or Compactness.*—What are the effects upon the strength of the finished product caused by various degrees of porosity? What are the allowable limits of porosity and uniformity of porosity to satisfy the above demands for strength?

4. *Toughness.*—How shall the toughness required to satisfy the above demands for strength be expressed?

5. *Age of Pipe before Use.*—How old should pipe be before fair tests can be made, and when can the ultimate strength be safely estimated?

6. *Hair Cracks.*—How deep, wide, long, extensive, and frequent may hair cracks be without materially affecting the strength of the pipe?

7. *Thickness of Shell.*—(*a*) In what manner should the relation between the thickness of shell and its strength be expressed for various loads?

(*b*) Should the thickness of pipe be expressed as a function of the diameter for constant load? If so, by what algebraic formula should the thickness be expressed?

(*c*) How many classes of pipe should be provided? (See external stress load.)

8. *Ends of Pipes and Jointing Materials.*—What details of joints are most important in affecting the strength of pipe laid?

(*a*) Shape and dimensions.
(*b*) Making of joints.
    1. Materials: their strength and rigidity.
    2. Methods of application of the materials.

9. *Branches, Spurs, etc.*—(*a*) Method of attachment to pipe.
(*b*) Angle of junction with pipe.
(*c*) Dimensions.

II. CONDITIONS AFFECTING DURABILITY.

A. RESISTANCE TO DISINTEGRATION BY ACIDS, ALKALIES, STEAM, AND FROST.

(*a*) *Clay Pipe.*

1. *Physical and Chemical Properties of Materials.*—What physical and chemical properties of clay or mixtures of clays are required to resist disintegration by acids, alkalies, steam, frost, etc., after proper burning?
2. *Porosity or Compactness.*—What relation has porosity to disintegration by acids, alkalies, steam, frost, etc., and what are its allowable limits?
3. *Vitrification.*—What is the relation between vitrification and disintegration by acids, alkalies, steam, frost, etc.?
4. *Glazing.*—What should be the character of the glazing to resist disintegration by acids, alkalies, steam, frost, etc.? (Salt and other glazings.)
5. *Limits allowable for Fire Cracks, Blisters, etc.*—What are the allowable limits for fire cracks, blisters, etc., to resist disintegration by acids, alkalies, steam and frost?

(*b*) *Cement-Concrete Pipe.*

1. *Physical and Chemical Properties of Ingredients: Cement, Sand, Stone, Water, and Waterproofing Compounds.*—What physical and chemical properties of the combination of ingredients—cement, sand, stone, water and waterproofing materials—will best resist disintegration by acids, alkalies, steam, frost, etc.?
2. *Proportions of Ingredients.*—What considerations should govern the best proportions of the ingredients to prevent disintegration by acids, alkalies, steam, frost, etc.? How can these proportions be varied to correspond with the physical characteristics of stone and sand available in different localities?
3. *Porosity or Compactness.*—What relation has porosity to disintegration by acids, alkalies, steam, frost, etc., and what are its allowable limits?
4. *Coatings.*—What kind of materials is best qualified for use as coatings to resist disintegration by acids, alkalies, steam, frost, etc.,and how should they be applied?
5. *Linings.*—What kind of artificial linings are efficient and practicable to resist disintegration by acids, alkalies, steam, frost, etc., and how should they be applied? (Brick, vitrified clay, asphalt, and other compositions.)

(*c*) *Materials Used for Joints.*—What consideration should be given to the disintegration of jointing materials by acids, etc.? What materials otherwise suitable best resist disintegration?

B. RESISTANCE TO ABRASION.

(*a*) *Clay Pipe.*

1. *Physical and Chemical Properties of Materials.*—What physical and chemical properties of clay or mixtures of clays will produce the necessary hardness and toughness in the burnt product to best resist abrasion?
2. *Porosity or Compactness.*—What relation has porosity to resistance to abrasion, and what are its allowable limits?

3. *Vitrification.*—What is the relation between vitrification and resistance to abrasion?
4. *Glazing.*—What should be the character of the glazing to resist abrasion? (Salt and other glazings.)

(*b*) *Cement-Concrete Pipe.*

1. *Physical and Chemical Properties of Ingredients: Cement, Sand, Stone, Water, and Waterproofing Compounds.*—What should be the physical and chemical properties of the combination of ingredients—cement, sand, stone, water, and waterproofing materials—in order to produce the necessary hardness and toughness to best resist abrasion?
2. *Proportions of Ingredients.*—What are the best proportions of the above ingredients? (See Question 2 under Cement-Concrete Pipe, Disintegration by Acids, etc., page 159.)
3. *Porosity or Compactness.*—What relation has porosity to resistance to abrasion, and what are its allowable limits?
4. *Coatings.*—Are coatings useful in resisting abrasion, and if so, what is their nature?
5. *Artificial Linings.*—What kind of artificial linings are efficient and practicable to best resist abrasion? (Brick, vitrified clay, asphalt, and other compositions.)
6. *Age before Use.*—How old should pipe be before fair tests can be made from which to judge the permanent resistance to abrasion?

(*c*) *Materials Used for Joints.*—What consideration should be given to the resistance of jointing materials to abrasion? What materials otherwise suitable best resist abrasion?

III. CONDITIONS AFFECTING SERVICEABILITY.

A. COMPLETE AND RAPID DELIVERY OF SEWAGE.

(*1*.) *Smoothness of Interior Surface.*

What are the practical effects of and reasons for demanding smoothness of the interior surface of sewer pipes?

(*a*) *Clay Pipe.*

1. *Glazing.*—What conditions affect the proper glazing of a pipe to produce the desired result of smoothness in the finished product? What are the allowable limits of imperfection in the glaze affecting smoothness?

2. *Blisters, Fire Cracks, etc.*—To what extent are fire cracks, blisters, and similar imperfections allowable, consistent with suitable smoothness of interior surface? (Retarding flow and retaining suspended matter.)

3. *Projecting Joints.*—To what extent are projecting joints allowable, consistent with suitable smoothness of interior surface?

4. *Branches, Spurs, etc.*—What are the requirements in the attachment of branches, spurs, etc., to secure suitable smoothness of interior surface and meet the requirements of manufacture? What is the most desirable and practicable angle of connection?

(*b*) *Cement-Concrete Pipe.*

1. *Natural Surface.*—Under what conditions of service is a natural surface sufficient and suitable, and what specification should govern as to its character to secure suitable smoothness of interior surface?

2. *Coatings.*—Under what conditions of service are coatings necessary or desirable, and what specifications as to their character and application should govern to secure suitable smoothness of interior surface?

3. *Linings.*—Under what conditions of service are linings necessary or desirable, and what materials are most suitable for this purpose? What specification as to character and application should govern to secure suitable smoothness of interior surface?

4. *Branches, Spurs, etc.*—What are the requirements in the attachment of branches and spurs to secure satisfactory results as to smoothness of interior surface and meet the requirements of manufacture? What is the most desirable and practicable angle of connection?

(2.) *Sectional Form.*

1. *Circular.*—What are the advantages and disadvantages of the circular section for the complete and rapid delivery of sewage?

2. *Egg-Shape and other Forms.*—What are the advantages and disadvantages of egg-shape and other forms for the complete and rapid delivery of sewage?

(3.) *Permissible Deviations from True Form.*

1. *Imperfect Forms: Inaccurate Dimensions, Irregular Shape, Eccentricity of Molds, Roughness of Surface, Improper Matching of Molds, etc.*—To what extent are imperfections of form allowable, consistent with complete and rapid delivery of sewage and with the requirements of manufacture?

2. *Warping.*—To what extent is warping allowable, consistent with complete and rapid delivery of sewage and with the requirements of manufacture? How should the permissible limits be expressed? To what extent does warping limit permissible length of pipe of various diameters?

3. *Shrinkage.*—What are the permissible limits of shrinkage, and what physical and chemical properties should a clay or mixture of clays have to satisfy this requirement?

(4.) *Permissible Defects Relating to Percolation and Leakage.*

1. *Porosity.*—How does porosity affect the complete and rapid delivery of sewage? To what extent, under various conditions, is it allowable?

2. *Cracks.*—What kinds of cracks are found in sewer pipe? What types, under what circumstances, and to what extent are they allowable to meet the requirements of serviceability?

3. *Fractures.*—To what extent are fractures allowable and what restrictions should be imposed as to the use of fractured pipe?

4. *Joints and Jointing Materials.*—What are the merits of the various types of joints available regarding percolation and leakage, and what should be the characteristics of suitable joints? What are the merits of the various materials available for filling joints regarding percolation and leakage, and how should they be applied to secure the best results? (Tightness, elasticity, etc.)

B. EASE AND ECONOMY OF HANDLING AND LAYING.

1. *Toughness of Material.*—To what extent should the requirements of ease and economy of handling and laying govern

the required toughness of the pipe? What measure of toughness should be required, and how should it be expressed?

2. *Weight.*—To what extent should ease and economy of handling and laying govern the weight of pipe?

3. *Length of Straight Pipe and Specials.*—What is the best length of straight pipe and specials so far as ease and economy of handling and laying are concerned?

4. *Form of Base.*—How do the forms of base of both clay and cement-concrete pipe affect the ease and economy of handling and laying? What forms of base are most desirable, and what are their advantages? (Integral bases, base blocks, etc.)

5. *Ends of Pipes.*—How does the manner in which the ends of pipes are formed affect the ease and economy of handling and laying? What are the advantages and disadvantages of the various types, such as (*a*) hub and spigot, (*b*) butt and collar, (*c*) beveled, (*d*) other forms? What details or special features of design should each of the above joints have, such as width and depth of joint, centering devices, dimensions of collar, angle of bevel, and treatment of contact surfaces, such as scoring, omission of glaze, etc.?

6. *Materials Used for Jointing.*—What consideration should be given to ease and economy of laying in the selection of jointing materials?

SECOND CLASS OF DATA: PRACTICAL EXAMINATIONS AND TESTS TO SECURE MATERIALS AND MANUFACTURE OF PIPE ANSWERING THE FOREGOING DEMANDS.

I. EXAMINATIONS LIMITED TO OBSERVATIONS BY THE SENSES, CRUDE MEASUREMENTS, AND CRUDE FORCES (MOSTLY MILL AND FIELD).

A. Clay Pipe.

1. *Identification.*—(*a*) What markings or imprints should a pipe bear to identify its manufacture and adaptability to its proposed use? (Manufacturer's name, location of mill, Class A, B, C, etc., indicating thickness of shell.)

(*b*) What should be the nature of these markings and where should they be placed?

2. *Examination of Material.*—(*a*) What observations are practicable in the field or at the mill to determine whether the pipe material is uniform in character, free from cinder and other interior structural defects?

(*b*) To what extent and up to what proportion should pipes furnished for use be so observed?

3. *True Sectional Form and Dimensions.*—(*a*) What measurements should be made to determine the true sectional form and dimensions of pipe?

(*b*) What are the allowable limits of deviation?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

4. *Warping.*—(*a*) What measurements in the field or at the mill are desirable to determine the extent of warp in a pipe?

(*b*) What are the allowable limits?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

5. *Uniform Thickness of Shell.*—(*a*) How many measurements are ordinarily desirable to determine the shell thickness and its uniformity in a pipe?

(*b*) What are the allowable limits of deviation?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

6. *Vitrification.*—(*a*) What observations in the field or at the mill are practicable for determining the degree and uniformity of vitrification in a pipe?

(*b*) What are the allowable limits and how should they be expressed?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

7. *Fractures.*—(*a*) What observations in the field or at the mill are practicable to determine the nature and extent of fractures in the pipe?

(*b*) What are the allowable limits of fractures?

(*c*) To what extent should pipes furnished for use be so observed?

8. *Cracks Passing through Shell.*—(*a*) What observations in

the field or at the mill are practicable to determine the existence of cracks passing through the shell?

(*b*) Is this defect permissible? If so, under what conditions?

(*c*) To what extent should pipes furnished for use be so observed?

9. *Laminations.*—(*a*) What observations in the field or at the mill are practicable to determine the extent of laminations in pipes?

(*b*) What are the allowable limits, and how should they be expressed?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

10. *Fire Cracks.*—(*a*) What observations in the field or at the mill are practicable to determine the extent of fire cracks in the pipe?

(*b*) What are the allowable limits, and how should they be expressed?

(*c*) To what extent should pipes furnished for use be so observed?

11. *Blisters and Protuberances.*—(*a*) How should the extent and character of blisters and protuberances on a pipe be observed and expressed?

(*b*) What are the allowable limits for each?

(*c*) To what extent should pipes furnished for use be so observed?

12. *Glazing.*—(*a*) How should the extent and character of glazing on a pipe be observed and expressed?

(*b*) What are the allowable deficiencies?

(*c*) To what extent should pipes furnished for use be so observed?

13. *Toughness.*—(*a*) What tests in the field or at the mill are practicable to determine the degree of toughness and hardness of the pipe material? (Resistance to impact.) (Resistance to abrasion.)

(*b*) How should the required toughness be expressed?

(*c*) To what extent and up to what proportion should pipes furnished for use be so tested?

14. *Ends of Pipes.*—(*a*) How should the ends of pipes be observed as to their finish, shape, and dimensions?

(*b*) What defects in these are permissible?

(*c*) To what extent should pipes furnished for use be so observed?

15. *Angles and Secure Attachment of Branch Pieces.*—(*a*) What observations in the field or at the mill are practicable to determine the accuracy of angles and secure attachment of branch pieces?

(*b*) What deviation from true angles is permissible?

(*c*) What defects, if any, in the attachment of spurs or branch pieces are permissible?

(*d*) To what extent should branches furnished for use be so observed?

B. CEMENT-CONCRETE PIPE.

1. *Identification and Age.*—(*a*) What markings or imprints should a pipe bear to identify its manufacture and adaptability to its proposed use? (Manufacturer's name, location of mill, Class, A, B, C, etc., indicating thickness of shell.)

(*b*) What should be the nature of these markings and where should they be placed?

2. *Examination of Materials.*—(*a*) What observations are practicable in the field or at the mill to determine whether the pipe material is uniform in character, free from deleterious ingredients and other interior structural defects?

(*b*) To what extent and up to what proportion should pipes furnished for use be so observed?

3. *True Sectional Form and Dimensions.*—(*a*) What measurements should be made to determine the true sectional form and dimensions of pipe?

(*b*) What are the allowable limits of deviation?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

4. *Uniform Thickness of Shell.*—(*a*) How many measurements are ordinarily desirable to determine the shell thickness and its uniformity in a pipe?

(*b*) What are the allowable limits of deviation?

(*c*) To what extent and up to what proportion should pipes furnished for use be so observed?

5. *Smoothness of Interior Surface.*—(*a*) What observations are practicable in the field or at the mill to determine the degree of smoothness of the interior surface of a cement concrete pipe?

(*b*) What degree of roughness is allowable?

(*c*) To what extent should pipes furnished for use be so observed?

6. *Security and Uniformity of Artificial Linings and Coatings.*—(*a*) What observations are practicable in the field or at the mill to determine the degree of security and degree of uniformity of artificial linings or coatings?

(*b*) What defects in these are permissible?

(*c*) To what extent should pipes furnished for use be so observed?

7. *Fractures.*—(*a*) What observations in the field or at the mill are practicable to determine the nature and extent of fractures in the pipe?

(*b*) What are the allowable limits of fractures?

(*c*) To what extent should pipes furnished for use be so observed?

8. *Cracks Passing through Shell.*—(*a*) What observations in the field or at the mill are practicable to determine the existence of cracks passing through the shell?

(*b*) Is this defect permissible? If so, under what conditions?

(*c*) To what extent should pipes furnished for use be so observed?

9. *Hair Cracks.*—(*a*) What observations in the field or at the mill are practicable to determine the extent of hair cracks in the pipe?

(*b*) What are the allowable limits, and how should they be expressed?

(*c*) To what extent should pipes furnished for use be so observed?

10. *Toughness.*—(*a*) What tests in the field or at the mill are practicable to determine the degree of toughness and hardness of the pipe material? (Resistance to impact.) (Resistance to abrasion.)

(*b*) How should the required toughness be expressed?

(*c*) To what extent and up to what proportion should pipes furnished for use be so tested?

11. *Ends of Pipes.*—(*a*) How should the ends of pipes be observed as to their finish, shape, and dimensions?

(*b*) What defects in these are permissible?

(*c*) To what extent should pipes furnished for use be so observed?

12. *Angles and Secure Attachment of Branch Pieces.*—(*a*) What observations in the field or at the mill are practicable to determine the accuracy of angles and secure attachment of branch pieces?

(*b*) What deviation from true angles is permissible?

(*c*) What defects, if any, in the attachment of spurs or branch pieces are permissible?

(*d*) To what extent should branches furnished for use be so observed?

II. TESTS DETERMINING PROPERTIES BY METHODS AND APPARATUS TO MEASURE THE EFFECTS OF PHYSICAL AND CHEMICAL FORCES (MOSTLY LABORATORY).

A. SPECIAL PHYSICAL AND CHEMICAL TESTS OF MATERIALS.

To what extent and under what circumstances should any or all of these tests be applied?

1. *Clay.*—(*a*) What physical tests are advisable and practicable to determine whether a clay or a mixture of clays will satisfy the requirements of strength, durability and serviceability in the finished product?

(*b*) What chemical tests are advisable and practicable to determine whether a clay or a mixture of clays will satisfy the requirements of strength, durability and serviceability in the finished product?

2. *Cement.*—(*a*) What physical tests are advisable and practicable to determine whether a cement will satisfy the requirements of strength, durability and serviceability in the finished product?

(*b*) What chemical tests are advisable and practicable to determine whether a cement will satisfy the requirements of strength, durability and serviceability in the finished product?

(*c*) Are the tests heretofore adopted by the American Society of Civil Engineers and the American Society for Testing Materials sufficient for this purpose?

3. *Sand.*—(*a*) What physical tests are advisable and practicable to determine the suitability of sands to satisfy the requirements of strength, durability and serviceability in the finished product?

(*b*) What chemical tests are advisable and practicable to determine the suitability of sands to satisfy the requirements of strength, durability and serviceability in the finished product?

(*c*) What chemical tests are advisable and practicable to determine the suitability of sands to satisfy the requirements of strength, durability and serviceability in the finished products?

4. *Stone.*—(*a*) What physical tests are advisable and practicable to determine the suitability of stone to satisfy the requirements of strength, durability and serviceability in the finished product?

(*b*) What chemical tests are advisable and practicable to determine the suitability of stone to satisfy the requirements of strength, durability and serviceability in the finished product?

5. *Water.*—What physical and chemical tests are advisable and practicable to determine the suitability of water to satisfy the requirements of strength, durability and serviceability in the finished product?

6. *Waterproofing Compounds.*—What physical and chemical tests are advisable and practicable to determine the suitability of waterproofing compounds to satisfy the requirements of strength, durability and serviceability in the finished product?

B. SPECIAL TESTS FOR CLAY PIPE.

To what extent and under what circumstances should any or all of these tests be applied?

1. *Vitrification.*—What tests are advisable and practicable to determine the extent and character of vitrification of a clay pipe required to satisfy the demands of strength, durability and serviceability?

2. *Toughness.*—(*a*) What tests are advisable and practicable to determine the degree and character of toughness of a clay pipe required to satisfy the demands of strength and durability (as opposed to brittleness, softness, etc.).

(*b*) What scale or standard of toughness will best satisfy the demands of strength and serviceability?

3. *Resistance to Abrasion.*—(*a*) What tests are advisable and practicable to determine the resistance of a clay pipe to abrasion?

(*b*) What scales or standards of resistance will best satisfy the various demands of durability? (Presence or absence of grit, high and low velocities, etc.)

4. *Resistance to Rupture.*

(*a*) *External Forces.*—1. What tests are most practicable to determine the resistance of a clay pipe to rupture by the external forces produced under various conditions of use?

2. What scales or standards of resistance will best satisfy the various demands of strength and serviceability?

(*b*) *Internal Forces.*—1. What tests are most practicable to determine the resistance of a clay pipe to rupture by the internal forces produced under varying conditions of use? (Inverted siphons, etc.).

2. What scales or standards of resistance will best satisfy the various demands of strength under different pressures?

5. *Resistance to Disintegration.*

(*a*) *By Acids.*—What tests are advisable and practicable to determine the resistance of a clay pipe to disintegration by acids occurring in sewage?

(*b*) *By Alkalies.*—What tests are advisable and practicable to determine the resistance of a clay pipe to disintegration by alkalies occurring in sewage?

(*c*) *By Steam.*—What tests are advisable and practicable to determine the resistance of a clay pipe to disintegration by steam where discharged into sewers?

(*d*) *By Frost.*—What tests are advisable and practicable to determine the resistance of a clay pipe to disintegration by frost?

6. *Glazing.*—What tests are advisable and practicable to determine the extent and character of glaze on a clay pipe required to satisfy the various demands of durability and serviceability?

C. SPECIAL TESTS FOR CEMENT-CONCRETE PIPE.

To what extent and under what circumstances should any or all of these tests be applied?

1. *Age before Testing.*—What age should a cement-concrete pipe have attained before conclusive results can be obtained from the following tests made thereon?

2. *Compactness.*—What tests are advisable and practicable to determine whether a cement-concrete pipe is sufficiently compact to satisfy the demands of strength, durability and serviceability?

3. *Toughness.*—(*a*) What tests are advisable and practicable to determine the degree and character of toughness of a cement-concrete pipe, as opposed to brittleness and softness?

(*b*) What scale or standard of toughness will best satisfy the demands of strength and serviceability?

4. *Resistance to Abrasion.*—(*a*) What tests are desirable and practicable to determine the resistance of a cement-concrete pipe to abrasion?

(*b*) What scales or standards of resistance will best satisfy the various demands of durability? (High and low velocities, presence and absence of grit, etc.)

5. *Resistance to Rupture.*

(*a*) *External Forces.*—1. What tests are advisable and practicable to determine the resistance of a cement-concrete pipe to rupture by external forces produced under various conditions of use?

2. What scales or standards of resistance will best satisfy the various demands of strength and serviceability?

(*b*) *Internal Forces.*—1. What tests are advisable and practicable to determine the resistance of a cement-concrete pipe to rupture by the internal forces produced under varying conditions of use? (Inverted siphons, etc.)

2. What scales or standards of resistance will best satisfy the various demands of strength under different pressures?

6. *Resistance to Disintegration.*

(*a*) *By Acids.*—What tests are advisable and practicable to determine the resistance of a cement-concrete pipe to disintegration by acids occurring in sewage?

(*b*) *By Alkalies.*—What tests are advisable and practicable to determine the resistance of a cement-concrete pipe to disintegration by alkalies occurring in sewage?

(*c*) *By Steam.*—What tests are advisable and practicable to determine the resistance of a cement-concrete pipe to disintegration by steam when discharged into sewers?

(*d*) *By Frost.*—What tests are advisable and practicable to determine the resistance of a cement-concrete pipe to disintegration by frost?

7. *Artificial Linings and Coatings.*—(*a*) What tests are advisable and practicable to determine the suitability of linings in cement-concrete pipes to satisfy the demands of durability and serviceability?

(*b*) What tests are advisable and practicable to determine the suitability of coatings in cement-concrete pipes to satisfy the demands of durability and serviceability?

D. GENERAL TESTS FOR CLAY AND CEMENT-CONCRETE PIPES.

To what extent and under what circumstances should any or all of the following tests be applied?

1. *Porosity and Specific Gravity.*—What tests, if any, are advisable and practicable to determine the character and degree of porosity on both clay and cement-concrete pipes?

2. *Resistance to Abrasion.*—What tests, if any, can be uniformly applied to both clay and cement-concrete pipes to determine their resistance to abrasion?

3. *Toughness.*—What tests, if any, can be uniformly applied to both clay and cement-concrete pipes to determine the degree and character of toughness which will best satisfy the demands of strength and serviceability?

4. *Resistance to Crushing.*—What tests, if any, can be uniformly applied to both clay and cement-concrete pipes to determine their resistance to crushing in compliance with the various demands for strength?

5. *Resistance to Bursting.*—What tests, if any, can be uniformly applied to both clay and cement-concrete pipes to determine their resistance to bursting in compliance with the demands for strength?

6. *Resistance of Branches, Spurs, etc., to Rupture.*—What tests, if any, can be uniformly applied to branches, spurs, etc., on clay and cement-concrete pipes to determine their resistance to rupture in compliance with the demands for strength?

REPORT OF COMMITTEE D-1 ON PRESERVATIVE COATINGS FOR STRUCTURAL MATERIALS.

In accordance with the policy stated in last year's report, the work that has been accomplished since that time will be presented in detail by the chairmen of the various sub-committees.

The report of Sub-Committee B, appointed to inspect the paint test on the Pennsylvania Railroad bridge over the Susquehanna River at Havre de Grace, contains an averaged statement of the condition of the paint on the panels and bridge. It was the intention to include photographs of these panels to show the condition of the metal under the paint after six years' exposure, but the photographs made do not show this point clearly. If possible another series of photographs will be made, as it is desired that a permanent record of the condition of these panels shall be obtained.

It was not possible for Sub-Committee D, appointed to cooperate with a sub-committee of Committee A-5, to inspect the steel plates at Atlantic City in time to present their report at this meeting, but the inspection has been made and the result will appear in the Proceedings for this year.*

Sub-Committee E on Linseed Oil has continued the investigation reported in the Proceedings of the Society in 1909. The analyses of four samples of oil obtained at that time and held sealed in air-tight containers have been repeated, and twelve additional samples representing the products crushed from last year's crop have also been examined. The re-tests of the original samples indicate that no appreciable change has occurred after nearly two years' storage. The analyses were made in seven different laboratories and all results carefully checked, so it is believed that the average of these results taken with the results previously reported can be considered as furnishing correct constants for raw linseed oil made from domestic seed. The report includes a carefully conducted investigation by the Bureau of Standards on these oils to determine the coefficient of expansion of linseed oil between the temperatures of 4° and 40° C. This determination will be of much value in correcting the specific gravity of linseed oils for variation in temperature.

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*See pp. 192-194.—Ed.

Sub-Committee F, appointed to frame definitions of terms used in paint specifications, has defined certain of the terms used and presents them in a tentative form subject to further revision and modification.

Sub-Committee J on the Testing of White Paints has developed the details of a comprehensive exposure test of white pigments. Some 120 pigment formulas have been designed from 13 single pigments in primary, binary, ternary, and quaternary combinations with a standard oil and drier, so as to show, if possible, the effect of the progressive increase in these pigments. The plan includes the details of a method to determine and obtain a uniform consistency of the paints and a design for the panels and fence for exposing them.

The pigments and oil for this test have been donated by paint manufacturers. The grinding and mixing of the paints and the preparation of the drier has been undertaken by Professor Allen Rogers of Pratt Institute of Brooklyn. The Pennsylvania Railroad Company will prepare the panels, and the Baltimore and Ohio Railroad Company will apply the paints. It is expected that the Departments of Agriculture and of Commerce and Labor will provide for the exposure of these panels at the experimental farm near Arlington, Va. It is believed that the details of the test have been so determined and specified that the final result will add materially to the general knowledge on this problem.

Sub-Committee C on Paint Vehicles* has made a promising start in this investigation. The program as planned includes laboratory tests on the time of drying and the physical character of films of various drying, semi-drying, and non-drying oils to which fixed and varying amounts of a standard and different types of driers have been added. These tests are to be applied to the oils and driers with and without pigments. In addition to these laboratory tests, exposure tests are to be made under service conditions, using a standard pigment but varying the vehicle as outlined above.

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*The Sub-Committee on Paint Vehicles was appointed during the past year, and was given the designation "C", after the discharge of former Sub-Committee C on Inspection of the Wooden Panels at Atlantic City on completion of its work in 1910.

The Committee has had this investigation in mind ever since its organization, as it is believed that the vehicle is fully as important as the pigment in this type of protective coatings, but it was necessary to test out one set of variables at a time. The investigation on pigments seems to have progressed far enough, however, to warrant at least a start on this coordinate branch of the subject.

The investigations of the other sub-committees have been continued during the past year.

Respectfully submitted on behalf of the Committee,

S. S. VOORHEES,
Chairman.

G. W. THOMPSON,
Secretary.

REPORT OF SUB-COMMITTEE B ON INSPECTION OF THE HAVRE DE GRACE BRIDGE.

The 1911 spring inspection of the Havre de Grace Bridge was called for April 21 and 22, under the following program: The first day to be devoted to an examination of the plate panels, when it was to be determined which of these were to be removed for photographing before and after the removal of the coatings; the second day to be devoted to a thorough inspection of the bridge proper. Rain prevented the carrying out of the second day's program as *originally* planned. The schedules of plate examinations are as follows:

A	Chalking.
B	Checking, cracking, alligatoring, etc.
C	General surface conditions.

with the following marking for each:

Excellent	10 to 8
Good	8 to 6
Fair	6 to 4
Poor	4 to 2
Failure	2 to 0

The results of the panel inspection of April 21 are given in the accompanying Table of Individual Ratings, which contains the averages of all nine panels, covering three rates of application.

It is of record that the examinations oftentimes showed marked variation in the condition of the protective coverings under the different rates of application, and it may be assumed that this clearly proves that each paint has what may be styled its "natural covering rate," and that the panel rate approximating thereto generally showed the best. This rate in the majority of cases was 600 sq. ft. per gallon.

REVIEW.

Under A (chalking), the average rating of 14 paints was 8.7, fairly excellent; of 4 paints 7.4, fairly good; of 1 paint 4.5, only fair.

TABLE OF INDIVIDUAL RATINGS.

* Paint.	Examined for	Aiken.	McNaughton.	Sabin.	Chapman.	Polk.	Gibboney.	Average, 1911.	Remarks.	Average, 1910.
1	A	8.0	7.0	9.0	8.0	10.0	7.0	8.0	Very good	8.4
	B	10.0	10.0	10.0	9.5	10.0	10.0	10.0	Excellent	10.0
	C	7.0	7.0	8.7	9.0	4.0	6.4	7.0	Good	6.6
2	A	8.0	10.0	9.0	7.0	8.5	6.6	8.2	Very good	8.5
	B	6.0	6.0	8.3	4.0	6.1	8.7	6.5	Fairly good	8.2
	C	6.5	4.7	5.0	8.0	5.0	6.0	5.7	Fair	5.3
3	A	8.0	8.0	8.5	8.0	8.0	7.0	7.6	Quite good	7.9
	B	10.0	10.0	9.7	10.0	10.0	9.0	9.8	Excellent	10.0
	C	8.0	8.0	7.2	8.0	7.3	6.5	7.5	Quite good	7.6
4	A	8.0	10.0	9.5	8.0	9.0	8.8	8.9	Fairly excellent	7.8
	B	10.0	10.0	9.8	10.0	10.0	8.8	9.8	Excellent	9.8
	C	7.0	8.7	6.8	8.0	7.7	7.4	7.6	Quite good	8.8
5	A	9.0	10.0	9.5	5.0	8.5	9.0	8.5	Fairly excellent	9.6
	B	10.0	10.0	10.0	10.0	10.0	10.0	10.0	Excellent	10.0
	C	9.0	9.7	8.0	9.0	8.3	8.1	8.3	Very good	8.7
6	A	9.0	10.0	9.0	5.0	8.3	10.0	8.6	Fairly excellent	9.0
	B	10.0	10.0	10.0	10.0	10.0	10.0	10.0	Excellent	10.0
	C	9.0	10.0	8.5	9.5	8.8	9.0	9.1	Fairly excellent	9.0
7	A	8.0	10.0	8.0	7.0	8.2	7.9	8.2	Very good	8.8
	B	10.0	10.0	10.0	10.0	10.0	8.7	9.8	Excellent	10.0
	C	8.0	9.0	7.7	8.0	7.0	7.8	7.9	Very good	8.7
8	A	9.0	10.0	9.0	8.0	9.0	9.8	9.1	Fairly excellent	9.2
	B	10.0	10.0	10.0	10.0	10.0	9.0	9.8	Excellent	10.0
	C	8.0	9.0	7.5	8.0	7.6	8.0	8.0	Very good	9.0
9	A	8.0	8.0	9.0	5.0	7.5	9.0	7.8	Very good	9.2
	B	10.0	10.0	10.0	10.0	10.0	9.0	9.8	Excellent	10.0
	C	8.8	8.8	7.0	8.0	7.0	8.3	7.9	Very good	8.6
10	A	8.0	6.0	8.5	7.0	7.6	7.0	7.3	Good	8.4
	B	6.0	7.0	8.0	6.0	6.8	7.0	6.8	Fairly good	8.0
	C	10.0	10.0	9.0	6.0	8.6	10.0	8.9	Very good	9.2
11	A	7.0	6.0	8.5	6.0	7.0	8.0	7.1	Good	8.2
	B	9.0	8.0	9.7	7.0	8.4	7.3	8.3	Very good	8.4
	C	10.0	10.0	9.0	5.0	8.3	9.8	8.7	Very good	8.8
12	A	9.0	10.0	9.5	9.0	9.4	9.0	9.3	Fairly excellent	9.2
	B	10.0	10.0	10.0	10.0	10.0	9.0	9.8	Excellent	10.0
	C	9.0	10.0	8.3	9.0	7.3	8.8	8.7	Very good	9.4
13	A	10.0	10.0	10.0	9.0	9.7	7.9	9.4	Fairly excellent	9.0
	B	10.0	10.0	10.0	10.0	10.0	8.9	9.8	Excellent	10.0
	C	8.0	7.7	7.0	8.0	7.3	7.2	7.5	Quite good	8.1
14	A	9.0	10.0	9.0	8.0	9.0	9.0	9.0	Fairly excellent	8.8
	B	10.0	10.0	10.0	10.0	10.0	9.0	9.8	Excellent	9.8
	C	8.0	10.0	9.0	8.5	7.3	9.8	8.8	Very good	8.6
15	A	3.0	5.0	7.0	6.0	3.8	2.0	4.5	Barely fair	3.6
	B	2.0	0.0	5.0	2.0	2.2	1.8	2.2	Failed	3.5
	C	2.0	2.0	3.5	5.0	0.0	1.3	2.3	Failed	3.6
16	A	9.0	10.0	9.0	8.0	9.0	9.0	9.0	Fairly excellent	9.0
	B	10.0	10.0	10.0	10.0	10.0	9.8	10.0	Excellent	10.0
	C	8.0	8.0	8.8	8.0	8.5	9.1	8.4	Fairly excellent	9.2
17	A	9.0	10.0	8.5	8.0	8.9	8.8	8.9	Fairly excellent	9.2
	B	10.0	10.0	9.7	10.0	9.9	9.0	9.8	Excellent	8.8
	C	8.0	7.3	7.8	7.0	8.6	6.9	7.6	Good	7.9
18	A	9.0	10.0	9.0	8.0	9.0	8.8	9.0	Fairly excellent	9.4
	B	10.0	10.0	10.0	10.0	10.0	9.0	9.8	Excellent	9.9
	C	9.0	8.0	9.0	8.5	8.6	9.1	8.7	Very good	9.2
19	A	8.0	10.0	8.5	8.0	8.8	8.0	8.6	Very good	9.2
	B	10.0	9.0	10.0	10.0	9.7	9.0	9.6	Excellent	9.3
	C	8.0	6.0	7.5	8.0	8.0	6.2	7.3	Good	8.8

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* The compositions of these paints are given in the *Proceedings*, Vol. VIII, 1908, pp. 173–178.

Under B (checking, etc.), the average rating of 16 paints was 9.7, nearly excellent; of 2 paints 6.6, more than fair; of 1 paint 2.2, failed.

Under C (general conditions), the average rating of 9 paints was 8.6, very good; of 8 paints 7.5, nearly good; of 1 paint 5.7, very fair; of 1 paint 2.3, failed.

For comparison the average results under the same scale of rating for the 1910 report are listed. Only two of the individual inspectors this year are the same as last year, so that additional interest is given to this comparison of averages. The results generally show intelligent differentiation, with some deterioration in the majority of cases. It is to be noted that one individual report, that of Mr. Chapman, a member of Committee D-1, though not of the actual official sub-committee of inspection, shows wider variation in marking, due very probably to the fact that this was his first official examination and naturally he was not as conversant with the scale used as the other members of the party.

It was determined to remove the average "general condition" plate of the three panels at the 900 sq. ft. per gallon rate, for each paint, for photographic examination. This was done under the supervision of the Chairman of the Inspection Committee on May 2, at which time a thorough inspection of the bridge proper was made by Mr. W. A. Aiken, assisted by Mr. Anderson Polk. The panels were carefully packed and have been shipped to New York.

The inspection of the bridge proper, referred to above, resulted in the following general conclusions in practical agreement with that reported last year as the average of several individual conclusions, though from a much less detailed inspection of the bridge proper at that time. It is undoubtedly a fact that generally the bridge structure is to-day in as good, and in the majority of cases better condition than similar panels. While there may be shades of difference in the protection afforded, it would be difficult to draw these sharply at this time except in the cases mentioned below.

Paint 1, while meeting very fully the tests for chalking, cracking, etc., is of markedly poor character, in some respects more pronounced than any. What protection is being afforded—and this is better than on the panels, which average only good—is being furnished by the under-coat.

Paint 2 is furnishing much better protection on the structure than on the panels, contrary to the previous indications of early disintegration as originally reported in 1908. This is undoubtedly due to the fact that the superintendent of tests, in his original report at the time of undertaking these investigations, marked the natural spreading rate of this paint to be 600 sq. ft. per gallon, and it is this rate which shows best in the panels, although it is not as good as on the bridge proper.

Paint 4 is exceptional in not furnishing as good protection to the bridge proper as to the panels.

Paint 10 shows better protection on the bridge proper than on the panels, even though these latter are rated very good. The paint, however, shows a marked tendency to decay and is likely to fail in the near future.

Paint 14, on the bridge proper, while showing minute cracking of the outer coat, as noted three years ago, disclosing the undercoat of red lead prominently throughout the structure, indicates failure to be superficial, more apparent than real, and to-day affords good protection, better in fact than in many of the paints which lack any evidence of cracking, checking or alligatoring.

Paint 15, on the bridge proper, except in numerous well-defined areas showing very badly throughout, is in better general condition than on the panels, which are all in very poor condition and are marked as having failed. The panels as a whole are typical in their condition of the well-defined areas referred to above, scattered throughout the structure proper.

With the above special exceptions, all paints are affording excellent protection on the bridge proper. It was noted that generally on the horizontal struts and laterals and their connections, affording the easier accumulation of moisture, most of the paints show more or less deterioration. This only emphasizes the necessity for special care in painting structures. The light latticing of the main riveted members shows this tendency more or less in the majority of cases, most markedly on the up-stream side of the structure.

The photographing of the typical plates was begun in Brooklyn, N. Y., on May 25, in the presence of the Chairman and Mr. A. H. Sabin. The removal of the coatings was subsequently accomplished. In all cases the condition of the plate material was found to fully corroborate the ratings given the several paints, as generally at this date affording excellent protection.

Respectfully submitted on behalf of the Sub-Committee,

W. A. AIKEN,
Chairman, Sub-Committee B.

REPORT OF SUB-COMMITTEE C ON PAINT VEHICLES.

At the January meeting of Committee D-1 a sub-committee consisting of the following members was appointed to investigate paint vehicles:

G. B. Heckel,
Glenn H. Pickard,
Allen Rogers,
A. H. Sabin,
H. A. Gardner, *Chairman.*

At a subsequent meeting of the sub-committee it was determined to start the investigations with a series of tests on certain drying, semi-drying, and non-drying oils, determining their drying values and rate of oxygen absorption, etc., when spread out in thin films. A quantity of the following oils was selected for the tests and subsequently secured from sources known to be reliable:

Lead and manganese linoleate drier.*
Boiled linseed oil (resinate type).
Boiled linseed oil (linoleate type).
Lithographic linseed oil.
Blown linseed oil (containing drier while being blown).
Heavy mineral oil.
Soya bean oil.
Rosin oil.
Cottonseed oil.
Corn oil.
Sunflower oil.
Menhaden oil.
Chinese wood oil, raw.
Chinese wood oil, treated.
Perilla oil.†
Lumbang oil.†
Dry rosin 20 per cent., boiled in 80 per cent. linseed oil.

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* The drier used, upon analysis, showed the presence of 4.36 per cent. PbO and 2.51 per cent. MnO_2.

† The lumbang and perilla oils were imported and arrived subsequent to the starting of the tests. They were therefore not included in the tests.

Four-ounce sample bottles of each oil were sent to the Committee members, with the request to proceed with the tests along the lines agreed upon at the Committee meeting. The instructions for making these tests are outlined as follows:

(*a*) A series of small glass plates, approximately 5 by 7 ins., are to be prepared by each member of the Committee. These plates are to be thoroughly cleaned, carefully numbered, and weighed upon a chemical balance. The oils to be used for the tests are to be numbered corresponding to the plates. A test of each oil is to be made by painting it upon the surface of a glass plate with a camel's hair brush, subsequently weighing the plate and the oil. These tests are to be exposed under constant conditions of temperature, if possible, for three weeks' time, making weighings of each plate every day for six days and then every other day for twelve days.

(*b*) Another series of tests shall be made, in which 80 per cent. of raw linseed oil is to be combined with each of the above-named oils. Previous to making any of the tests, *there should be added to each oil or to each combination, 5 per cent. of a drier containing lead and manganese.* The drier to be used is of the standard grade submitted together with the oil samples. The results of the tests showing the increase by oxygen absorption, etc., are to be charted and submitted at the end of the tests, so that they may be compared with the results obtained by each member of the Committee.

(*c*) If possible, the oils and mixtures of oils used in the above tests are to be ground with pure silica and painted out upon sized paper, three-coat work, the films to be stripped and tested for strength upon a paint filmometer, at two periods two months apart.

The drying of oils to a firm surface when spread in a thin layer is accompanied by an increase in weight, due to oxidation, etc. The percentage of oxygen absorbed often affords a criterion of the drying of the oil under examination, and this factor, together with data regarding the appearance of the oil film, should be taken into consideration when judging the value of an oil or oil mixture. Conditions of light, air, temperature, etc., often cause great variations in the drying of oils and the percentage of oxygen absorbed, etc., as shown by the results obtained in the following tests. Although it was impossible in these tests to have the conditions under which each experimenter worked parallel in nature, the tests afford nevertheless considerable information for guiding future work of a similar nature.

An examination of the results obtained, which are given in Tables I to XV, following this report (pages 184–191), showed generally that the greatest increase in weight occurred during the period in which the oil dried to a firm film. This occurred in most cases within 48 hours. After this period, a slight increase in weight was often noticed and then a more or less steady decline, varying with the oil examined. Had the oil tests been continued for a greater length of time, a much greater loss might have been observed.

It was impossible to include in the tests the oil-silica film work, on account of lack of time. It is believed, however, that these tests should be conducted, as they would throw much light on the elasticity and strength given to paint films by various oils.

Respectfully submitted on behalf of the Sub-Committee,

H. A. GARDNER,
Chairman, Sub-Committee C.

TABLE I.—(*a*) BOILED LINSEED OIL (RESINATE TYPE), 100 PER CENT.

Observer	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner	0.1997	11.9	12.5	12.7	13.1	12.8	12.7	.....	12.7	.....	12.6	.....	12.8	.....	12.8	.....	12.7	.....	12.9	.....	Dried to firm, smooth film in 2 days.
Sabin	0.6242	14.42	13.37	12.53	11.79	11.03	.....	10.17	10.34	10.12	10.09	.....	9.69	.....	.....	9.04	.....	8.63	.....	8.13	
Pickard	0.5027	10.21	10.00	9.57	9.65	8.99	.....	8.57	.....	8.93	.....	8.81	.....	9.31	.....	9.43	.....	.....	9.11	.....	
Rogers. North.	0.6024	13.69	13.01	12.50	12.29	12.00	12.25	.....	11.64	.....	10.73	.....	10.68	.....	11.18	.....	10.68	.....	.....	.....	Tacky at end of 1st day. Nearly dry, end of 2d day. Perfectly dry, end of 10th day.

(*b*) BOILED LINSEED OIL (RESINATE TYPE), 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner	0.1933	13.6	14.7	14.9	14.9	14.8	14.8	.....	14.8	.....	14.8	.....	14.7	.....	14.5	.....	14.7	.....	14.7	.....	Dried to firm, smooth film in 2 days.
Sabin	0.3660	0.57	1.66	10.50	13.30	.....	.....	12.51	.....	11.40	.....	.....	10.20	.....	.....	9.84	.....	.....	.....	.....	
Pickard	0.4640	12.45	11.92	11.49	11.10	10.84	.....	9.48	.....	7.41	.....	7.56	.....	8.36	.....	8.54	.....	.....	8.51	.....	
Rogers. North.	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	

TABLE II.—(*a*) BOILED LINSEED OIL (LINOLEATE TYPE), 100 PER CENT.

Gardner	0.1226	10.9	12.2	12.7	12.5	12.8	12.2	.....	12.2	.....	12.4	.....	12.1	.....	12.1	.....	12.1	.....	12.1	.....	Dried firmly with smooth, even film in 2 days.
Sabin	0.5384	14.34	13.26	12.18	11.29	10.75	.....	9.88	10.25	10.01	9.91	.....	9.60	.....	.....	9.12	.....	8.37	.....	8.30	
Pickard	0.5096	10.25	10.41	10.22	10.16	9.90	.....	9.60	.....	9.72	.....	9.48	.....	9.97	.....	10.36	.....	.....	9.59	.....	
Rogers. North.	0.3306	12.09	11.33	10.94	11.10	10.86	11.25	.....	10.87	.....	9.72	.....	10.02	.....	10.62	.....	10.46	.....	.....	.....	Tacky at end of 1st day. Slightly tacky, end 2d day. Dry, but curled, end of 10th day.

(*b*) BOILED LINSEED OIL (LINOLEATE TYPE), 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner	0.1843	11.8	13.9	15.1	15.2	15.0	14.6	.....	14.6	.....	14.5	.....	14.4	.....	14.4	.....	14.6	.....	14.7	.....	Dried with smooth film in 2 days.
Sabin	0.5790	10.14	15.71	13.29	12.12	11.43	.....	10.05	10.26	9.55	9.32	.....	8.84	.....	.....	8.46	.....	7.68	.....	7.55	
Pickard	0.4653	12.40	11.90	11.50	11.11	10.90	.....	9.37	.....	8.53	.....	7.48	.....	8.43	.....	8.02	.....	.....	7.27	.....	
Rogers. North.	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	

TABLE III.—(*a*) LITHOGRAPHIC LINSEED OIL, 100 PER CENT.

Observer.	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner.	0.4011	6.9	8.5	8.9	8.9	8.7	8.6	.....	8.6	.....	8.6	.....	8.6	.....	8.4	.....	8.4	.....	8.3	.....	Dried to glossy, firm film, slightly crinkled, in 2 days. Oil made very thick film on account of heavy body.
Sabin...	0.8733	0.87	3.85	5.14	6.07	6.40	.....	6.84	7.22	7.36	7.57	.....	7.75	.....	.....	7.98	.....	7.83	.....	7.80	
Pickard..	0.8812	3.60	5.19	5.99	6.78	6.97	.....	7.38	.....	7.42	.....	7.44	.....	8.01	.....	8.03	.....	.....	7.99	.....	
Rogers. } North.. }	2.7318	0.051	0.051	0.051	0.041	0.081	0.169	.....	0.19	.....	0.752	.....	1.184	.....	1.641	.....	2.00	.....	.....	.....	Remained sticky to 10 days, and even at end of 38 days was slightly tacky.

(*b*) LITHOGRAPHIC LINSEED OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Observer.	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner.	0.1300	10.2	11.3	11.9	12.0	11.8	11.8	.....	11.8	.....	11.8	.....	11.6	.....	11.8	.....	11.9	.....	11.9	.....	Dried to firm, glossy film in 2 days.
Sabin...	0.7750	11.35	11.48	10.93	10.77	10.25	.....	9.51	9.93	9.80	9.68	.....	9.65	.....	.....	9.51	.....	9.07	.....	8.67	
Pickard..	0.6538	9.94	10.41	10.39	10.35	9.93	.....	9.54	.....	9.36	.....	8.99	.....	9.61	.....	9.70	.....	.....	9.13	.....	
Rogers. } North.. }	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	

TABLE IV.—(*a*) BLOWN LINSEED OIL, 100 PER CENT.

Observer.	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner.	0.2105	8.5	10.2	10.2	10.2	10.0	9.9	.....	9.8	.....	9.8	.....	9.7	.....	9.8	.....	9.8	.....	9.9	.....	Ropiness of oil made very thick film, but dried in less than 2 days to smooth film. Films exhibited ridges.
Sabin...	0.8394	9.30	8.97	5.30	9.30	8.99	.....	8.49	8.89	8.73	8.89	.....	8.73	.....	.....	8.52	.....	8.07	.....	7.74	
Pickard..	0.8457	5.07	6.16	6.48	6.94	6.73	.....	6.99	.....	6.89	.....	7.11	.....	7.60	.....	7.95	.....	.....	7.86	.....	
Rogers. } North.. }	1.0398	4.41	4.91	5.22	5.62	5.73	6.06	.....	6.43	.....	6.18	.....	6.51	.....	6.95	.....	7.00	.....	.....	.....	Formed skin, end 1st day. Slightly tacky, end 2d day: dry, but curled, end 10th day.

(*b*) BLOWN LINSEED OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Observer.	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner.	0.0774	10.4	12.8	13.1	12.9	12.1	11.9	.....	12.0	.....	11.8	.....	11.7	.....	11.6	.....	11.6	.....	11.8	.....	Dried up to very glossy film in 2 days.
Sabin...	0.5329	11.82	12.76	0.98	10.39	9.81	.....	8.69	9.15	8.91	8.97	.....	8.67	.....	.....	8.22	.....	7.63	.....	7.32	
Pickard..	0.6218	10.71	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Glass broke.
Rogers. } North.. }	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	

TABLE V.—(*a*) MINERAL OIL, 100 PER CENT.*

Observer	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner	0.1632	12.5 (—)	14.2 (—)	16.7 (—)	19.4 (—)	19.4 (—)	19.5 (—)	.....	19.5 (—)	.....	19.5 (—)	.....	19.3 (—)	.....	19.4 (—)	.....	19.5 (—)	.....	19.5 (—)	.....	Oil lost in weight throughout test on account of presence of volatiles. No drying action observed. Film wet at end of test.
Sabin	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Broken before weighings were made.
Pickard	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	
Rogers North	0.1975	8.12 (—)	16.22 (—)	21.23 (—)	25.58 (—)	28.41 (—)	28.92 (—)	.....	35.25 (—)	.....	35.76 (—)	.....	43.86 (—)	.....	45.28 (—)	.....	48.08 (—)	.....	.....	.....	Remained oily during entire test.

* Lost in weight throughout test.

(*b*) MINERAL OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Observer	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner	0.1884	6.4	6.8	7.2	7.8	8.1	7.9	.....	7.9	.....	8.1	.....	7.8	.....	7.8	.....	7.8	.....	7.8	.....	Fair drying observed end of 2d day. Film tacky until end 8th day; after that, fairly firm film shown.
Sabin	0.5663	11.51	8.21	6.51	5.19	4.36	.....	2.72	3.12	2.82	2.59	.....	2.35	.....	.....	1.36	.....	0.53	.....	0.14 (—)	
Pickard	0.4050	9.66	8.92	6.82	6.03	4.68	.....	2.64	.....	0.30 (—)	.....	0.56 (—)	.....	0.04 (—)	.....	0.14 (—)	.....	.....	0.86 (—)	.....	
Rogers North	0.2598	6.69 (—)	5.06	2.88	1.52	1.29	1.68	.....	2.07	.....	0.08	.....	0.93 (—)	.....	0.54 (—)	.....	.....	.....	.....	.....	Sticky, end of 1st day; tacky, end of 2d day and end of 38 days.

TABLE VI.—(*a*) SOYA BEAN OIL, 100 PER CENT.

Observer.	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner.	0.1377	7.5	8.4	9.5	12.8	12.9	12.7	.....	12.6	.....	12.5	.....	12.4	.....	12.3	.....	12.3	.....	12.3	.....	Film tacky until 3d day. Clear and fairly firm after 4th day.
Sabin...	0.3972	9.79	9.69	8.56	7.60	7.09	.....	6.00	6.22	6.00	5.54	.....	5.36	.....	.....	4.73	.....	4.23	.....	3.70	
Pickard..	0.4366	9.87	9.87	9.35	8.66	8.13	.....	6.44	.....	4.88	.....	4.26	.....	4.99	.....	4.94	.....	.....	4.94	.....	
Rogers. North..	0.3564	8.25	7.58	7.02	6.74	6.46	6.74	.....	6.46	.....	5.40	.....	5.59	.....	5.80	.....	5.67	.....	.....	.....	Sticky, end of 1st day; tacky, end of 2d day; slightly tacky, end of 10th and 38th days.

(*b*) SOYA BEAN OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner.	0.2218	11.5	11.8	12.5	13.9	14.0	14.0	.....	14.1	.....	14.1	.....	13.8	.....	13.6	.....	13.6	.....	13.6	.....	Clear, firm film observed at end of 2d day.
Sabin...	0.2877	12.78	12.78	11.74	11.23	10.60	.....	9.35	10.08	9.76	9.59	.....	9.59	.....	.....	9.00	.....	8.09	.....	8.00	
Pickard..	0.4581	13.16	12.64	11.84	11.50	11.01	.....	9.15	.....	7.29	.....	6.61	.....	7.43	.....	6.96	.....	.....	6.66	.....	
Rogers. North..	0.2249	11.74	12.27	10.38	9.43	9.66	9.75	.....	10.29	.....	9.08	.....	8.18	.....	8.95	.....	.....	.....	.....	.....	Tacky at end of 1st and 2d days. Dry, end 10th day.

TABLE VII.—(*a*) ROSIN OIL, 100 PER CENT.

Gardner.	0.2590	1.5	1.5	1.8	3.0	5.2	4.9	.....	4.8	.....	4.8	.....	4.8	.....	4.8	.....	4.8	.....	4.8	.....	Tacky throughout test.
Sabin...	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	
Pickard..	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	Too much on. Showed constantly increasing loss owing to fact that it did not dry, and ran off glass.
Rogers. North..	0.4822	2.24	2.53	2.32	1.27	1.06	0.66	.....	0.24	.....	0.78	.....	0.68	.....	0.41	.....	0.39	.....	.....	.....	Oily on 1st and 2d days. Tacky, end of 10th and 38th days.

(*b*) ROSIN OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner.	0.1636	7.4	7.8	8.5	8.5	8.4	8.1	.....	8.0	.....	8.0	.....	8.0	.....	7.9	.....	7.9	.....	8.2	.....	Film dried up nicely during 3d day, but remained slightly soft.
Sabin...	0.7105	6.64	6.40	6.05	5.63	5.23	.....	4.42	4.92	4.83	4.57	.....	4.68	.....	.....	4.13	.....	3.81	.....	3.43	
Pickard..	0.4016	12.21	11.45	11.13	10.53	10.13	.....	8.81	.....	8.12	.....	7.45	.....	8.27	.....	8.52	.....	.....	8.62	.....	
Rogers. North..	0.3263	11.48	12.02	10.60	10.26	10.42	10.42	.....	10.95	.....	9.96	.....	9.53	.....	9.96	.....	.....	.....	.....	.....	Oily at end of 1st and 2d days. Slightly tacky, end of 10th day.

TABLE VIII.—(*a*) COTTON SEED OIL, 100 PER CENT.

Observer.	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner	[illegible]	4.5	4.8	4.8	5.1	8.6	8.7	.....	8.1	.....	7.9	.....	8.0	.....	8.0	.....	8.1	.....	8.0	.....	Film showed very little hardening and remained soft and tacky.
Sabin	0.7247	8.03	7.48	6.68	6.00	5.65	.....	4.85	5.09	4.95	4.80	.....	.....	.....	.....	.....	.....	.....	.....	.....	
Pickard	0.4153	7.04	7.16	6.62	6.24	5.78	.....	3.72	.....	2.08	.....	1.72	.....	2.52	.....	2.35	.....	.....	2.32	.....	
Rogers, North	0.3583	6.67	5.61	4.85	4.65	4.37	4.71	.....	4.57	.....	2.07	.....	3.11	.....	3.39	.....	3.30	.....	.....	.....	Tacky, end 2d day. Slightly tacky, end 10th and 38th days.

(*b*) COTTON SEED OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Observer.	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner	0.1516	8.5	8.7	9.1	10.8	11.9	11.8	.....	11.9	.....	11.9	.....	11.8	.....	11.8	.....	11.8	.....	10.7	.....	Fair drying observed at end of 4th day. Film slightly tacky at end of test.
Sabin	0.[illegible]498	11.00	11.15	10.58	10.17	9.82	.....	9.02	9.42	9.35	9.27	.....	9.32	.....	.....	8.81	.....	8.24	.....	7.92	
Pickard	0.6160	10.94	10.81	10.51	10.37	9.87	.....	8.93	.....	8.90	.....	8.70	.....	9.29	.....	9.63	.....	.....	8.47	.....	
Rogers, North	0.2553	11.83	11.83	10.15	9.29	9.29	9.45	.....	10.00	.....	8.95	.....	8.06	.....	8.61	.....	.....	.....	.....	.....	Tacky on 1st and 2d days. Dry on 10th day.

TABLE IX.—(*a*) CORN OIL, 100 PER CENT.

Observer.	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner	0.0574	1.9	4.2	4.6	4.8	7.5	7.1	.....	7.1	.....	7.1	.....	7.2	.....	7.1	.....	7.0	.....	6.9	.....	Film soft and sticky throughout test. Very soapy in appearance.
Sabin	0.5858	0.22 (—)	7.03	8.79	7.43	7.17	.....	5.85	6.02	5.84	5.58	.....	5.33	.....	.....	4.78	.....	4.15	.....	3.63	
Pickard	0.4981	1.22	5.86	7.27	11.35 (*)	11.35	.....	11.37	.....	6.26	.....	4.97	.....	5.62	.....	5.34	.....	.....	5.34	.....	*Moth got in.
Rogers, North	0.3300	4.63	7.27	7.14	6.99	6.69	6.93	.....	6.84	.....	5.11	.....	5.17	.....	5.38	.....	5.17	.....	.....	.....	

(*b*) CORN OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Observer.	Wt. of Oil for Test, grams.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner	0.1[illegible]4	7.5	8.4	8.6	10.2	10.4	10.6	.....	10.5	.....	10.3	.....	10.3	.....	10.3	.....	10.2	.....	10.0	.....	Film tacky at end of test.
Sabin	0.5460	13.01	12.41	.....	11.13	11.52	.....	11.22	10.98	10.38	9.64	.....	9.07	.....	.....	8.38	.....	8.77	.....	.....	
Pickard	0.3716	13.81	12.92	12.16	11.71	11.11	.....	9.23	.....	8.29	.....	7.24	.....	8.42	.....	8.26	.....	.....	7.94	.....	
Rogers, North	0.1711	11.87	11.69	9.78	8.33	8.50	8.62	.....	9.61	.....	8.16	.....	7.00	.....	8.28	.....	.....	.....	.....	.....	Tacky, end of 1st and 2d days. Dry, end of 10th day.

TABLE X.—(*a*) SUNFLOWER OIL, 100 PER CENT.

Observer.	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner.	0.1414	6.3	8.2	11.5	11.6	11.5	11.5	.....	11.3	.....	11.3	.....	11.3	.....	11.3	.....	11.2	.....	11.0	.....	Film fairly firm, end of 3d day.
Sabin...	0.6292	9.69	9.42	7.99	7.43	7.04	.....	6.12	6.45	6.12	5.92	.....	5.69	.....	.....	5.24	.....	4.57	.....	4.26	
Pickard..	0.5837	7.85	7.73	7.45	7.02	6.36	.....	5.16	.....	4.57	.....	4.20	.....	4.54	.....	4.61	.....	.....	4.30	.....	
Rogers. North..	0.2540	8.39	6.94	6.21	6.13	5.81	6.01	.....	6.09	.....	4.81	.....	4.73	.....	4.81	.....	5.01	.....	.....	.....	Sticky, end 1st day; tacky, end 2d day; slightly tacky, end 10th day.

(*b*) SUNFLOWER OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner.	0.1600	9.5	11.0	11.1	11.3	11.4	10.9	.....	10.8	.....	10.8	.....	10.8	.....	10.6	.....	10.6	.....	10.9	.....	Good firm glossy film shown at end of 2d day.
Sabin...	0.5030	14.21	14.21	12.66	14.01	11.59	.....	10.24	10.63	10.34	10.34	.....	10.27	.....	.....	11.33	.....	10.73	.....	10.30	
Pickard..	0.4470	12.62	12.02	11.48	11.65	10.25	.....	8.14	.....	6.26	.....	5.54	.....	6.22	.....	5.82	.....	.....	5.35	.....	
Rogers. North..	0.2261	11.54	11.85	9.92	9.13	8.95	9.04	.....	9.52	.....	8.55	.....	7.67	.....	8.20	.....	.....	.....	.....	.....	Dry on 1st, 2d and 10th days.

TABLE XI.—(*a*) MENHADEN OIL, 100 PER CENT.

Gardner.	0.1944	7.7	8.1	8.9	10.1	9.8	9.8	.....	9.8	.....	9.8	.....	9.8	.....	9.6	.....	9.6	.....	9.6	.....	Good drying during 2d day. Fairly firm film.
Sabin...	0.5282	12.47	12.17	11.70	11.47	11.13	.....	10.28	11.20	11.15	11.02	.....	11.37	.....	.....	10.85	.....	10.34	.....	9.90	
Pickard..	0.7005	10.79	10.98	10.85	10.99	10.57	.....	9.27	.....	8.48	.....	8.27	.....	8.91	.....	8.75	.....	.....	9.21	.....	
Rogers. North..	0.3150	11.27	10.16	9.72	9.97	9.94	10.27	....	10.36	.....	8.80	.....	9.22	.....	9.40	.....	9.31	.....	.....	.....	Sticky, end 1st day. Slightly tacky, end 2d and 10th days.

(*b*) MENHADEN OIL, 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner.	0.2448	8.5	10.4	12.2	12.9	12.9	12.9	.....	12.9	.....	12.9	.....	12.9	.....	12.8	.....	12.7	.....	12.9	.....	Good firm elastic film shown after 2d day.
Sabin...	0.4959	14.11	13.47	12.68	12.04	11.59	.....	10.44	11.09	11.04	10.74	.....	10.90	.....	.....	10.18	.....	9.48	.....	8.93	
Pickard..	0.4201	13.19	12.88	12.23	11.81	11 17	.....	9.50	.....	8.48	.....	7.77	.....	8.33	.....	8.24	.....	.....	8.12	.....	
Rogers. North..	0.2456	10.99	11.28	9.56	8.90	8 72	8.72	.....	9.34	.....	8.40	.....	7.37	.....	8.11	.....	.....	.....	.....	.....	Nearly dry on 1st and 2d days.

TABLE XII.—(a) CHINESE WOOD OIL (RAW), 100 PER CENT

Observer.	Wt. of Oil for Test, grams.	Percentage Increase in Weight, in — Days.																			Remarks.
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	
Gardner.	0.2266	4.1	11.2	14.9	14.4	14.4	14.2	.....	14.2	.....	14.2	.....	14.2	.....	14.2	.....	14.2	.....	14.5	.....	Film crystallized and remained soft until 3d day. Hard but opaque film shown after 4th day.
Sabin...	0.5545	0.0	0.0	11.02	11.53	11.03	.....	10.53	10.74	10.47	10.27	.....	10.22	.....	.....	9.80	.....	9.25	.....	8.86	
Pickard..	0.4903	0.59	2.09	5.13	7.56	8.68	.....	10.11	.....	9.65	.....	9.43	.....	9.77	.....	9.73	.....	.....	9.33	.....	
Rogers. North..	0.4036	0.54	2.80	5.10	6.00	6.27	7.09	.....	8.39	.....	8.01	.....	8.55	.....	9.13	.....	9.27	.....	.....	.....	Sticky, end of 1st and 2d days; dry but drawn, end of 10th day.

(b) CHINESE WOOD OIL (RAW), 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner.	0.3087	9.0	12.1	12.9	12.8	12.8	12.8	.....	12.7	.....	12.6	.....	12.6	.....	12.5	.....	12.5	.....	12.7	.....	Clear firm film shown after 3d day.
Sabin...	0.2967	14.46	13.11	11.72	10.68	9.77	.....	8.66	8.86	8.80	8.49	.....	8.15	.....	.....	8.05	.....	7.41	.....	7.04	
Pickard..	0.3683	14.37	13.66	13.11	12.41	11.78	.....	10.51	.....	8.72	.....	7.01	.....	8.82	.....	8.39	.....	.....	7.98	.....	
Rogers. North..	0.2285	11.99	11.90	10.14	9.30	9.08	9.30	.....	9.70	.....	8.90	.....	7.34	.....	7.78	.....	.....	.....	.....	.....	Dry at end of 1st day.

TABLE XIII.—(a) CHINESE WOOD OIL (TREATED), 100 PER CENT.*

Gardner.	0.1678	38.0 (—)	30.0 (—)	28.0 (—)	28.0 (—)	28.0 (—)	28.2 (—)	.....	28.0 (—)	.....	27.5 (—)	.....	26.0 (—)	.....	26.0 (—)	.....	26.0 (—)	.....	26.2 (—)	.....	Loss observed due to presence of volatiles. Firm, clear film shown at end of 1st day.
Sabin...	0.4159	19.06 (—)	20.16 (—)	20.47 (—)	20.47 (—)	20.80 (—)	.....	21.09 (—)	20.87 (—)	20.98 (—)	20.78 (—)	.....	20.70 (—)	.....	.....	20.97 (—)	.....	21.22 (—)	.....	21.11 (—)	
Pickard..	0.2934	0.92 (—)	0.41 (—)	0.72	0.79	0.13	.....	0.22	.....	0.46	.....	0.44	.....	0.43	.....	0.42	.....	.....	0.43	.....	
Rogers. North..	0.3937	3.53 (—)	3.58 (—)	3.25 (—)	3.25 (—)	3.33 (—)	2.93 (—)	.....	2.55 (—)	.....	3.40 (—)	.....	3.23 (—)	.....	2.61 (—)	.....	2.48 (—)	.....	.....	.....	Dry at end of 1st day.

* Lost in weight throughout test.

(b) CHINESE WOOD OIL (TREATED), 20 PER CENT.; RAW LINSEED OIL, 80 PER CENT.

Gardner.	0.1638	8.4	9.4	9.8	9.7	9.9	9.9	.....	10.0	.....	9.6	.....	9.5	.....	9.5	.....	9.5	.....	9.6	.....	Clear hard film shown during 2d day.
Sabin...	0.6572	9.25	8.07	7.36	6.75	6.25	.....	5.49	5.87	5.70	5.67	.....	4.37	.....	.....	5.15	.....	4.69	.....	4.17	
Pickard..	0.4892	8.93	8.71	8.44	8.16	7.95	.....	6.75	.....	5.99	.....	5.50	.....	6.40	.....	6.01	.....	.....	5.87	.....	
Rogers. North..	0.2644	3.21	3.48	2.15	1.58	1.50	1.77	.....	2.30	.....	1.62	.....	0.86	.....	1.50	.....	.....	.....	.....	.....	Dry at end of 1st day.

Table XIV.—(*a*) 20 Per Cent. Dry Rosin in 80 Per Cent. Linseed Oil, 100 Per Cent.

Observer.	Wt. of Oil for Test, grains.	Percentage Increase in Weight, in — Days. 1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner.	0.2030	12.0	14.1	14.8	14.2	14.5	14.0	.....	14.1	.....	14.1	.....	14.0	.....	14.0	.....	14.0	.....	14.1	.....	Rapid drying observed. Hard film shown during 2d day.
Sabin...	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	
Pickard..	0.5185	3.76	8.76	9.20	9.20	8.49	.....	9.07	.....	9.01	.....	9.09	.....	10.50	.....	10.16	.....	.....	10.18	.....	
Rogers. North..	0.2554	1.80	11.78	12.17	12.29	12.02	12.49	.....	13.15	.....	11.85	.....	11.78	.....	12.69	.....	12.83	.....	.....	.....	Oily end 1st and 2d days; slightly tacky, end 10th day.

(*b*) 20 Per Cent. Dry Rosin in 80 Per Cent. Linseed Oil, 20 Per Cent.; Raw Linseed Oil, 80 Per Cent.

Observer.	Wt. of Oil for Test, grains.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Gardner.	0.1500	10.9	13.5	13.6	13.0	13.0	13.0	.....	13.1	.....	13.1	.....	13.0	.....	12.9	.....	13.0	.....	13.2	.....	Clear hard film after 2d day.
Sabin...	0.7105	14.19	13.17	11.84	11.46	10.87	.....	9.80	10.33	10.40	10.04	.....	10.35	.....	.....	9.64	.....	8.98	.....	8.62	
Pickard..	0.4568	12.86	12.73	12.13	12.02	11.30	.....	10.95	.....	11.21	.....	10.53	.....	11.21	.....	10.88	.....	.....	11.43	.....	
Rogers. North..	......	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	.....	

Table XV.—(*a*) Raw Linseed Oil, 100 Per Cent.*

Observer.	Wt. of Oil for Test, grains.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Sabin...	0.5274	0.26	0.51	0.11	2.35	9.14	.....	14.48	14.48	14.18	13.86	.....	13.00	.....	.....	12.23	.....	11.66	.....	11.07	
Pickard..	0.5326	12.42	12.39	11.88	11.83	11.08	.....	10.29	.....	9.56	.....	9.85	.....	10.30	.....	10.12	.....	.....	10.78	.....	

* The test of this oil was made without the addition of 5 per cent. of drier, the quantity used in all the other tests.

(*b*) Drier, 100 Per Cent.†

Observer.	Wt. of Oil for Test, grains.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Remarks.
Rogers. North..	0.3445	48.95 (—)	48.53 (—)	48.68 (—)	48.68 (—)	48.48 (—)	48.26 (—)	.....	48.43 (—)	.....	48.89 (—)	.....	48.22 (—)	.....	48.22 (—)	.....	.....	.....	.....	.....	Dry at end of 1st day

† Lost in weight throughout test.

REPORT OF SUB-COMMITTEE D ON THE ATLANTIC CITY STEEL PAINT TESTS.

On Wednesday, June 28, 1911, the second inspection of the Atlantic City Steel Test Panels erected in October, 1908, was made by the Sub-Committee, which had agreed to report upon the condition of the painted sufaces, leaving any report on the comparative corrosion of the various types of metal used in the test to Committee A-5 on the Corrosion of Iron and Steel.

Ratings were given each panel according to the amount of rust apparent on the painted surfaces of the panels, as well as the degree of checking, chalking, scaling, cracking, peeling, loss of color, and other signs of paint failure shown. The system of rating which took into consideration all of the above conditions, was similar to the system used at the first inspection during 1910, when 0 (zero) recorded the worst results and 10 (ten) the best results.

In Table I there is shown the rating accorded by each inspector to each panel, as well as an average for each panel.

In Table II there is shown the rating of those panels which were considered by the Sub-Committee as meriting from 8 to 10 and having given the best all-around service.

For full details of the tests as well as an outline of former reports, the reader is referred to the Report of the First Inspection, Report of Committee A-5, Proceedings, Vol. X, 1910, pp. 73 ff.

Respectfully submitted on behalf of the Sub-Committee,

H. A. Gardner,
Chairman, Sub-Committee D.

TABLE I.—SECOND INSPECTION OF PAINT-TEST STEEL PANELS, ATLANTIC CITY, N. J.

BY SUB-COMMITTEE D OF COMMITTEE D-1.

Panel.	Pigment.	W. H. Walker.	P. H. Walker.	Gardner, *Chairman.*	Chapman	Average.
1	Dutch process white lead	2	3	3	5	3.7
2	Quick process white lead	4	4	3	6	4.2
3	Zinc oxide (xx)	1	1.5	1	2.5	1.5
4	Sublimed white lead	9	9.5	9	8.5	9.0
5	Sublimed blue lead	9	9.5	9.5	7.5	8.8
6	Lithopone	2	1.5	2	3.5	2.2
7	Zinc white lead	3	4	5	7	4.7
9	Orange mineral (American)	9	9	9	6.5	8.3
10	Red lead	9	9	9	6.5	8.3
12	Bright red oxide	8.5	9	8	7	8.1
14	Venetian red	7	9	7	9	8.0
15	Prince metallic brown	5	7.5	6	8	6.3
16	Natural graphite	6	8	4	9.5	6.8
17	Artificial graphite	5	7.5	4	7	5.9
19	Lamp black	5	7.5	5	8	6.3
20	Willow charcoal	9	8.5	9	9	8.8
21	Carbon black (gas)	7	8.5	5	8.5	7.2
24	Yellow ochre (French)	5	7	2	8	5.5
27	Barytes (natural)	1	1	1	0	0.7
28	Barytes (precipitated)	2	1.5	2	2	1.8
29	Calcium carbonate (whiting)	0	0	0	0	0.0
30	Calcium carbonate (precipitated)	0	0	0	0	0.0
31	Calcium sulphate (gypsum)	1	1	2	3	1.7
32	China clay (kaolin)	6	6	7	6.5	6.3
33	Abestine (magnesium silicate)	5	4.5	6	5	5.1
34	American vermilion (chrome scarlet)	10	10	10	10	10.0
36	Lead chromate	7	7.5	8.5	8	7.7
39	Zinc chromate	9	9.5	10	9.5	9.5
40	Zinc and barium chromate	9	9.5	10	9.5	9.5
41	Chrome green (blue tone)	10	10	10	9.5	9.8
44	Prussian blue (W. S.)	9	9.5	8.5	9	9.0
45	Prussian blue (W. 1.)	8	9.5	8	8.5	8.5
48	Ultramarine blue	0	0	0	0	0.0
49	Zinc and lead chromate	10	9.5	10	9.5	9.7
51	Magnetic black oxide	9	9.5	10	9.5	9.5
111	Brown (composite paint)	7	9	9	9	8.5
222	Black (composite paint)	9	9	9	8.5	8.8
333	White (composite paint)	4	4	7	3	4.5
444	Green (composite paint)	5	7	7	8	6.7
555	Black (composite paint)	9	9	6	9	8.2
666	Brown (composite paint)	8	8	6	9	7.7
777	White (composite paint)	7	10	5	7	7.2
888	Green (composite paint)	7	8	8	9	8.0
2000	1 coat zinc chromate, 1 coat iron oxide excluder	8	8.5	8	8	8.1
3000	1 coat lead chromate, 1 coat iron oxide excluder	7	8	7	7.5	7.3
4000	1 coat red lead, 1 coat iron oxide excluder	7	8.5	8	7.5	7.7
100	Straight carbon black paint with turps and drier	5	8.5	4	8.5	6.5
90	Straight lamp black paint with turps and drier	5	7	3	8	5.7
5555	Coal tar paint (over red lead)	4	8	2	7	5.2
1000	Chrome resinate in oil, 1 coat	1	0	0	2	0.7
1 (plate)	Boiled (linseed) oil, 3 coats	1	0	1	4	1.5

TABLE II.—ANALYSIS OF AVERAGES. GRADE OF EXCELLENCE FROM 8 TO 10.

Panel.	Pigment.	Average
34	American vermilion (chrome scarlet)	10.0
41	Chrome green (blue tone)	9.8
49	Zinc and lead chromate	9.7
39	Zinc chromate	9.5
40	Zinc and barium chromate	9.5
51	Magnetic black oxide	9.5
4	Sublimed white lead	9.0
44	Prussian blue (W. S.)	9.0
5	Sublimed blue lead	8.8
20	Willow charcoal	8.8
222	Black (composite paint)	8.8
45	Prussian blue (W. I.)	8.5
111	Brown (composite paint)	8.5
9	Orange mineral (American)	8.3
10	Red lead	3.3
555	Black (composite paint)	8.2
12	Bright red oxide	8.1
2000	1 coat zinc chromate 1 coat iron oxide excluder	8.1
14	Venetian red	8.0
888	Green (composite paint)	8.0

REPORT OF SUB-COMMITTEE E ON LINSEED OIL.

This Sub-Committee reports as follows:

In 1909 four samples of linseed oil were taken under the direction of this Committee, and portions of these samples were re-bottled and sent out for analysis, the results obtained being reported at the meeting of the Society in that year. In addition to the portions which were bottled, two 5-gallon cans of each sample were held in reserve for further work. One can of each sample was bottled under the direction of your chairman for analysis in the early part of this year. These samples had been hermetically sealed for about two years. The purpose of the re-analysis was to discover how far they had changed during this time.

In addition to these four samples (numbered 1 to 4), twelve other samples were taken for analysis. The particulars of the sixteen samples as shown by their labels, etc., are as follows:

Sample No.

1........Sample of Linseed Oil taken at National Lead Co.'s Atlantic Branch Mill, Feb. 2, 1909. Bottled under direction of G. W. Thompson, Feb. 23, 1911. North American seed.

2........Sample of Linseed Oil taken at Hirst & Begley's Chicago Mill, Feb. 3, 1909. Bottled under direction of G. W. Thompson, Feb. 23, 1911. North American seed.

3........Sample of Linseed Oil taken at American Linseed Co.'s South Chicago Mill, Feb. 6, 1909. Bottled under direction of G. W. Thompson, Feb. 23, 1911. North American seed.

4........Sample of Linseed Oil taken at Archer-Daniels Linseed Co.'s Mill at Minneapolis, Feb. 13, 1909. Bottled under direction of G. W. Thompson, Feb. 23, 1911. North American seed.

5........Sample of Linseed Oil taken at American Linseed Co.'s South Chicago Mill, June 15, 1910, by Edw. Gudeman and T. J. Bryan. Bottled under direction of G. W. Thompson, Mar. 1, 1911. North American seed.

6........Sample of Linseed Oil taken at Archer-Daniels Linseed Co.'s Mill at Minneapolis, July 21, 1910, by A. S. Mitchell and F. G. Smith. Bottled under direction of G. W. Thompson March 1, 1911. North American seed.

Sample No.

7........Sample of Linseed Oil taken at Archer-Daniels Linseed Co.'s Mill at Minneapolis, about Sept. 30, 1910, by A. S. Mitchell. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American seed.

8........Sample of Linseed Oil taken at Archer-Daniels Linseed Co.'s Mill at Minneapolis, Oct. 27, 1910, by F. G. Smith. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American seed.

9........Sample of Linseed Oil taken at Archer-Daniels Linseed Co.'s Mill at Minneapolis, about Nov. 30, 1910, by A. S. Mitchell. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American seed.

10........Sample of Linseed Oil taken at Archer-Daniels Linseed Oil Co.'s Mill at Minneapolis, about Dec. 31, 1910 by A. S. Mitchell. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American seed.

11........Sample of Linseed Oil taken at Hirst & Begley's Chicago Mill, Sept. 30, 1910, by H. Roehling. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American—largely Southwestern—seed.

12........Sample of Linseed Oil taken at Hirst & Begley's Chicago Mill about Oct. 31, 1910, by H. Roehling. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American Northwestern seed.

13........Sample of Linseed Oil taken at Hirst & Begley's Chicago Mill, Nov. 29, 1910, by H. Roehling. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American Southwestern seed.

14........Sample of Linseed Oil taken at Hirst & Begley's Chicago Mill, Dec. 29, 1910, by A. L. Winton. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American Northwestern seed.

15........Sample of Linseed Oil taken at Hirst & Begley's Chicago Mill, Jan. 31, 1911, by A. L. Winton. Bottled under direction of G. W. Thompson, Feb. 18, 1911. North American Northwestern seed.

16 ....Sample of Linseed Oil taken at National Lead Co.'s Atlantic Mill, Feb. 24, 1911, by A. H. Sabin. Bottled under direction of G. W. Thompson, Mar. 1, 1911. Argentine seed.

Samples 7 to 15 were taken with the idea of observing the variations in the constants of linseed as affected by their origin and the age of the seed from which they were made.

Sample 16 from Argentine Seed was taken so that a line could be obtained on its constants.

It was decided that the analysis of these samples should be confined to the following determinations:

1. Specific Gravity $\frac{15^\circ.5\ \text{C.}}{15^\circ.5\ \text{C.}}$
2. Specific Gravity $\frac{25^\circ\ \text{C.}}{25^\circ\ \text{C.}}$
3. Acid Number.
4. Saponification Number.
5. Unsaponifiable Matter.
6. Refractive Index at 25° C.
7. Iodine Number (Hanus).

The methods of analysis prescribed were those detailed in the report of 1909.

The following analysts or laboratories undertook to examine these samples:

1. G. W. Thompson, Chief Chemist, National Lead Company, 129 York St., Brooklyn, N. Y.
2. P. H. Walker, Chief Chemist Contracts Laboratory, Department of Agriculture, Bureau of Chemistry, Washington, D. C.
3. A. D. Little, Inc., 25 Broad St., Boston, Mass.
4. J. W. Kellogg, Chief Chemist, Bureau of Chemistry, Harrisburg, Pa.
5. Spencer Kellogg and Sons, Buffalo, N. Y.
6. S. S. Voorhees, Bureau of Standards, Washington, D. C.
7. John B. Tuttle, Bureau of Standards, Washington, D. C.

The results of their analyses are given in Tables I to VI, inclusive, pages 200-202.

It will be noted in studying these results that the ageing of Samples 1 to 4, inclusive, has not materially changed the constants of these samples except in the acid number, as is shown by the following comparative figures:

Property Considered.	Year.	Samples.			
		1	2	3	4
Specific Gravity $\frac{15^\circ.5}{15^\circ.5}$ C....	1909	0.9347	0.9331	0.9331	0.9344
	1911	0.9342	0.9329	0.9331	0.9344
Specific Gravity $\frac{25^\circ}{25^\circ}$ C.......	1909	0.9298	0.9284	0.9285	0.9295
	1911	0.9299	0.9285	0.9286	0.9297
Acid Number	1909	1.15	3.50	1.94	1.58
	1911	1.39	4.38	2.79	1.86
Saponification Number	1909	190.6	190.1	190.1	190.2
	1911	190.7	190.8	190.2	190.4
Unsaponifiable Matter.......	1909	0.99	0.96	0.99	0.98
	1911	0.96	0.95	0.99	1.04
Refractive Index at 25° C....	1909	1.4800	1.4794	1.4797	1.4797
	1911	1.4799	1.4792	1.4793	1.4794
Iodine Number (Hanus)	1909	187.9	184.5	186.1	186.0
	1911	186.9	183.0	186.0	184.1

It would appear that the provisional specifications for linseed oil suggested in the 1909 report are fairly reliable for oils made from North American seed. The Committee is not qualified at this time to make recommendations of specifications for linseed oil from East Indian or Argentine seed. We do not find any material or rational difference in the oils produced from North American seed based on differences in the age of the seed.

The Committee has been unable so far to do any work on oil from damaged or off-grade seed.

In view of the fact that there is so great a variation in the iodine number and specific gravity of various samples of linseed oil, and the possibility of the adulteration of linseed oil with non-drying oils, without the oils being so affected in iodine number and specific gravity as to enable the analyst to say definitely that they have been adulterated, your Committee would urge the importance of the development of other tests for the purity of linseed oil, such as the hexabromide test, both on the oil and the fatty acids obtained from it, the acetyl value test, and the percentage of solid or saturated fatty acids present.

In the matter of boiled linseed oil, the Committee has made no tests of certified samples, but has considered the question carefully and agrees on the following points:

1. Boiled linseed oil should contain no appreciable amount of volatile matter.
2. The organic unsaponifiable matter in boiled linseed oil should not materially exceed that present in raw linseed oil.
3. While the Committee could not agree that resin compounds should be excluded from linseed oil, it did agree that if resin compounds are present, the amount should not appreciably exceed what would combine to form normal compounds with the lead and manganese present.

During the past year the Committee has offered samples of the 1909 oils to investigators for the purpose of developing these tests. Several sets of samples were sent out; the only report so far received has been one from the Lederle Laboratories, which is appended in full hereto without comment.

We append hereto (see Appendices to this report):

I. A detailed report by the Lederle Laboratories (pages 203–207).
II. Abstracts from S. S. Voorhees' report (pages 208–210).
III. An exceedingly valuable report by H. W. Bearse of the Bureau of Standards on the Coefficient of Expansion of Linseed Oil (pages 211–222).

The thanks of your Committee and of the Society are due to the Department of Agriculture, Bureau of Chemistry, for the assistance rendered in taking the samples of linseed oil; to the American Linseed Company, the Archer-Daniels Company, Hirst and Begley, and the National Lead Company for the oil samples contributed; and also to the various analysts and investigators who have done work on these samples.

Respectfully submitted on behalf of the Sub-Committee,

G. W. THOMPSON,
Chairman, Sub-Committee E.

TABLE I.—SPECIFIC GRAVITY AT $\frac{15^{\circ}.5\text{ C.}}{15^{\circ}.5\text{ C.}}$

Analyst	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1	0.9344	0.9329	0.9329	0.9342	0.9328	0.9333	0.9337	0.9338	0.9345	0.9345	0.9324	0.9329	0.9323	0.9336	0.9340	0.9322
2	0.9349	0.9334	0.9333	0.9342	0.9324	0.9333	0.9341	0.9343	0.9348	0.9344	0.9330	0.9332	0.9327	0.9337	0.9347	0.9323
3	0.9342	0.9325	0.9328	0.9349	0.9329	0.9323	0.9338	0.9341	0.9340	0.9337	0.9316	0.9328	0.9317	0.9335	0.9340	0.9300
4	0.9343	0.9333	0.9332	0.9341	0.9322	0.9325	0.9340	0.9342	0.9345	0.9346	0.9332	0.9336	0.9327	0.9338	0.9345	0.9324
5	0.9336	0.9331	0.9333	0.9341	0.9325	0.9335	0.9340	0.9335	0.9344	0.9345	0.9325	0.9331	0.9325	0.9338	0.9340	.....
6 (a)	0.9339	0.9315	0.9317	0.9336	0.9313	0.9326	0.9335	0.9333	0.9341	0.9341	0.9322	0.9326	0.9321	0.9333	0.9340	0.9317
(b)	0.9342	0.9326	0.9332	0.9347	0.9322	0.9334	0.9342	0.9337	0.9338	0.9345	0.9327	0.9327	0.9331	0.9341	0.9350	0.9324
7	0.9341	0.9326	0.9331	0.9343	0.9320	0.9332	0.9341	0.9342	0.9345	0.9346	0.9327	0.9331	0.9328	0.9340	0.9347	0.9324

SPECIFIC GRAVITY AT $\frac{25^{\circ}\text{ C.}}{25^{\circ}\text{ C.}}$

Analyst	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1	0.9299	0.9283	0.9285	0.9297	0.9275	0.9285	0.9292	0.9293	0.9294	0.9296	0.9275	0.9287	0.9273	0.9289	0.9295	0.9275
2	0.9300	0.9287	0.9289	0.9300	0.9278	0.9288	0.9298	0.9299	0.9300	0.9286	0.9284	0.9290	0.9283	0.9292	0.9301	0.9278
3	0.9294	0.9284	0.9282	0.9294	0.9272	0.9286	0.9291	0.9283	0.9288	0.9282	0.9273	0.9278	0.9275	0.9287	0.9291	0.9264
6 (a)	0.9295	0.9273	0.9275	0.9293	0.9269	0.9279	0.9283	0.9297	0.9304	0.9301	0.9285	0.9291	0.9285	0.9296	0.9306	0.9282
(b)	0.9301	0.9286	0.9286	0.9300	0.9276	0.9287	0.9295	0.9293	0.9297	0.9301	0.9285	0.9286	0.9285	0.9295	0.9304	0.9279
7	0.9299	0.9284	0.9286	0.9296	0.9271	0.9285	0.9292	0.9297	0.9297	0.9298	0.9280	0.9284	0.9281	0.9292	0.9300	0.9278

(*a*)....Plummet. (*b*)....Pyknometer

TABLE II.—ACID NUMBER.

Analyst	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1.............	1.57	4.45	2.79	2.04	4.72	2.07	2.38	1.54	2.00	1.77	0.97	0.91	1.03	0.92	1.78	1.49
2.............	1.5	4.3	2.7	1.9	4.3	2.1	2.1	1.4	1.8	1.5	1.0	0.9	0.8	0.8	1.6	1.2
3.............	1.25	4.40	2.64	1.86	4.45	2.08	2.17	1.56	1.82	1.59	1.07	1.09	0.97	1.43	1.67	0.98
5.............	1.41	4.47	2.88	1.83	4.73	2.12	2.07	1.61	1.93	1.57	1.00	1.00	1.02	0.92	1.80	....
6.............	1.40	4.48	2.94	1.82	4.48	1.96	2.10	1.56	1.68	1.82	0.84	1.12	0.98	1.12	1.68	1.40
7.............	1.23	4.20	2.81	1.68	4.55	1.92	1.89	1.27	1.74	1.29	0.94	0.85	0.83	0.74	1.70	1.13

TABLE III.—SAPONIFICATION NUMBER.

Analyst	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1.............	190.2	191.5	191.2	190.1	190.8	190.6	191.6	191.3	191.3	191.7	190.5	190.4	190.6	190.7	190.2	190.6
2.............	191.1	190.3	189.9	190.2	190.7	191.8	191.1	189.1	190.6	192.0	192.3	190.3	191.1	191.3	192.7	190.5
3.............	190.0	190.0	188.0	190.0	190.0	189.0	191.0	190.0	191.0	194.0	190.0	188.0	193.0	189.0	190.0	190.0
4.............	191.5	192.5	191.8	192.0	191.9	191.8	191.5	192.8	192.9	192.8	192.0	191.8	192.5	190.9	191.8	191.1
5.............	192.4	191.8	191.2	191.2	191.8	191.2	191.9	191.7	190.7	190.3	191.0	190.9	191.0	192.5	192.5	.....
6.............	189.4	189.1	189.0	189.3	189.9	188.3	190.5	190.0	190.2	190.0	189.6	189.8	189.8	190.0	190.5	190.2
7.............	190.5	190.7	190.1	190.3	191.0	191.2	191.2	191.2	191.5	191.3	190.8	191.3	190.3	191.3	191.2	190.9

TABLE IV.—UNSAPONIFIABLE MATTER.

Analyst	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1.............	0.97	1.03	1.02	0.96	0.86	1.00	0.94	1.00	1.06	0.98	0.98	0.95	0.98	0.90	0.91	1.01
2.............	1.05	1.03	1.04	1.24	1.07	1.12	1.23	1.19	1.24	1.15	1.21	1.12	1.26	1.12	1.15	1.09
3.............	0.94	0.85	0.98	1.12	0.99	1.05	....	....	..	....	....	....	....	....	....	....
7.............	0.87	0.91	0.90	0.85	....	....	....	....	....	....	....	....	....	....	....	....

TABLE V.—REFRACTIVE INDEX AT 25° C.

Analyst.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1.............	1.4802	1.4796	1.4795	1.4796	1.4787	1.4785	1.4790	1.4792	1.4792	1.4795	1.4790	1.4792	1.4790	1.4796	1.4796	1.4780
2.............	1.4793	1.4787	1.4789	1.4791	1.4783	1.4780	1.4788	1.4790	1.4790	1.4794	1.4786	1.4790	1.4786	1.4794	1.4793	1.4776
3.............	1.4805	1.4796	1.4798	1.4798	1.4791	1.4782	1.4798	1.4804	1.4800	1.4799	1.4796	1.4794	1.4794	1.4799	1.4798	1.4782
4.............	1.4799	1.4793	1.4797	1.4796	1.4788	1.4789	1.4790	1.4793	1.4794	1.4796	1.4789	1.4791	1.4792	1.4798	1.4799	1.4780
7.............	1.4795	1.4788	1.4788	1.4789	1.4778	1.4775	1.4784	1.4788	1.4787	1.4792	1.4785	1.4791	1.4787	1.4795	1.4794	1.4777

TABLE VI.—IODINE NUMBER (HANUS).

Analyst.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1.............	188.4	184.3	186.6	185.2	179.7	179.8	182.2	184.5	183.5	184.3	179.8	184.3	181.3	187.5	186.0	173.0
2.............	187.1	184.8	186.5	184.4	180.0	178.8	182.6	183.0	182.8	184.6	179.2	182.7	180.4	187.3	185.2	172.1
4.............	188.9	183.3	188.6	185.5	179.6	179.3	182.5	184.9	183.1	186.2	180.9	184.7	181.5	189.1	187.0	175.9
5.............	187.8	182.4	185.4	183.2	178.4	177.0	180.1	182.0	181.2	182.8	179.0	182.1	182.4	186.5	183.9	......
6.............	185.6	182.6	185.6	183.5	178.5	175.9	180.9	181.9	181.6	184.1	178.3	183.7	179.5	185.9	184.4	170.7
7.............	183.8	180.8	183.6	182.6	177.5	176.7	179.8	181.8	181.2	183.5	179.0	183.1	179.1	185.7	183.0	170.3

APPENDICES TO REPORT OF SUB-COMMITTEE E.

I. REPORT OF LEDERLE LABORATORIES, NEW YORK CITY, ON LINSEED OIL RECEIVED FROM THE AMERICAN SOCIETY FOR TESTING MATERIALS.

1. Specific Gravity.

(Pyknometers of 50 cc. capacity were used.)

(*a*) At 15°.5 C.

Sample 1.	Sample 2.	Sample 3.	Sample 4.
0.93407	0.93319	0.93307	0.93352

(*b*) At 25° C.

0.92983	0.92841	0.92864	0.92963

2. Turbidity and Foots in Cubic Centimeters.

Twenty-five cubic centimeters of the unfiltered oil were allowed to stand 14 days in glass tubes 1 cm. in diameter. The tubes were supported in an upright position and stood in a light place at an average daily temperature of 81°.9 F.

0.59	0.20	0.17	0.58

3. Breaking Test.

Twelve cubic centimeters of the filtered oil were heated in a test tube ⅜ in. by 6 ins. over a free flame to 300° C.

Breaks at 270° C.	Does not break.	Does not break.	Breaks at 265° C.

4. Percentage of Moisture and Volatile Matter.

(*a*) The samples were dried in 4-oz. Erlenmeyer flasks at 105° C. in a current of pure dry carbon dioxide. Two grams of unfiltered oil were used, drying to constant weight. Samples 1, 2 and 3 were constant after 2 hours, Sample 4 after 4 hours.

0.02	0.03	0.34	0.23

5. Percentage of Ash.

Ten grams of unfiltered oil were ignited in platinum dishes in a muffle at dull red heat.

0.139	0.031	0.033	0.190

6. Drying Test on Glass in Hours.

Glass plates 7 cm. square and 1.5 to 3.0 mm. thick were dried at 110° C. and the oil applied while the glass was still warm; after cooling one hour in a dessicator, the plates were weighed and sufficient oil added to make 0.1 gram. The plates were kept under a ventilated bell jar in a light place. Drying was completed at an average daily temperature of 87° F. without a damp day intervening.

Sample 1.	Sample 2.	Sample 3.	Sample 4.
60	54	69	54

7. OXYGEN ABSORPTION, USING LEAD MONOXIDE.

Lead dishes of the prescribed size and shape were used in place of aluminum dishes. After the completion of the test, the dishes all appeared clean and bright.

(*a*) Five Grams Litharge.

Per cent.	Hours.	Per cent.	Hours.	Per cent.	Hours.	Per cent.	Hours.
13.18	141	11.31	141	11.75	141	12.69	141

(*b*) Ten Grams Litharge.

13.74	141	13.29	141	12.78	141	13.40	141

The detailed results are presented in Table I.

TABLE I.—PERCENTAGE OF OXYGEN ABSORPTION.

Time in Hours.	Sample 1.		Sample 2.		Sample 3.		Sample 4.	
	10 Grams Pb O.	5 Grams Pb O.	10 Grams Pb O.	5 Grams Pb O.	10 Grams Pb O.	5 Grams Pb O.	10 Grams Pb O.	5 Grams Pb O.
24.....	12.89	12.04	11.98	8.58	11.81	10.31	12.49	11.01
45.....	12.39	12.04	11.75	9.29	11.44	10.31	12.09	11.16
53.....	12.52	12.04	11.85	9.48	11.57	10.50	12.15	11.28
69.....	12.70	12.25	12.18	9.93	11.81	10.75	12.40	11.59
77.....	12.95	12.52	12.37	10.25	12.00	11.04	12.69	11.90
117.....	13.16	12.66	12.68	10.81	12.26	11.31	12.89	12.16
125.....	13.39	12.93	12.85	10.96	12.45	11.46	13.09	12.35
141.....	13.74	13.18	13.29	11.31	12.78	11.75	13.40	12.69
149.....	13.45	13.07	13.00	11.20	12.54	11.63	13.18	12.48
165.....	13.24	12.79	12.79	10.99	12.30	11.37	12.97	12.20
173.....	13.19	12.75	12.79	11.01	12.30	11.37	12.97	12.20
189.....	13.16	12.73	12.81	11.11	12.28	11.42	12.93	12.21
197.....	12.97	12.60	12.64	10.98	12.09	11.18	12.72	12.09
213.....	13.12	12.66	12.81	11.09	12.28	11.44	12.89	12.20
221.....	13.49	12.99	13.17	11.43	12.61	11.69	13.23	12.50
309.....	13.47	13.07	13.31	11.62	12.59	11.69	13.21	12.59
Oil taken (grams)	0.5165	0.5157	0.5205	0.5464	0.5374	0.5225	0.5273	0.5328

8. ACID NUMBER.

(Expressed in milligrams of KOH per gram of oil.)

Sample 1.	Sample 2.	Sample 3.	Sample 4.
1.20	3.60	2.50	1.40

9. SAPONIFICATION NUMBER.

(Expressed in milligrams of KOH per gram of oil.)

Saponification was effected by boiling one hour under a reflux condenser; corrections were applied for the action of the alkali on the glass.

190.6	190.4	192.0	192.7
191.1	189.9	191.8	192.5
190.6	190.2	192.1	191.8
190.8	190.2	191.9	191.3

11. LIEBERMANN-STORCH TEST.

Sample 1.	Sample 2.	Sample 3.	Sample 4
Negative	Negative	Negative	Negative
Green	Green	Green	Green

12. REFRACTIVE INDEX.

In this determination, the Zeiss Butyro-Refractometer was used, using sodium light as a source of illumination. Determinations were made on the original oils and on the fatty acids prepared from the oils.

(*a*) Original Oils at 25° C.

1.4799	1.4793	1.4794	1.4797

(*b*) Fatty Acids at 25° C.

1.4710	1.4703	1.4708	1.4704

13. ACETYL VALUE.

Twenty cubic centimeters of the filtered oil were boiled with an equal volume of acetic anhydride for 2 hours under a reflux condenser, the flask being ground to the condenser. Both the distillation and filtration method were used.

(*a*) Distillation.

28.3	21.0	18.5	18.3
30.3	36.0 (?)	18.8	20.3

(*b*) Filtration.

29.0	23.0	45.0	40.9
21.5	21.3	39.2	34.3

In the distillation method, it appears that the results are fairly uniform, omitting the 36.0 value for Sample 2. In the filtration method, however, the results are far from satisfactory. From our experiments it seems that these discrepancies are due to free fatty acids coming through with the final filtrates, as indicated by the fact that the more turbid the filtrate the higher the result obtained. In the results given under (*b*), after adding the correct amount of standard acid to the soap solution, the resulting solutions were heated under a reflux condenser until quite clear and then filtered. Even this procedure did not yield perfectly clear filtrates.

In one case where the filtrate was fairly turbid, a value of 36.3 was obtained for Sample 1. In another case, where the filtrate was markedly turbid, a value of 55.6 was obtained for Sample 2. These results are excluded for this reason.

Lewkowitsch* makes the following statement:

"In order to facilitate the separation of the insoluble fatty acids in the filtration process, it will be found useful to add a slight excess of mineral acid. Of course this amount must be measured accurately and deducted from the alkali required for determining the dissolved acids."

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* *Chem. Tech. and Anal. of Oils, Fats and Waxes*, Vol. I, 1904, p. 270.

We made no determinations by adding more than the equivalent quantity of acid, but it seems that work in this direction would be of some value in the filtration method.

14. HEXABROMIDE TEST.

(*a*) Bromine Precipitate (Tolman's Method), per cent.

Sample 1.	Sample 2.	Sample 3.	Sample 4.
55.93	57.01	58.48	49.31

(*b*) Melting Point of Bromine Precipitate, degrees Centigrade.

137	138	138.5	136

In addition to the above determinations, the hexabromides of the fatty acids were also determined according to the method of Hehner and Mitchell as described in Lewkowitch.*

(*c*) Hexabromides of Fatty Acids (Hehner and Mitchell), per cent.

35.08	31.79	36.31	35.46

Our experience with this determination leads us to believe that the volumes of reagents used is of importance. The following series were conducted as follows:

Thirty centimeters of glacial acetic acid were added for every gram of fatty acids used, adding bromine in excess. After standing for three hours in the refrigerator, the bromides were filtered through a Gooch crucible and washed successively with 5 cc. each of chilled glacial acetic acid, ethyl alcohol and ethyl ether. The crucible was dried one hour at 100° C. and weighed. The results obtained are given under (*d*).

(*d*) Hexabromides of Fatty Acids (Hehner and Mitchell), per cent.

29.94	28.16	30.41	31.29
29.87	27.38	29.75	31.54
		32.51 (?)	

(*e*) Melting Point of Hexabromides of Fatty Acids, from Determinations (*d*), degrees Centigrade.

178	178.5	178.3	178.4

Inasmuch as the bromides are not pure white as obtained above, another series of determinations was made, in which the bromides were washed with a still further quantity of ethyl ether until pure white. The results are given under (*f*).

(*f*) Hexabromides of Fatty Acids (Hehner and Mitchell) washed until pure white, per cent.

28.33	26.92	27.67	28.79

A larger quantity of hexabromides of the fatty acids was prepared qualitatively and washed until pure white. The melting points are given under (*g*).

***Ibid*, p. 365.

(*g*) Melting Point of Hexabromides of Fatty Acids, degrees Centigrade.

Sample 1.	Sample 2.	Sample 3.	Sample 4.
177.3	177.8	177.3	177.7

It is evident from these results that the determinations as carried out under (*d*), with the exception of the 32.51 per cent. on Sample 3, show quite uniform results, and it is our opinion that a further study along these lines is of extreme importance.

15. IODINE NUMBER.

(*a*) Hanus Method.

186.1	182.1	185.3	183.6
185.4	182.1	184.0	183.8
185.75	182.1	184.65	183.7

(*b*) Wijs Method.

187.3	183.9	186.9	184.6
187.2	184.4	187.1	185.8
187.25	184.15	187.0	185.2

In both the Hanus and Wijs method, the solution of the oil was allowed to remain in contact with the reagent for one hour in a dark place before titration.

Respectfully submitted,

JOSEPH A. DEGHUEE, PH.D.,
Director, Department of Chemistry.
PAUL POETSCHKE,
Ass't Director, Department of Chemistry.

II. COMMENTS BY S. S. VOORHEES.*

Specific Gravity.

(*a*) *Plummet.*—The specific gravity was obtained by means of a plummet approximately 5 cc. in volume, containing an accurate thermometer. The weighings were made on an accurate analytical balance. The determination was first made at 25° C., since that temperature was nearest the room temperature. The temperature of the oil in the cylinder was raised two or three degrees above 25° C., the cylinder placed on the balance with the plummet in the oil, and when the temperature of the oil fell to 25° C. the weighing was quickly made.

To obtain the specific gravity at 15°.5 C. the oil was cooled in chilled water to about 13° C., the cylinder placed in the balance case with plummet suspended in the oil, and when the temperature rose to 15°.5 C. the weight was taken. The plummet was standardized in pure water at these temperatures in the same manner.

(*b*) *Pyknometer.*—These determinations were made by means of three 25-cc. and four 50-cc. pyknometers provided with thermometers and perforated caps. The pyknometers were filled with the oil chilled to 2° or 3° below 15° C. The thermometer was inserted and the temperature allowed to rise to 15°.5 C. When that temperature was reached the pyknometer was capped, cleaned with gasoline, wiped dry, and weighed.

To determine the specific gravity at 25° C., the caps were removed and the pyknometer placed in a room having a temperature of about 30° C. When the thermometer showed the temperature of the oil to be 25° C. the pyknometers were again capped, cleaned with gasoline, wiped dry, and weighed as before. The standardization of the pyknometers with pure water was conducted in the above manner.

Acid Value.

Ten grams of oil were weighed into an Erlenmeyer flask and 25 cc. of neutralized alcohol added, brought to a boil on the steam plate, and titrated with N/2 KOH and phenolphthalein as indicator to distinct coloration.

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* These comments refer to the determinations, etc., made by Mr. Voorhees, shown in Tables I to VI, inclusive, of this report, pp. 200–202.

LIEBERMANN-STORCH TESTS.

The alcoholic extract of the above determination was evaporated to dryness and tested for rosin. In every case an apple-green coloration was obtained.

SAPONIFICATION VALUE.

A water-white standard alcoholic KOH and an N/2 HCl, standardized by $AgNO_3$ and weighing AgCl in a Gooch crucible, were used. Five grams of oil were accurately weighed out in an Erlenmeyer flask, 50 cc. of alcoholic KOH run in from a standard pipette, a glass tube reflex condenser inserted, and the flask heated on the water bath for one hour. Then it was cooled and titrated with N/2 HCl using phenolphthalein as an indicator.

VOLATILE MATTER AT 110° C.

Ten grams of oil were accurately weighed in a porcelain crucible and heated for 2 hours in the oven at 110° C.

Sample No.	Per cent. Loss.	Sample No.	Per cent. Loss.
1	0.05	9	0.04
2	0.03	10	0.05
3	0.13	11	0.06
4	0.04	12	0.04
5	0.13	13	0.04
6	0.06	14	0.04
7	0.04	15	0.05
8	0.04	16	0.05

PERCENTAGE OF ASH.

The oil from the volatile determinations was ashed and the ash tested for manganese by means of ammonium persulphate. The manganese coloration was obtained in every case and in some cases was quite marked.

Sample No.	Per cent.	Sample No.	Per cent.
1	0.16	9	0.19
2	0.02	10	0.21
3	0.03	11	0.05
4	0.16	12	0.03
5	0.03	13	0.03
6	0.18	14	0.04
7	0.18	15	0.16
8	0.16	16	0.16

IODINE NUMBER.

About 0.15 gram of oil was weighed into freshly ignited porcelain boats, introduced into tightly stoppered bottles of 500-cc. capacity, and dissolved in 10 cc. of chloroform. Twenty-five cubic centimeters of Hanus solution were added and allowed to stand 30 minutes, the bottle being shaken three times during that period. At the end of 30 minutes 10 cc. of 10-per cent. KI solution were added, the bottle shaken, 100 cc. of water added, and the excess of iodine titrated with N/10 thiosulphate solution using starch as an indicator. The excess of iodine was about 59 per cent. of the total amount added. The thiosulphate solution was standardized with N/10 potassium dichromate prepared by dissolving 4.90838 grams of the pure salt dried at 130° C. in one liter of distilled water. Blanks were run with each set of determinations.

S. S. VOORHEES, *Engineer-Chemist.*

III. THE DENSITY AND THERMAL EXPANSION OF LINSEED OIL.

By H. W. Bearse.

Introduction

An investigation of the density and thermal expansion of linseed oil was undertaken at the Bureau of Standards in response to a demand for more complete and accurate knowledge of these physical constants as an aid in setting up standards of purity of this substance. It was hoped also that a comprehensive study might reveal some definite relations between different physical properties, such, for example, as density and coefficient of expansion.

The work was very greatly aided by the American Society for Testing Materials, from whom the samples were secured through the courtesy of Mr. S. S. Voorhees, a member of the special committee on linseed oil and a member of this Bureau.

Experimental Work.

Sixteen samples, of known origin, were secured and their densities determined at 10°, 20°, 30°, and 40° C. by the method of hydrostatic weighing, that is, by weighing in the sample a sinker of known mass and volume.

The apparatus used in making the density determinations was that devised by Mr. N. S. Osborne, formerly of this Bureau, and used by him in the work on the density and thermal expansion of ethyl alcohol.

Arrangement of Apparatus.

The arrangement of apparatus is as follows: The sample under investigation and the sinker are placed in a tube having a length of about 50 cm. and a diameter of 2 cm., and surrounded by a temperature bath kept in constant circulation by a small propeller. This inner bath is surrounded by a second bath, which is also kept in constant circulation and whose temperature can be maintained constant or varied at will over a range of 10° to 40° C., by means of a thermostat and an electric heating coil and a copper coil through which refrigerating brine may be passed. The outside containing vessel for the various tubes and baths is a large unsilvered Dewar cylinder provided with a brass cap suitable for supporting the inner parts of the apparatus. The cylinder is mounted in a vertical position and covered with a layer of nickeled paper through which windows are cut to permit observations to be made.

The temperature is read on two mercurial thermometers suspended in water in a tube placed symmetrically with that containing the sample of oil. The thermometers are so mounted on a movable cap that by its rotation they may be successively brought into position for reading, which is done with the aid of a long-focus microscope. The object of placing the thermometers in a tube instead of directly in contact with the water of the inner circulating bath is to eliminate, as far as possible, errors due to temperature lag within the densimeter tube. Since the sample under test is separated from the circulating bath by the densimeter tube, the thermometers should be separated from it by a similar tube, in order that when a constant temperature is indicated by them the same constant temperature shall obtain within the densimeter tube.

The same thermometers were used repeatedly over the same temperature range, and from previous experience with these thermometers over this range, it was thought unnecessary to take ice-point readings after each reading of the thermometers. Ice-point readings were, however, taken occasionally throughout the work and these, together with an extended series obtained earlier in the year, were found to be sufficiently concordant to warrant their use in fixing the corrections to be applied at the various temperatures.

The thermometers used were:

B. S. No. 4653, made by Tonnelot in 1888, of verre dur glass.
B. S. No. 2040, " " Haak " 1906, " Jena 16^{III} glass.
B. S. No. 264, " " Richter " 1902, " " " "

All thermometers were graduated to 0°.1 C.

The volume of the sinker used in making the density determinations was calculated from its mass and its apparent weight in twice distilled water at 4°, 10°, 20°, 30°, and 40° C., assuming Chappuis' values for the density of water. The equation

$$V_t = V_x + \alpha(t-x) + \beta(t-x)^2$$

was assumed to represent sufficiently well the expansion of the sinker. The values of V_x, α and β obtained by making a least squares reduction of the observations were:

$$V_x = V_{24} = 47.7174 \text{ cc.}$$
$$\alpha = 0.0011001$$
$$\beta = 0.0000009734$$

Therefore,

$$V_t = 47.7174 + 0.0011001(t-24) + 0.0000009734(t-24)^2,$$

from which the calculated volumes are as follows:

Temperature, deg. Cent.	Volume, cc.
10	47.7022
20	47.7130
30	47.7240
40	47.7352

The sinker is suspended by a hook from a small secondary sinker attached to a wire let down from one pan of the balance. The secondary sinker has a mass sufficient to keep the suspension wire straight and in its proper position, and is always immersed in the liquid whether the larger sinker (of known mass and volume) is attached or not.

The suspension wire, at the point where it passes through the surface of the liquid, has a diameter of 0.3 mm. and is covered with a layer of unpolished gold by electro-deposition. In the case of liquids which imperfectly wet the suspension, this roughening of the surface is essential, but with oil it is probably unnecessary.

All weighings were made by the method of substitution, that is, a constant mass was kept on one pan of the balance and the weighings made on the other, sufficient weights being placed on the pan to secure equilibrium, first with the sinker attached and then detached. The weighings were made on a Rueprecht analytical balance (B. S. No. 5156) having a capacity of 200 grams and a sensibility of 0.08 gram per division when undamped. When the immersed sinker was attached the sensibility was greatly reduced, especially at the lower temperatures, owing to the viscosity of the oil. At the higher temperatures, weighings could be made to a few tenths of a milligram, but at 10° C. difficulty was experienced in weighing closer than from one to two milligrams.

Of the weights used, those of less than 1 gram were a special set provided with the balance, and were manipulated by keys on the outside of the balance without opening the case. Those between 1 and 100 grams were platinum-plated brass weights (B. S. No. 5157). These weights, used only for special purposes, were re-calibrated a few months before the beginning of this work, and the maximum error of any possible combination of weights was found to be so small in comparison with accidental errors as to be negligible.

All weighings were reduced to vacuo by means of a special device originally designed for use in correcting weighings of water in the test of volumetric apparatus. This apparatus consists of a glass bulb of such a volume (about 900 cc.) that when suspended from one arm of a balance and counterpoised against a brass weight of equal mass, the number of milligrams that must be added to the pan from which the bulb is suspended to secure equilibrium is equal to the buoyancy on a liter of water weighed in air against brass weights. This buoyancy constant when divided by 881.3 (which is the difference in volume between the brass weights and the glass bulb) gives the air density. For convenience, a table is arranged giving the air density corresponding to any observed buoyancy constant.

METHOD OF PROCEDURE.

In making the density determinations the method of procedure was as follows: The water in the outside circulating bath was first brought to the desired temperature. When the thermometers in the inner tube indicated a constant temperature, it was assumed that the liquid in the densimeter tube was at the same constant temperature, and observations were begun. First, a weighing was made with the sinker suspended in the sample, then the temperature was observed on each of the two thermometers; next a weighing was made with the sinker off, then a second with the sinker on, and after that a second observation of temperature. The temperature was then changed to the next point of the series, and here the same order was followed, except that at 40° only one thermometer was read. After completing the observations at the four temperatures, 10°, 20°, 30°, and 40° C., the density at each temperature was calculated from the weighings and the observed temperatures. The mean of the differences between the weighings with the sinker on and off was taken as the apparent weight of the sinker in the sample at the temperature of observation, this temperature being

TABLE I.—OBSERVATIONS AND CALCULATION OF DENSITIES, SAMPLE 6.

Date. 4-13-1911.	Temperature, deg. Cent.		Corrected Temperature.	Balance Reading, grams.	Apparent Weight, grams.	Air Buoyancy.	Corrected Weight, grams	Weight of Sinker (in vacuo), grams.	Weight of Displaced Liquid, grams.	Volume, cc.	D_t	D_t (Reduced)
	No. 2040	No. 4653										
12:30 p.m.	10.055 10.028 (−0.008)	9.975 9.972 (+0.091)	10.047 10.066 10.020 10.063 10.049	38.5045 93.8308	55.3263 55.3263	1052 22°.8 25°.2 1042 0.001182 −0.0078	55.3185	99.9990	44.6805	47.6805	0.936655 (+34)	0.936689 (10° C.)
1:40 p.m.	19.870 19.875 (−0.003)	19.828 19.848 (+0.036)	19.867 19.864 19.872 19.884 19.872	38.1802 93.8191 38.1790	55.6389 55.6401 55.6395	−0.0078	55.6317	99.9990	44.3673	47.71282	0.929882 (−88)	0.929794 (20° C.)
2:40 p.m.	29.960 29.965 (−0.005)	29.960 29.692 (+0.005)	29.955 29.965 29.960 29.967 29.962	37.8496 93.8072 37.8486	55.9576 55.9586 55.9581	−0.0078	55.9503	99.9990	44.0487	47.72394	0.922990 (−26)	0.922964 (30° C.)
3:25 p.m.	No 264 39.780 39.795 (−0.033)	....	39.747 ...... 39.762 ...... 39.754	37.5280 93.7962 37.5274	56.2682 56.2688 56.2685	−0.0079	56.2606	99.9990	43.7384	47.73492	0.916277 (−168)	0.916109 (40° C)

NOTE: In the column headed "Air Buoyancy,"
1052 = observed buoyancy constant.
22°.8 = temperature at the buoyance bulb.
25°.2 = temperature in the balance.
1042 = buoyancy constant corrected for temperature.
0.001182 = air density.
0.0078 = buoyancy correction to the apparent weight, in grams.

taken as the mean of the four corrected thermometer readings. The apparent weight of the sinker was corrected for the buoyancy of the air by subtracting from it the volume of the brass weights multiplied by the air density. This corrected weight of the sinker in the sample, subtracted from its weight in vacuo, gives the weight of the liquid displaced by the sinker; and the weight of this displaced liquid divided by its volume—that is, the volume of the sinker—gives the density of the sample at the temperature of observation. Or, if expressed in the form of an equation,

$$D_t = \frac{W - \frac{(w-w_1)+(w-w_2)}{2}\left(1-\frac{\rho}{8.4}\right)}{V_t}$$

in which

D_t = density of sample at the temperature t.
W = weight of sinker in vacuo.
w = weighing with sinker off.
w_1 and w_2 = weighings with sinker on.
ρ = air density.
8.4 = assumed density of brass weights.
V_t = volume of sinker at temperature t.

After calculating the density of a sample at 10°, 20°, 30°, and 40° C., it was assumed that the rate of expansion could be represented by the equation

$$D_t = D_x + \alpha(t-x) + \beta(t-x)^2,$$

and a least squares reduction was applied to the measured densities in order to find D_x, α, and β, and the most probable value of the density at each temperature.

Table I contains the observations on an average sample, No. 6. The reduction of these observations is also given as follows:

REDUCTION OF OBSERVATIONS, SAMPLE 6.

t.	$C_1 = t - t_m$	C_1^2	$C_2 = C_1^2 - (C_1^2)$	C_2^2	D_t	$N = D_t - (D_t)_m$	C_1N	C_2N
10	−15	225	100	10000	0.936689	+0.010300	−0.15450	+1.0300
20	−5	25	−100	10000	0.929794	+0.003405	−0.01702	−0.3405
30	5	25	−100	10000	0.922964	−0.003425	−0.01712	+0.3425
40	15	225	100	10000	0.916109	−0.010280	−0.15420	−1.0280
$t_m = 25$		500 $(C_1^2)_m = 125$		40000	0.926389 $= (D_t)_m$		−0.34284	+0.0040

$$500\ \alpha = -0.34284 \qquad 40000\ \beta = +0.004$$

$$\alpha = -0.0006857 \qquad \beta = +0.0000001$$

$$D_x + 125\ \beta = (D_t)_m$$

$$D_x = D_{25} = 0.926389 - 0.000012$$

$$D_{25} = 0.926377$$

$$D_t = D_{25} + \alpha(t-25) + \beta(t-25)^2$$

$$D_t = 0.926377 - 0.0006857(t-25) + 0.0000001(t-25)^2$$

t	$t-25$	$(t-25)^2$	$\alpha(t-25)$	$\beta(t-25)^2$	D_t		Observed minus Calculated.
					Calculated.	Observed.	
10	−15	225	+0.010286	+0.000022	0.93668	0.93669	1
20	− 5	25	+0.003428	+0.000002	0.92981	0.92979	−2
30	5	25	−0.003428	+0.000002	0.92295	0.92296	1
40	15	225	−0.010286	+0.000022	0.91611	0.91611	0

$$D_t = D_{25} + \alpha(t-25) + \beta(t-25)^2 \qquad (1)$$

Taking for α and β the mean values of the sixteen samples, the general equation becomes

$$D_t = D_{25} - 0.0006847\ (t-25) + 0.00000012\ (t-25)^2$$

If it is desired to reduce the expansion to a single term, for use over a short temperature range, this may be done by differentiating the general equation and combining α and β into a single term α′, which will be different for different temperatures.

$$D_t = D_{25} + \alpha(t-25) + \beta(t-25)^2$$

$$\frac{dD_t}{dt} = \alpha + 2\beta(t-25) = \alpha' \qquad (2)$$

Substituting for t the values 10, 15, 20, 25, 30, 35, and 40, we have for the rate of change of density at the different temperatures the following values:

Temperature, deg. Cent.	Change per deg. Cent.
10	0.0006883
15	0.0006871
20	0.0006859
25	0.0006847
30	0.0006835
35	0.0006823
40	0.0006811

The densities of the sixteen samples of linseed oil, reduced to the basis of specific gravity at 15°.5/15°.5 C. and at 25°/25° C., are as follows:

Sample No.	Density.	
	Sp. Gr. $\frac{15^\circ.5}{15^\circ.5}$ C.	Sp. Gr. $\frac{25^\circ}{25^\circ}$ C.
1	0.93499	0.93028
2	0.93309	0.92840
3	0.93325	0.92857
4	0.93447	0.92978
5	0.93221	0.92751
6	0.93379	0.92909
7	0.93428	0.92960
8	0.93420	0.92951
9	0.93458	0.92992
10	0.93471	0.93002
11	0.93317	0.92849
12	0.93336	0.92868
13	0.93272	0.92805
14	0.93414	0.92944
15	0.93463	0.92992
16	0.93238	0.92771

Table II is a summary of the results on the sixteen samples arranged in the increasing order of their densities. It will be seen from the tabulated results that the coefficient of expansion as determined from sixteen samples varies over rather narrow limits; for example, at 25° C. the change of density per degree lies between 0.0006823 and 0.0006873, the mean of the sixteen samples being 0.0006847. It will also be seen that the rate of change of density is, in general, greater at the lower than at the higher temperatures, Sample 9 being the only exception. In regard to this exception, it should perhaps be stated that the tabulated values of α, β, and D_{25} are the means of two independent determinations on different portions of Sample 9 and that these two determinations were in substantial agreement. The truth of the exception would, therefore, seem to be established. It further appears from the tabulated results that the coefficient of expansion is somewhat greater for the heavier than for the lighter oils; the mean of the first half of the series being 0.0006842 at 25° C. and for the last half 0.0006852. This fact, however, should not be

TABLE II.—DENSITY AND THERMAL EXPANSION OF LINSEED OIL AT TEMPERATURES BETWEEN 10° AND 40° C.
(Arranged in increasing order of the densities.)

Sample No.	Density.					$\alpha \times 10^7$	$\beta \times 10^9$
	$\frac{10^\circ}{4^\circ}$ C.	$\frac{20^\circ}{4^\circ}$ C.	$\frac{30^\circ}{4^\circ}$ C.	$\frac{40^\circ}{4^\circ}$ C.	$\frac{25^\circ}{4^\circ}$ C.		
5	0.93513	0.92823	0.92139	0.91460	0.92480	−6844	+262
16	0.93526	0.92841	0.92158	0.91475	0.92499	−6836	+ 50
13	0.93561	0.92875	0.92193	0.91514	0.92534	−6823	+192
2	0.93598	0.92911	0.92226	0.91543	0.92568	−6851	+ 90
11	0.93606	0.92920	0.92235	0.91552	0.92578	−6848	+ 58
3	0.93614	0.92928	0.92244	0.91561	0.92586	−6843	+ 90
12	0.93624	0.92938	0.92255	0.91573	0.92596	−6838	+ 95
6	0.93668	0.92981	0.92295	0.91611	0.92638	−6857	+100
14	0.93704	0.93016	0.92329	0.91642	0.92672	−6875	+ 38
8	0.93710	0.93022	0.92338	0.91657	0.92680	−6843	+168
7	0.93716	0.93031	0.92347	0.91664	0.92688	−6840	+ 78
4	0.93738	0.93049	0.92365	0.91686	0.92706	−6842	+240
9	0.93746	0.93062	0.92377	0.91691	0.92720	−6850	− 88
15	0.93755	0.93063	0.92378	0.91699	0.92720	−6852	+320
10	0.93761	0.93073	0.92389	0.91707	0.92730	−6844	+155
1	0.93789	0.93100	0.92413	0.91727	0.92756	−6873	+ 78
				Mean	0.92634	−6847	+120

given great weight, as three samples in the first half fall above and four in the last half below the mean value.

It appears from this investigation that if the density of any sample of pure linseed oil be determined at 25° C., its density at any other temperature between 10° and 40° C. may be calculated within the limits of ordinary experimental errors by use of the general equation

$$D_t = D_{25} + \alpha(t-25) + \beta(t-25)^2,$$

in which α is taken as -0.0006847 and β as $+0.000000120$. Or, the density may be measured at any other convenient temperature and for short temperature intervals the corresponding value of α' used. For example, to reduce from 19° C. to 20° C. the value 0.0006859 would be used.

It should be borne in mind that all values in this paper are referred to water at 4° C. as unity and not, as is sometimes done, referred to water at the same temperature. If, however, as is sometimes convenient, density determinations are referred to water at the same temperature, they may, of course, be reduced to the basis of 4° C. by multiplying by the density of water at the temperature of observation.

In regard to the accuracy of the results given in this paper, it is believed that at all temperatures except 10° C. the values of density are correct to within three units of the fifth decimal place, and at 10° C. to within five units of the fifth decimal place. At 10° C. the method employed was not so satisfactory as it was at the higher temperatures, owing to the increased viscosity of the oil. As there is also some objection to the use of this method on those samples in which a large amount of suspended matter is present, it is expected that in the immediate future a series of measurements will be made with a special pyknometer of the Sprengle type, designed for the temperature bath already described, to determine the magnitude of the change of density caused by the settling out of the suspended matter.

A comparison of the values of the coefficient of expansion given in this paper with those generally accepted will show that the values given by Allen* and by Andés† appear to be too small; they are respectively 0.000649 and 0.00063 per degree Centigrade. Ennis‡ gives 0.00045 per degree Fahrenheit, which is equal to 0.00081 per degree Centigrade and is undoubtedly too large. The low value given by Allen may be explained, at least in part, by his assumption that the rate of expansion is the same at all temperatures. He determined the specific gravity at two widely separated temperatures, and took the mean value thus found as the coefficient of expansion. Since the present work has shown that the rate of expansion is less at the higher than at the lower temperatures, the mean coefficient given by Allen is obviously too low for ordinary laboratory temperatures.

Hurst§ gives the value 0.00065 as the change of density per degree Centigrade. Maire‖ gives the value of 0.00035 per degree Fahrenheit (0.00063 per degree Centigrade), but he applied the temperature correction in the wrong direction. The most complete and definite results found are those obtained by Sabin.¶ He gives the rate of change between 15°.5 and 28° C. as 0.000692, and between 28° and 100° C. as 0.000720 per degree Centigrade.

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* Commercial Organic Analysis, Vol. I, Part 1.

† Boiled Oils, Drying Oils, etc.

‡ Linseed Oil and Other Seed Oils.

§ Painters' Colors, Oils and Varnishes.

‖ Modern Pigments and Their Vehicles.

¶ Technology of Paint and Varnish.

This value of the expansion between 15°.5 and 28° C. is in better agreement with the results of the present investigation than any other yet found. The increased coefficient at high temperatures, reported by Sabin, was not confirmed by this work. It is possible, of course, that the coefficient does increase at temperatures higher than that to which this work was carried.

A majority of the references consulted give the limits of the specific gravity of pure raw linseed oil at 15°.5 C. as 0.931 to 0.937, referred to water at 15°.5 C. as unity. The limits found in the present investigation, if reduced to the same temperature basis, are 0.9322 and 0.9350.

From the scant data available, and in the inadequacy of the usual definitions of specific gravity, it is impossible to make a satisfactory comparison of published results. For example, by the term "coefficient of expansion" one author means the change of specific gravity per degree Centigrade divided by the specific gravity; another means the change of specific gravity per degree, all specific gravities being referred to water at the temperature of observation; while still another means the change of specific gravity per degree, the specific gravities being referred to water at two different temperatures—that is, the coefficient of expansion last defined is the difference between the expansion of the oil in question and that of water.

The writer wishes to protest most vigorously against these many and varied practices, and to suggest that all specific gravities be referred to water at some one definitely stated temperature (preferably 4° C., in order that specific gravity and density expressed in grams per cubic centimeter may become identical), and that the coefficient of expansion of a liquid be understood to mean the change of density per degree Centigrade.

A word of explanation in regard to this proposed uniform temperature basis for all specific gravity determinations may perhaps not be out of place, as the objection is often raised, by chemists and others, that 4° C. is not a convenient temperature at which to make determinations. Now in reality that is no objection at all, for the reason that determinations need not be made at 4° C., but may be made as usual at any convenient temperature and then reduced to the basis of water at 4° C. as unity, by multiplying by the ratio of the densities of water at the temperature of observation and at 4° C. Since expressing specific gravities in terms of water at a definite temperature simply means that a definite value has been chosen for the unit to be employed, it follows that if the relative size of the different units be known, the specific gravities may be expressed in terms of water at any desired temperature.

Suppose, for example, that it is desired to determine by the use of the pyknometer the specific gravity of a sample of oil at 20° C. in terms of water at 4° C. as unity (that is, the density at 20° C.). It is only necessary to determine it at 20° C. in terms of water at 20° C. in the usual manner, and then multiply the observed value by the density of water at 20° C. Suppose the value of the specific gravity at 20°/20° C. is found to be 0.93182; then the value at 20°/4° C. will be 0.93182 × 0.998234 = 0.93017.

For other temperatures the transformation is equally simple:

$$\text{Specific Gravity at } \frac{15^\circ.5}{4^\circ}\text{C.} = \text{Specific Gravity at } \frac{15^\circ.5}{15^\circ.5}\text{C.} \times 0.999050.$$

$$\text{Specific Gravity at } \frac{25^\circ}{4^\circ}\text{C.} = \text{Specific Gravity at } \frac{25^\circ}{25^\circ}\text{C.} \times 0.997077.$$

REPORT OF SUB-COMMITTEE F ON THE DEFINITION OF TERMS USED IN PAINT SPECIFICATIONS.

In this report only general terms have been included. Other terms are under consideration and will, it is hoped, be reported on at later meetings of the Society.

Standard.—A term designating a quality or qualities specified.

Equal to.—The use of this term should be avoided if possible.

Pure.—Standard, without adulteration.

Commercially Pure.—Is not defined and should not be used in specifications as it involves the absence of "*standard.*"

Adulteration.—The partial substitution of one substance for another.

Adulterant.—A substance partially substituted for another.

Bulk.—The bulk of a pigment shall be considered as the total volume of the pigment and the voids, and varies inversely as the specific gravity of that volume.

Voids.—The space between the particles of a pigment, even though occupied by air or by a vehicle, whether liquid or dried.

Opacity.—The obstruction to the direct transmission of visible light afforded by any substance—comparison being made with sections of equal thickness. The opacity in the case of pigments should be considered as referable to their opacity in a vehicle under standard conditions.

Crystalline.—Having a definite structure referable to one of the systems of crystallography.

Amorphous.—The opposite of crystalline.

Paint.—A mixture of pigment with vehicle, intended to be spread in thin coats for decoration or protection, or both.

Pigment.—The fine solid particles used in the preparation of paint, and substantially insoluble in the vehicle.

Vehicle.—The liquid portion of a paint.

Volatile Thinner.—All that portion of a paint, except water, which is volatile in a current of steam at atmospheric pressure.

Non-Volatile Vehicle.—The liquid portion of paint, excepting water, which is not a volatile thinner by the above definition.

Tinting Strength.—The relative power of coloring a given quantity of paint or pigment selected as standard for comparison.

Water.—Dissolved water or water not definitely or chemically combined.

Dry.—Containing no uncombined water.

Specific Gravity.—The relative weight of a unit volume of a substance compared with the weight of the unit volume of water at defined temperatures.

Color.—A generic term including the colors of the spectrum, white and black, and all tints, shades and hues which may be produced by their admixture.

Tint.—A color produced by the admixture of a commercial coloring material, excepting white, with a white pigment or paint, the white predominating.

Drying.—The solidification of a liquid, independent of change in temperature.

Respectfully submitted on behalf of the Sub-Committee,

G. W. THOMPSON,
Chairman, Sub-Committee F.

REPORT OF SUB-COMMITTEE J ON THE TESTING OF WHITE PAINTS.

This Sub-Committee begs to submit the following report:

It was decided in conducting these tests to limit them strictly to white paints, not to use any tints. The paints are to be applied on both sides of yellow poplar panels, 8 by 1 by 36 ins. Three of these single pieces are to be clamped together, forming a temporary surface 24 by 36 ins. for the application of the paint. The paint is to be applied at a definite spreading rate; three coats with at least seven days time between coats for drying. Two sets of these panels, each consisting of three pieces painted on both sides, are to be used for each test. These panels are to be exposed in a fence or rack to be constructed as follows: Posts of locust or red cedar, 10 ft. 10½ ins. long by approximately 6 ins. in diameter, planted in rough concrete 2½ ft. deep, 10 ft. 6 ins. in the clear between the posts, are to be connected by three lines of 2 by 4-in. yellow-pine stringers. The top stringers are to be spiked to the top of the posts; the middle and bottom stringers are to be notched in and spiked with a clearance of 36½ ins. Two strips of 1-in. quarter-round molding are to be fastened with brass screws to the upper surface of the bottom stringers, to the upper and lower surface of the middle stringers, and to the lower surface of the top stringers, with a clearance of 1 in. between parallel lines of molding. The whole frame is to be painted with three coats of red oxide of iron ground in linseed oil. The panels after having been painted and dried under cover are to be placed in the rack by removing one of the strips of molding, inserting the panels, and replacing the molding strip. This fence is to be located so as to run north and south.

The mixtures of pigments are designed on the volume basis; that is, the mixtures, which are directly comparable, contain the same relative volumes of different pigments. For purposes of mixing, however, it is practically necessary to mix the pigments by weight; therefore in the accompanying list of formulas the proportions by weight as well as by volume are given. The pigments are to be mixed with raw oil to which is to be added a known quantity of lead and manganese as linonates; sufficient oil is to be used to give a standard viscosity.

The Committee has received donations of a sufficient quantity of the following pigments:

Number.	Pigment.
101	White Lead, Dutch Process.
102	White Lead, Carter's Process.
103	White Lead, Acme White Lead Company.
104	Zinc Oxide, French Process.
105	Zinc Oxide, American Process.
106	Sublimed Lead.
107	Zinc Lead White.
108	Silica.
109	Asbestine.
110	China Clay.
111	Calcium Carbonate.
112	Calcium Sulphate.
113	Barytes.

Since silica, asbestine, China clay, calcium carbonate, calcium sulphate and barytes are never used alone as pigments for oil paints, the number of single pigment paints is reduced to seven.

The following list gives the mixtures of pigments to be tested:

Number.	PRIMARY PIGMENTS.
101	White Lead, Dutch Process.
102	White Lead, Carter's Process
103	White Lead, Acme White Lead Company.
104	Zinc Oxide, French Process.
105	Zinc Oxide, American Process.
106	Sublimed Lead.
107	Zinc Lead White.

BINARY COMPOSITION.*

Number.	Percentage by Volume.	Percentage by Weight.	
201	White Lead 80, Zinc Oxide 20	83	17
202	" " 80, Sublimed Lead 20	81	19
203	" " 80, Silica 20	91	9
204	" " 80, Asbestine 20	90	10
205	" " 80, China Clay 20	91	9
206	" " 80, Calcium Carbonate 20	91	9
207	" " 80, Calcium Sulphate 20	92	8
208	" " 80, Barytes 20	86	14

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*Unless otherwise specified, the zinc oxide used shall be American Process.

BINARY COMPOSITION. *(Continued.)*

Number.	Percentage by Volume.	Percentage by Weight.	
209	White Lead 50, Zinc Oxide 50*	55	45
210	" " 50, Sublimed Lead 50	51	49
211	" " 50, Silica 50	71	29
212	" " 50, Asbestine 50	70	30
213	" " 50, China Clay 50	71	29
214	" " 50, Calcium Carbonate 50	71	29
215	" " 50, Calcium Sulphate 50	74	26
216	" " 50, Barytes 50	61	39
217	" " 60, Zinc Oxide 40	64	36
218	" " 60, Zinc Oxide 40*	64	36
219	" " 60, Sublimed Lead 40	61	39
220	Zinc Oxide 50, White Lead 50	45	55
221	" " 50, Sublimed Lead 50	46	54
222	" " 50, Silica 50	67	33
223	" " 50, Asbestine 50	66	34
224	" " 50, China Clay 50	67	33
225	" " 50, Calcium Carbonate 50	67	33
226	" " 50, Calcium Sulphate 50	70	30
227	" " 50, Barytes 50	56	44
228	Subl. Lead 50, Silica 50	71	29
229	" " 50, Asbestine 50	69	31
230	" " 50, China Clay 50	71	29
231	" " 50, Calcium Carbonate 50	71	29
232	" " 50, Calcium Sulphate 50	72	28
233	" " 50, Barytes 50	60	40
234	Zinc Oxide 60, White Lead 40	55	45
235	Zinc Oxide* 60, White Lead 40	55	45

TERNARY COMPOSITION.

Number.	Volume.	Percentage by Weight.
301	White Lead ⅓, Zinc Oxide ⅓, Sublimed Lead ⅓	36–29–35
302	" " ⅓, " " ⅓, Silica ⅓	45–37–18
303	" " ⅓, " " ⅓, Asbestine ⅓	44–36–20
304	" " ⅓, " " ⅓, China Clay ⅓	45–37–18
305	" " ⅓, " " ⅓, Calcium Carbonate ⅓	45–37–18
306	" " ⅓, " " ⅓, Calcium Sulphate ⅓	46–38–16
307	" " ⅓, " " ⅓, Bartyes ⅓	40–33–27
308	" " ½, " " ¼, Sublimed Lead ¼	53–25–22
309	" " ½, " " ¼, Silica ¼	62–25–13
310	" " ½, " " ¼, Asbestine ¼	62–25–13
311	" " ½, " " ¼, China Clay ¼	62–25–13
312	" " ½, " " ¼, Calcium Carbonate ¼	62–25–13

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* French Zinc Oxide.

TERNARY COMPOSITION. (*Continued.*)

Number.	Volume.	Percentage by Weight.
313	White Lead ½, Zinc Oxide ¼, Calcium Sulphate ¼	63-26-11
314	" " ½, " " ¼, Barytes ¼	58-23-19
315	" " ¼, " " ½, Sublimed Lead ¼	28-45-27
316	" " ¼, " " ½, Silica ¼	33-54-13
317	" " ¼, " " ½, Asbestine ¼	33-54-13
318	" " ¼, " " ½, China Clay ¼	33-54-13
319	" " ¼, " " ½, Calcium Carbonate ¼	33-54-13
320	" " ¼, " " ½, Calcium Sulphate ¼	33-55-12
321	" " ¼, " " ½, Barytes ¼	30-50-20
322	" " ¼, " " ¼, Sublimed Lead ½	27-22-51
323	" " ¼, " " ¼, Silica ½	38-31-31
324	" " ¼, " " ¼, Asbestine ½	38-31-31
325	" " ¼, " " ¼, China Clay ½	38-31-31
326	" " ¼, " " ¼, Calcium Carbonate ½	38-31-31
327	" " ¼, " " ¼, Calcium Sulphate ½	40-32-28
328	" " ¼, " " ¼, Bartytes ½	32-26-42
329	Subl. Lead ⅓, Zinc Oxide ⅓, Silica ⅓	44-37-19
330	" " ⅓, " " ⅓, Asbestine ⅓	44-37-19
331	" " ⅓, " " ⅓, China Clay ⅓	44-37-19
332	" " ⅓, " " ⅓, Calcium Carbonate ⅓	44-37-19
333	" " ⅓, " " ⅓, Calcium Sulphate ⅓	45-39-16
334	" " ⅓, " " ⅓, Barytes ⅓	39-34-27
335	" " ½, " " ¼, Silica ¼	61-26-13
336	" " ½, " " ¼, Asbestine ¼	61-26-13
337	" " ½, " " ¼, China Clay ¼	61-26-13
338	" " ½, " " ¼, Calcium Carbonate ¼	61-26-13
339	" " ½, " " ¼, Calcium Sulphate ¼	62-26-12
340	" " ½, " " ¼, Barytes ¼	56-24-20
341	" " ¼, " " ½, Silica ¼	32-54-14
342	" " ¼, " " ½, Asbestine ¼	32-54-14
343	" " ¼, " " ½, China Clay ¼	32-54-14
344	" " ¼, " " ½, Calcium Carbonate ¼	32-54-14
345	" " ¼, " " ½, Calcium Sulphate ¼	33-55-12
346	" " ¼, " " ½, Barytes ¼	29-50-21
347	White Lead ⅓, Subl. Lead ⅓, Silica ⅓	42-40-18
348	" " ⅓, " " ⅓, Asbestine ⅓	42-40-18
349	" " ⅓, " " ⅓, China Clay ⅓	42-40-18
350	" " ⅓, " " ⅓, Calcium Carbonate ⅓	42-40-18
351	" " ⅓, " " ⅓, Calcium Sulphate ⅓	43-41-16
352	" " ⅓, " " ⅓, Barytes ⅓	38-36-26
353	" " ½, " " ¼, Silica ¼	60-28-12
354	" " ½, " " ¼, Asbestine ¼	60-28-12
355	" " ½, " " ¼, China Clay ¼	60-28-12
356	" " ½, " " ¼, Calcium Carbonate ¼	60-28-12

TERNARY COMPOSITION. (*Continued.*)

Number.	Volume.	Percentage by Weight.
357	White Lead ½, Subl. Lead ¼, Calcium Sulphate ¼	60–29–11
358	" " ½, " " ¼, Barytes ¼	55–27–18
359	" " ¼, " " ½, Silica ¼	30–58–12
360	" " ¼, " " ½, Asbestine ¼	30–58–12
361	" " ¼, " " ½, China Clay ¼	30–58–12
362	" " ¼, " " ½, Calcium Carbonate ¼	30–58–12
363	" " ¼, " " ½, Calcium Sulphate ¼	30–58–12
364	" " ¼, " " ½, Barytes ¼	28–52–20
365	" " ¼, " " ¼, Silica ½	36–35–29
366	" " ¼, " " ¼, Asbestine ½	36–35–29
367	" " ¼, " " ¼, China Clay ½	36–35–29
368	" " ¼, " " ¼, Calcium Carbonate ½	36–35–29
369	" " ¼, " " ¼, Calcium Sulphate ½	38–36–26
370	" " ¼, " " ¼, Barytes ½	30–29–41

QUATERNARY COMPOSITION.

401.	Volume.	Weight.
White Lead	¼	31
Zinc Oxide	¼	25
Subl. Lead	¼	30
Silica	¼	14

402	Volume.	Weight.
White Lead	¼	31
Zinc Oxide	¼	25
Subl. Lead	¼	30
Asbestine	¼	14

403	Volume.	Weight.
White Lead	¼	31
Zinc Oxide	¼	25
Subl. Lead	¼	30
China Clay	¼	14

404	Volume.	Weight
White Lead	¼	31
Zinc Oxide	¼	25
Subl. Lead	¼	30
Calc. Carb.	¼	14

405	Volume.	Weight.
White Lead	¼	31
Zinc Oxide	¼	26
Subl. Lead	¼	31
Calc. Sulph	¼	12

406	Volume.	Weight.
White Lead	¼	29
Zinc Oxide	¼	24
Subl. Lead	¼	28
Barytes	¼	19

The Committee has received as donations sufficient linseed oil for grinding these pigments and cans for keeping them. The paints are being ground in the laboratory of Pratt Institute of Brooklyn under the direction of Professor Rogers. Offers have been made by two railroads to have the paints applied in their shops.

Respectfully submitted on behalf of the Sub-Committee,

P. H. WALKER,
Chairman, Sub-Committee J.

DISCUSSION

Mr. Rinald. MR. C. D. RINALD.—Every year thus far our Committee on Preservative Coatings has presented us with one or more sub-committees. Their activity serves to render its reports more valuable and, it must be admitted, harder to digest, because more complicated.

This year I have been pleasantly surprised by the arrival of a Sub-Committee on Vehicles. Too much exclusive attention has been given thus far to pigments, the solid constituents of paint. True, they offer a tangible hold to the chemist for conclusions based on analysis and on those ingenious tests described in our previous Proceedings. When we turn to the liquid constituents of paint, the so-called vehicle, which often consists of a mixture of various liquids and solutions, chemistry is not equally available.

Without any desire to criticise, or even discuss, the substance of to-day's report on vehicles, I simply want to suggest that such tests will prove more useful if conducted along physical, perhaps even empirical, rather than chemical lines. Paint makers will understand me, because it is by such tests that they have years ago found uses for such unorthodox oils as coal oil, corn oil, cottonseed oil, tung oil, fish oil, etc. All these oils, if judiciously used, perform important functions and lead to results which cannot be obtained otherwise. I think I can make this clear to all of you who are paint users, by reminding you of the saponifying and oxidizing action of red lead upon linseed oil, as shown by their mixture hardening in the can over night. The practice formerly was to mix only enough of both each morning to last during the day. Now you buy your red lead paint ready mixed in barrels, and your chemists find that it answers their tests for pure red lead and pure linseed oil. I have also been assured that it gives no trouble from settling,—i. e., hard sediment *does not* form in the bottom of the barrels,—and that it dries as hard and protects as much as heretofore. This shows how important physical advantages may be

Mr. Rinald.

obtained by the judicious admixture of oils, which conform to none of the rules laid down by the chemist, who quite correctly finds each single one of them unfit to act as a paint oil. All of which seems to me to indicate that it would be most important to study the action of various oils and solutions upon each other when combined into a vehicle, and the reactions between the latter and the pigment in the paint. I have spent a good deal of time examining the paint fence at Ventnor and I am not the only one here who thinks that some of those pigments which theoretically are "inhibitors" show up worse than others termed "accelerators." All of them were ground alike in linseed oil. I should like to have our Committee on Preservative Coatings, in future tests, consider the inclusion of pigments ground in vehicles specially prepared for them, such as the Sub-Committees on Varnish and on Vehicles ought to be able to suggest.

REPORT OF COMMITTEE D-4 ON STANDARD TESTS FOR ROAD MATERIALS.

During the past year the Committee on Standard Tests for Road Materials has worked along the lines planned at the last annual meeting, which were described in its report for 1910. A number of special sub-committees on bituminous paving materials have done valuable work in investigating existing methods and devising new methods for the examination of such materials. The reports of these sub-committees have been considered by the Committee, which is now prepared to recommend the following four methods for adoption by the Society as provisional:

(1) Proposed Provisional Method for the Determination of Soluble Bitumen.
(2) Proposed Provisional Method for the Determination of the Penetration of Bitumen.
(3) Proposed Provisional Method for the Determination of the Loss on Heating of Oil and Asphaltic Compounds.
(4) Proposed Provisional Method of Sizing and Separating the Aggregate in Asphalt Paving Mixtures.

The Committee, in presenting the proposed Provisional Method for the Determination of Soluble Bitumen, wish it understood that they do not recommend it as the best for general use, as it is longer and in many cases gives no better results than other more expeditious methods, but only as a method to be resorted to in case of dispute, as it seems to have the widest range of applicability of any of the methods considered. Moreover, they wish it to be understood that with some classes of materials the method will show a lower percentage of soluble bitumen than many of the shorter methods.

The Committee also wish to state that no great degree of refinement is to be expected from the proposed Provisional Method for the Determination of the Loss on Heating of Oil and Asphaltic Compounds, owing to the fact that no absolutely satisfactory standard oven has yet been found. This matter will be given careful attention during the coming year, and it is hoped that an oven will be found or developed which will be an improvement over the one at present recommended.

The proposed provisional methods above mentioned are appended to this report.

While a number of the special sub-committees do not feel justified, from their work during the past year, in recommending methods of testing for adoption as standard, the results obtained by the Sub-Committee on Distillation are considered of such value that they are appended to this report in the form in which they were submitted to the Committee.

Respectfully submitted on behalf of the Committee,

L. W. PAGE,
Chairman.

PRÉVOST HUBBARD,
Secretary.

NOTE.—The four Proposed Provisional Methods referred to above were adopted by letter ballot of the Society on August 21. 1911, and follow this report (pages 245–249).—ED.

APPENDIX.

REPORT OF SUB-COMMITTEE ON DISTILLATION.

The Sub-Committee on Distillation begs to report as follows:

The work consisted in distilling three standard samples by four methods. The properties of these samples, as determined in the three laboratories which made the tests, are given in Table I.

The detailed results are given in Tables II, III and IV, and a summary of the averages of the fractions expressed as percentage by weight in Table V. Table VI shows the slight effect of variations in the dimensions of an Engler flask on the results. Table VII gives a comparison of the specifications for the thermometers that were used by the different members of the Committee. Fig. 1 shows the apparatus set up for the distillation test, and gives the detailed dimensions.

Table I.—Properties of Three Standard Samples.

Letter.	Laboratory.	Analyst.	Specific Gravity, 25°/25° C.			Free Carbon, per cent.		
			No. 9	No. 15	No. 18	No. 9	No. 15	No. 18
A	Office of Public Roads, Washington, D. C.	P. Hubbard	0.9110	1.1110	1.2110	0.19	0.91	18.79
B	Barrett Manufacturing Company, Boston, Mass.	P. P. Sharples ..	0.9120	1.1137	1.2048	0.43	1.22	17.01
C	United Gas Improvement Company, Philadelphia, Pa..	W. H. Fulweiler	0.9177	1.1165	1.2090	0.22	1.90	19.80

The Committee did not feel warranted from the work done so far in recommending the adoption as standard of any of the methods used, but felt that the close agreement by the three laboratories with the method using the 250-cc. Engler flask, which was tentatively proposed last year, warranted its serious consideration and further study as a possible standard.

The possibility of securing in this country flasks of specified dimensions, the slight effect of variations in size of flasks on the

TABLE II.—STANDARD SAMPLE NO. 9.

Figures given are average ± one-half variation between maximum and minimum. Laboratory B reported average values only.

METHOD	Office of Public Roads.			American Society of Civil Engineers.			American Railway Engineering Association.			Proposed Tentative Method.		
USING	750-cc. Glass Retort.			1800-cc. Iron Retort.			250-cc. Glass Retort.			250-cc. Engler Flask.		
LABORATORY	A	B	C	A	B	C	A	B	C	A	B	C
PER CENT. BY WEIGHT.												
Start to 110° C.	0.00	0.00	0.32± 0 21	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
110° " 170°	0.22±0.17	0.37	0.75± 0.07	0.25±0 05	0.02	0 00	0.00	0.00	0.00	0.00	0 00	0.00
170° " 235°	3.85±0.85	0.80	6.23± 3.30	0.12±0.07	0.06	0.80±0.10	0.77±0.1	0.00	0.00	0.07±0.10	0.55	0.00
235° " 270°	20.10±0.60	1.46	23.51± 5.31	0.22±0.17	0.17	2.45±0.24	2.27±0.7	0.50	1.21±0.19	0.83±0.15	0.87	0.55±0.12
270° " 315°	(*)	19.52	48.00±12.60	18.70‡	13.53	6.27±0.60	14.53±2.1	8.00	9.69±1.71	17.37±1.00	17.32	17.62±0.23
Residue	75.65+1.75	77.85†	20.27± 4.25	80.70‡	86.22†	89.55±0.45	81.93±1.6	91.50†	88.75±1.50	81.53±1.25	81.26†	81.50±0.20
Total	99.82±0.12	100.00	99.13± 0.35	99.99	100.00	99.07±0.50	99.51±0.45	100.00	99.65±0.13	99.80±0.15	100.00	99.67±0.18
PER CENT. BY VOLUME.												
Start to 110° C.	0.0	0.0	0.37± 0.2	0 0	0 00	0.00	0.0	0.00	0 00	0 0	0.0	0.0
110° " 170°	0.2±0.16	0.3	0.84± 0.1	0.3+0 0	0.02	0.00	0.0	0.00	0.00	0.0	0.0	0.0
170° " 235°	4 2±0.70	0.9	6.37± 3.4	0.2+0.1	0.05	0.91±0.15	0.8±0.15	0.00	0.00	0.1±0.15	0.5	0.0
235° " 270	22.0±0.60	1.6	25.02± 5.6	0.3+0.2	0.18	2.70±0.80	2.6±1.85	0.25	1.39±0 2	0.8±0 20	1.0	0.6±0.21
270° " 315°	(*)	17.4	50.20±13.0	20 3‡	14.74	6.89±0.70	15.9±2.25	8.66	10.45±1.6	19.0+0.95	18.5	18.8±0.51
Total Distillate	26.4±1.45	20.2	82.80± 4 2	21.1	14.99	10.50±0.4	19.3±1.5	8.91	11.84±1.0	19.9±0.85	20.0	20.4±0.45
SPECIFIC GRAVITY, 25°/25°C.												
Start to 110° C.	......	...	0.8105	......	...	......	......	...	......	......	...	......
110° " 170°	0.8328	...	0.8215	0 7938	...	......	......	...	......	......	..	......
170° " 235°	0 8346	...	0.8436	0.8090	...	......	0.8290	...	......	......	...	......
235° " 270°	0.8536	...	0.8605	0.8299	...	0.8337	0 8392	...	0.8369	0.8161	...	......
270° " 315°	(*)	...	0.8782	0.8539	...	0.8465	0.8511	...	0.8503	0.8502	...	0.8492
Residue	......	...	1.1470	......	...	0.9992	......	...	0.9220	......	...	0.9222
FREE CARBON IN RESIDUE, PER CENT.	0.13	...	3.20	0.13	...	0.26	0.16	...	0.20	0.17	...	0.24
CALCULATED DUE TO CONCENTRATION, PER CENT.	0 25	...	1.82	0 24	...	0.25	0.23	...	0.25	0.23	...	0.26
DIFFERENCE	−0.12	...	+1.38	−0.11	...	+0.01	−0.07	...	−0.05	−0.06	...	−0.02
TIME, MINUTES	79±18	90	99±6	150±30	120	76±5	61±9	45	17±1	51±8	45	21±2

* Distillation carried to 270° C. only. † Residue determined by difference. ‡ Only one test available for these fractions.

TABLE III.—STANDARD SAMPLE NO. 15.

Figures given are average ± one-half variation between maximum and minimum. Laboratory B reported average values only.

METHOD	Office of Public Roads.			American Society of Civil Engineers.			American Railway Engineering Association.			Proposed Tentative Method.		
USING	750-cc. Glass Retort.			1800-cc. Iron Retort.			250-cc. Glass Retort.			250-cc. Engler Flask.		
LABORATORY	A	B	C	A	B	C	A	B	C	A	B	C
PER CENT. BY WEIGHT.												
Start to 110° C.	0.00	0.20	0.29	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
110° " 170°	0.23±0.10	0.85	1.44	0.05±0.05	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
170° " 235°	21.53±2.45	16.21	14.61	0.80±0.40	0.60	0.00	1.63±0.6	0.40	0.35	0.07±0.10	0.68	0.37
235° " 270°	20.80±2.20	8.36	24.06	10.70±0.75	6.83	4.09	10.93±1.4	4.50	4.39	11.90±0.25	11.56	7.62
270° " 315°	(*)	16.22	15.73	42.80±3.90	34.30	27.59	25.50±1.8	24.00	25.67	26.97±0.30	28.66	26.41
Residue	57.13±1.70	58.16†	43.31	43.50±3.20	58.27†	67.35	61.80±2.4	71.10†	69.45	60.87±0.30	59.10†	65.22
Total	99.69±0.20	100.00	99.24	97.85±0.45	100.00	99.03	99.86±0.4	100.00	99.62	99.81±0.15	100.00	99.62
PER CENT. BY VOLUME.												
Start to 110° C.	0.0	0.20	0.31	0.0	0.0	0.00	0.0	0.00	0.00	0.0	0.0	0.0
110° " 170°	0.3±0.2	1.00	1.52	0.1±0.05	0.0	0.00	0.0	0.00	0.00	0.0	0.0	0.0
170° " 235°	24.7±2.8	19.6	14.93	1.0±0.55	0.75	0.00	2.0±1.00	0.26	0.36	0.1±0.10	1.0	0.6
235° " 270°	23.3±2.6	10.0	23.88	12.1±0.90	8.15	4.22	12.8±2.35	4.70	4.50	13.8±0.15	13.5	8.7
270° " 315°	(*)	19.6	15.72	48.0±4.50	40.84	27.95	29.5±2.00	23.70	26.25	30.9±0.10	33.5	29.8
Total Distillate	48.3±1.9	50.4	55.91	61.2±4.2	49.74	32.17	44.3±3.6	28.66	31.11	44.8±0.10	48.0	39.1
SPECIFIC GRAVITY, 25°/25°C.												
Start to 110° C.	......	...	0.9550	......	...	......	......	...	......	......	...	......
110° " 170°	0.9675	...	0.9637	......	...	......	......	...	......	......	...	......
170° " 235°	0.9854	...	0.9900	0.9546	...	......	0.9743	...	0.9650	......	...	......
235° " 270°	1.0000	...	1.0188	0.9852	...	0.9783	0.9845	...	0.9772	0.9836	...	0.9903
270° " 315°	(*)	...	1.0422	1.0075	...	0.9995	0.9982	...	0.9893	0.9957	...	1.0039
Residue	......	...	1.2310	......	...	1.1920	......	...	1.1760	......	...	1.1700
FREE CARBON IN RESIDUE, PER CENT.	5.19	...	8.60	8.12	...	6.60	2.80	...	5.50	3.34	...	3.80
CALCULATED DUE TO CONCENTRATION, PER CENT.	1.59	...	4.39	1.82	...	2.82	1.47	...	2.74	1.50	...	2.77
DIFFERENCE	+3.60	...	+4.21	+6.30	...	+3.78	+1.33	...	+2.76	+1.84	...	+1.03
TIME, MINUTES	90±30	90	75	120	150	139	64±10	45	46	64±4	45	42±2

* Distillation carried to 270° C. only. † Residue determined by difference.

TABLE IV.—STANDARD SAMPLE NO. 18.

Figures given are average ± one-half variation between maximum and minimum. Laboratory B reported average values only.

METHOD	Office of Public Roads.			American Society of Civil Engineers.			American Railway Engineering Association.			Proposed Tentative Method.		
USING	750-cc. Glass Retort.			1800-cc. Iron Retort.			250-cc. Glass Retort.			250-cc. Engler Flask.		
LABORATORY	A	B	C	A	B	C	A	B	C	A	B	C
PER CENT. BY WEIGHT.												
Start to 110° C.	0.03±0.05	0.00	0.52±0.20	0.10±0.10	0.00	0.00	0.00	0.00	0.00	0.01±0.01	0.00	0.00
110° " 170°	1.36±0.50	0.07	3.06±1.80	0.25±0.15	0.20	0.15±0.15	0.18±0.16	0.00	0.00	0.01±0.01	0.00	0.00
170° " 235°	17.43±2.20	14.80	13.39±0.33	5.75±0.95	3.54	2.84±1.40	5.90±1.60	3.40	2.84±0.40	6.40±0.35	6.65	5.04±0.45
235° " 270°	9.80±1.90	6.64	12.78±4.00	11.65±0.05	11.70	11.41±1.90	10.33±9.48	9.40	9.16±0.55	9.07±0.60	9.06	9.87±0.65
270° " 315°	(*)	10.47	10.57±0.95	11.60±1.50	12.42	12.66±0.41	10.37±0.60	9.60	11.40±0.16	10.10±0.30	9.14	10.25±0.35
Residue	70.80±1.95	68.02†	58.92±5.40	70.10±0.80	72.14†	72.00±4.00	73.07±1.55	77.60†	76.45±1.15	74.10±0.20	75.15†	74.62±0.45
Total	99.42±0.35	100.00	99.24±0.56	99.45±0.25	100.00	99.06±0.06	99.85±0.15	100.00	99.85±0.06	99.60±0.15	100.00	99.78±0.10
PER CENT. BY VOLUME.												
Start to 110° C.	0.1±0.1	0.0	0.64±0.3	0.2±0.15	0.00	0.00	0.0	0.00	0.00	0.02±0.02	0.0	0.0
110° " 170°	1.9±0.6	0.2	3.66±2.0	0.4±0.20	0.25	0.18±0.2	0.2±0.17	0.00	0.00	0.02±0.02	0.0	0.0
170° " 235°	21.0±2.7	13.2	15.95±1.5	6.9±1.10	4.48	3.41±1.5	7.6±2.10	4.21	3.45±0.25	8.00±0.60	8.0	6.5±0.4
235° " 270°	11.6±2.5	7.6	14.91±5.0	14.0±0.15	13.85	13.65±2.0	12.6±1.00	10.84	10.72±1.35	11.10±0.70	11.5	11.5±0.7
270° " 315°	(*)	11.4	11.72±1.2	13.6±1.50	14.29	14.61±0.5	12.6±0.55	11.56	12.23±0.51	12.20±0.25	10.0	11.9±0.3
Total Distillate	34.6±2.15	32.4	46.88±6.0	35.1±0.30	32.87	31.85±1.6	33.0±1.80	26.61	26.40±0.50	31.30±0.40	29.5	29.9±0.6
SPECIFIC GRAVITY, 25°/25°C.												
Start to 110° C.	......	...	0.9862	......	...	......	......	...	......	......	...	......
110° " 170°	0.9850	...	1.0095	0.9627	...	......	......	...	......	......	...	......
170° " 235°	1.0150	...	1.0146	1.0210	...	1.0020	1.023	...	0.9968	1.024	...	1.0140
235° " 270°	1.0530	...	1.0357	1.0340	...	1.0176	1.016	...	1.0197	1.015	...	1.0326
270° " 315°	(*)	...	1.0785	1.0380	...	1.0476	1.043	...	1.0267	1.043	...	1.0401
Residue	......	...	1.2850	......	...	1.2800	......	...	1.2685	......	...	1.2770
FREE CARBON IN RESIDUE, PER CENT.	30.45	...	39.10	30.62	...	30.3	27.13	...	29.9	26.37	...	30.30
CALCULATED DUE TO CONTRATION, PER CENT.	26.55	...	33.60	26.81	...	27.5	25.73	...	25.9	25.39	...	26.50
DIFFERENCE	+3.90	...	+5.5	+3.81	...	+2.8	+1.40	...	+4.0	+0.98	...	+3.80
TIME, MINUTES	96±6	105	84	100±20	210	102±10	93±10	50	35±2	58±14	50	38±1

* Distillation carried to 270° C. only. † Residue determined by difference.

TABLE V.—COMPARISON OF FRACTIONS EXPRESSED AS PERCENTAGE BY WEIGHT.

STANDARD SAMPLE NO. 9.

METHOD	Office of Public Roads.			American Society of Civil Engineers.			American Railway Engineering Association.			Proposed Tentative Method.		
USING	750-cc. Glass Retort.			1800-cc. Iron Retort.			250-cc. Glass Retort.			250-cc. Engler Flask.		
LABORATORY	A	B	C	A	B	C	A	B	C	A	B	C
TIME, MINUTES	79	90	99	150	120	76	61	45	17	51	45	21
PER CENT. BY WEIGHT.												
Start to 110° C.	0.00	0.00	0.32	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
110° " 170°	0.22	0.37	0.75	0.25	0.02	0.00	0.00	0.00	0.00	0.00	0.00	0.00
170° " 235°	3.85	0.80	6.23	0.12	0.06	0.80	0.77	0.00	0.00	0.07	0.55	0.00
235° " 270°	20.10	1.46	23.51	0.22	0.17	2.45	2.27	0.50	1.21	0.83	0.87	0.55
270° " 315°	(*)	19.52	48.00	18.70	13.53	6.27	14.53	8.00	9.69	17.37	17.32	17.62
Residue	75.65	77.85†	20.27	80.70	86.22†	89.55	81.93	91.50†	88.75	81.53	81.26†	81.50
Total	99.82	100.00	99.13	99.99	100.00	99.07	99.51	100.00	99.65	99.80	100.00	99.67

STANDARD SAMPLE NO. 15.

LABORATORY	A	B	C	A	B	C	A	B	C	A	B	C
TIME, MINUTES	90	90	75	120	150	139	64	45	46	64	45	42
PER CENT. BY WEIGHT.												
Start to 110° C.	0.00	0.20	0.29	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
110° " 170°	0.23	0.85	1.44	0.05	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
170° " 235°	21.50	16.21	14.61	0.80	0.60	0.00	1.63	0.40	0.35	0.07	0.08	0.37
235° " 270°	20.80	8.36	24.06	10.70	6.83	4.09	10.93	4.50	4.35	11.90	11.56	7.62
270° " 315°	(*)	16.22	15.73	42.80	34.30	27.59	25.50	24.00	25.67	26.97	28.66	26.41
Residue	57.13	58.16†	43.31	43.50	58.37†	67.35	61.80	71.10†	69.45	60.87	59.11†	65.22
Total	99.69	100.00	99.24	97.85	100.00	99.03	99.86	100.00	99.62	99.81	100.00	99.62

STANDARD SAMPLE NO. 18.

LABORATORY	A	B	C	A	B	C	A	B	C	A	B	C
TIME, MINUTES	96	105	84	100	210	102	93	50	35	58	50	38
PER CENT. BY WEIGHT.												
Start to 110° C.	0.03	0.00	0.52	0.10	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00
110° " 170°	1.36	0.07	3.06	0.25	0.20	0.15	0.18	0.00	0.00	0.01	0.00	0.00
170° " 235°	17.54	14.80	13.39	5.75	3.54	2.84	5.90	3.40	2.84	6.40	6.65	5.04
235° " 270°	9.80	6.64	12.78	11.65	11.70	11.41	10.33	9.40	9.16	9.07	9.06	9.87
270° " 315°	(*)	10.47	10.57	11.60	12.42	12.66	10.37	9.60	11.40	10.10	9.14	10.25
Residue	70.80	68.02†	58.92	70.10	72.14†	72.00	73.07	77.60†	76.45	74.10	75.15†	74.62
Total	99.42	100.00	99.24	99.45	100.00	99.06	99.85	100.00	99.85	99.69	100.00	99.78

* Distillation carried to 270° C. only. † Residue determined by difference.

TABLE VI.—COMPARISON OF FLASKS HAVING DIFFERENT SIZES OF BULB AND DIFFERENT HEIGHTS OF TAKE-OFF, WITH SAMPLE OF LIGHT CREOSOTING OIL.

NOMINAL SIZE OF BULB, CC.	150				250				350			
DISTANCE—LIQUID TO TAKE-OFF, CM.	3.8	7.0	9.9	12.8	5.6	7.9	11.2	13.9	6.2	9.3	11.9	15.1
TIME, MINUTES	83	77	79	82	78	76	75	80	78	82	80	84
PER CENT. BY WEIGHT.												
Start to 110° C.	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
110° " 170°	0.25	0.40	0.61	0.68	0.66	0.93	1.11	1.13	1.07	1.40	1.62	1.63
170° " 235°	21.39	21.45	20.55	21.00	21.03	19.24	20.90	20.60	20.35	20.17	22.18	21.17
235° " 270°	30.80	30.55	33.83	33.45	31.16	31.62	31.20	32.33	30.83	33.28	31.62	31.10
270° " 315°	25.55	26.40	23.20	24.80	24.58	25.49	26.00	24.05	25.57	23.65	23.14	25.70
Residue	21.61	20.60	21.40	19.40	22.10	22.20	20.20	21.40	21.72	21.05	20.86	19.81
Total	99.60	99.40	99.59	99.33	99.49	99.48	99.41	99.51	99.54	99.51	99.41	99.31
PER CENT. BY VOLUME.												
Start to 110° C.	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
110° " 170°	0.4	0.5	0.8	0.8	0.6	1.1	1.2	1.6	1.2	1.7	1.9	1.9
170° " 235°	22.8	22.9	22.5	22.2	22.5	20.7	21.3	22.0	21.8	21.5	23.5	22.8
235° " 270°	31.1	31.9	32.9	34.0	31.5	32.2	31.5	32.7	31.3	33.8	32.0	31.5
270° " 315°	25.5	26.5	24.0	25.1	24.5	25.6	25.9	24.0	25.6	23.9	23.2	25.7
Total Distillate	79.8	80.8	80.2	82.1	79.1	79.6	79.9	80.3	79.9	80.9	80.6	81.9
SPECIFIC GRAVITY, 25°/25° C.												
Start to 110° C.	......	......	......	......	......	......	......	......	......	......	......	......
110° " 170°	......	......	......	......	......	......	0.8732	0.8730	0.8940	0.8950	0.8805	0.8809
170° " 235°	0.9430	0.9422	0.9490	0.9458	0.9394	0.9377	0.9422	0.9440	0.9453	0.9429	0.9433	0.9488
235° " 270°	0.9868	0.9860	0.9974	0.9891	0.9922	0.9936	0.9863	0.9881	0.9833	0.9880	0.9881	0.9947
270° " 315°	1.0079	1.0057	1.0116	1.0126	1.0031	1.0291	1.0061	1.0113	1.0195	1.0152	1.0170	1.0150
Residue	1.0878	1.0765	1.0885	1.0810	1.0839	1.0778	1.0776	1.0770	1.0787	1.0843	1.0822	1.0834

NOTE.—Work done by W. H. Fulweiler, The United Gas Improvement Company Laboratory, Philadelphia, Pa. The same thermometer was used throughout.

results, and the quickness and general ease in operation of this method, seemed very promising. The precise dimensions of the flask, such as bulb capacity, height of take-off, size of neck, etc., should be studied further.

The question of the thermometer seems exceedingly important, and while the thermometer specified last year has given satisfactory results, further study of this question is felt to be desirable. The Committee, however, suggests changing from a nitrogen-filled to a carbon-dioxide-filled thermometer.

The calibration of the thermometer was not specified before, and the best method at present appears to be to calibrate the thermometer with the immersion and in the position in which it is to be used. The thermometer is tested by using water, naphthalene and dimethylamine and comparing with a standard thermometer. For check calibration the ice and steam points alone are necessary, since any change in the calibration seems to apply linearly throughout the scale. The Committee suggests checking new thermometers after every third analysis until they become seasoned.

TABLE VII.—COMPARISON OF THERMOMETER SPECIFICATIONS.

	Office of Public Roads.	Barrett Mfg. Co.	United Gas Improvement Co.
Filled under pressure with	Nitrogen	Nitrogen	Nitrogen
Expansion chamber...	At top	At top	At top
Graduated in degrees to	400° C.	400° C.	450° C.
Accuracy required ...	¼° C.	¼° C.	¼° C.
Immersion	....	3 ins.	....
Total length	430 mm.	370 mm.	335 to 350 mm.
Scale length.........	363.5 mm. —23° to 400°C.	267 mm., 0° to 400°	285 to 300 mm. 0° to 450° C.
Diameter of stem....	7.2 to 7.4 mm.	7.8 mm.	6.75 to 7.25 mm.
Length of bulb......	30.2 mm.	11.1 mm.	20 to 22 mm.
Diameter of bulb....	5.7 to 6.7 mm.	6.35 mm.	5.25 to 6.50 mm.
Sensibility..........	15° to 95° in from 3 to 5 sec.	80° in from 3 to 5 sec.	15° to 95° in from 3 to 5 sec.

With the three samples examined, considerable decomposition was apparent above 300° C., and since it is felt that no accurate results are possible under such circumstances, it is therefore suggested to stop the distillation at 300° C.

Many crude, and occasionally some refined materials are found to contain water, which seriously affects the accuracy of the first fractions in a distillation. No easy method of dehydration seems possible, so the Committee suggests that the method given in Section 2 of the Tentative Method for Distillation accompanying this report be used.

The Committee wishes to call attention to the fact that some materials have a tendency to stratify on standing, and suggests that all samples be carefully and thoroughly stirred before taking a portion for analysis.

The Committee would suggest that the determination of the specific gravity of bitumens is not so simple as it appears, and believes that a standard method should be recommended by the Society.

Embodying the above points, the Committee suggests the following tentative method for the distillation of bituminous materials suitable for road treatment.

TENTATIVE METHOD FOR THE DISTILLATION OF BITUMINOUS MATERIALS SUITABLE FOR ROAD TREATMENT.

1. *Sampling.*—The sample as received shall be thoroughly stirred and agitated, warming, if necessary, to insure a complete mixture before the portion for analysis is removed.

2. *Dehydration.*—If the presence of water is suspected, or known, the material shall be dehydrated before distillation. About 500 cc. of the material is placed in an 800-cc. copper still provided with a distilling head connected with a water-cooled condenser. A ring burner is used, starting with a small flame at the top of the still, and gradually lowering it, if necessary, until all the water has been driven off. The distillate is collected in a 200-cc. separatory funnel with the tube cut off close to the stopcock. When all the water has been driven over and the distillate has settled out, the water is drawn off and the oils returned to the residue in the still. The contents of the still shall have cooled to below 100° C. before the oils are returned, and they shall be well stirred and mixed with the residue.

3. *Apparatus.*—The apparatus shall consist of the following standard parts:

(*a*) *Flask.*—The distillation flask shall be a 250-cc. Engler distilling flask, having the following dimensions:

Diameter of bulb	8.0 cm.
Length of neck	15.0 "
Diameter of neck	1.7 "
Surface of material to lower side of tubulature	11.0 "
Length of tubulature	15.0 "
Diameter of tubulature	0.9 "
Angle of tubulature	75°

A variation of 3 per cent. from the above measurements will be allowed.

(*b*) *Thermometer.*—The thermometer shall be of hardened glass, filled with carbon dioxide under pressure and provided with an expansion chamber at the top; it shall read to 450° C., and shall be graduated in single degrees Centigrade, and shall have the following dimensions:

Diameter of stem	6.75	to	7.25	mm.
Length of thermometer	335	"	350	"
Length from 0° to 450° marks	285	"	300	"
Length of bulb	20	"	22	"
Diameter of bulb	5.25	"	6.50	"

It shall rise from 15° to 95° in not less than 3 seconds or more than 5 seconds when plunged into boiling water.

The thermometer shall be set up as for the distillation test, using water, naphthalene and dimethylamine as distilling liquids. The correctness of the thermometer shall be checked at 0° C. and 100° C. after each third distillation until seasoned.

(*c*) *Condenser.*—The condenser tube shall have the following dimensions:

Length of tube	500 mm.
Width of tube	12 to 15 "
Width of adaptor end of tube	20 " 25 "

(*d*) *Stands.*—Two iron stands shall be provided, one with a universal clamp for holding the condenser, and one with a light grip arm with a cork-lined clamp for holding the flask.

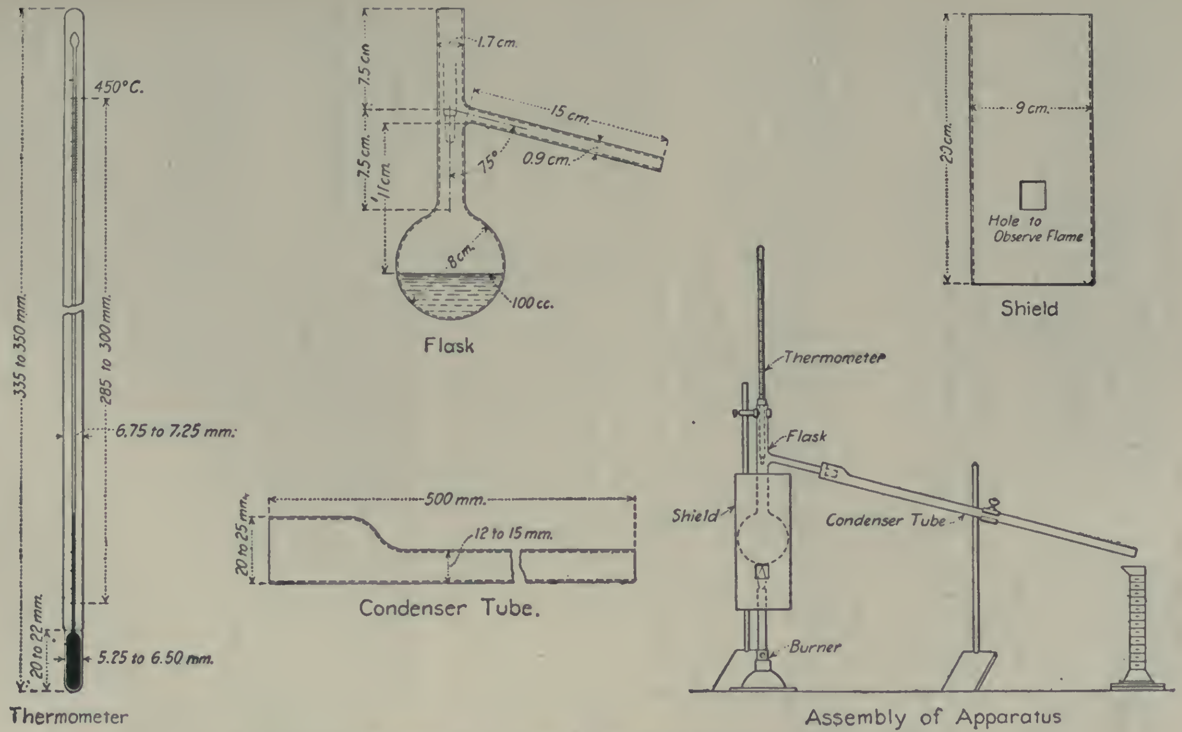

FIG. 1.—Apparatus for Proposed Tentative Method of Distillation.

(*e*) *Burner and Shield.*—A Bunsen burner shall be provided with a tin shield 20 cm. long by 9 cm. in diameter. The shield shall have a small hole for observing the flame.

(*f*) *Cylinders.*—The cylinders used in collecting the distillate shall have a capacity of 25 cc., and shall be graduated in tenths of a cubic centimeter.

4. *Setting up the Apparatus.*—The apparatus shall be set up as shown in Fig. 1, the thermometer being placed so that the top of the bulb is opposite the middle of the tubulature. All connections should be tight.

5. *Method.*—One hundred cubic centimeters of the dehydrated material to be tested shall be placed in a tared flask and weighed. After adjusting the thermometer, shield, condenser, etc., the distillation is commenced, the rate being so regulated that 1 cc. passes over every minute. The receiver is changed as the mercury column just passes the fractionating point.

The following fractions should be reported:

Start of Distillation	to	110° C.
110° C.	"	170° C.
170° C.	"	235° C.
235° C.	"	270° C.
270° C.	"	300° C.
Residue.		

To determine the amount of residue, the flask is weighed again when distillation is complete. During the distillation the condenser tube shall be warmed when necessary to prevent the deposition of any sublimate. The percentages of fractions should be reported both by weight and by volume.

Respectfully submitted on behalf of the Sub-Committee,

W. H. Fulweiler, *Chairman.*

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA, PA., U. S. A.
AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROVISIONAL METHOD FOR THE DETERMINATION OF SOLUBLE BITUMEN.*

ADOPTED AUGUST 21, 1911.

Drying the Sample and Preparing it for Analysis.—It was decided, owing to the great variety of conditions met with in bituminous compounds, that it is impossible to specify any one method of drying that would be satisfactory in every case. It is therefore supposed that the material for analysis has been previously dried either in the laboratory or in the process of refining or manufacture, and that water, if present, exists only as moisture in the hydroscopic form.

The material to be analyzed, if hard and brittle, is ground and spread in a thin layer in a suitable dish (iron or nickel will answer every purpose) and kept at a temperature of 125° C. for one hour. In the case of paving mixtures and road materials, where it is not desirable to crush the rock or sand grains, a lump may be placed in the drying oven until it is thoroughly heated through, when it can be crushed down into a thin layer and dried as above. If the material under examination contains any hydrocarbons at all volatile at this temperature, it will of course be necessary to resort to other means of drying. Tar or oils may be dehydrated by distillation and the water-free distillate returned to the residue and thoroughly incorporated with it.

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* The Committee, in presenting the Provisional Method for the Determination of Soluble Bitumen, wish it understood that they do not recommend it as the best for general use, as it is longer and in many cases gives no better results than other more expeditious methods, but only as a method to be resorted to in case of dispute, as it seems to have the widest range of applicability of any of the methods considered. Moreover, they wish it to be understood that with some classes of materials the method will show a lower percentage of soluble bitumen than many of the shorter methods.

Analysis of Sample.—After drying, from 2 to 15 grams (as may be necessary to insure the presence of 1 to 2 grams of pure bitumen) is weighed into a 150-cc. tared Erlenmeyer flask, and treated with 100 cc. of carbon disulphide. The flask is then loosely corked and shaken from time to time until all large particles of the material have been broken up. It is then set aside for 48 hours to settle. The solution is decanted into a similar flask that has been previously weighed. As much of the solvent is poured off as possible without disturbing the residue. The contents of the first flask are again treated with fresh carbon disulphide, shaken as before, and then put away with the second flask for 48 hours to settle.

The liquid in the second flask is then carefully decanted upon a weighed Gooch crucible, 3.2 cm. in diameter at the bottom, fitted with an asbestos filter, and the contents of the first flask are similarly treated. The asbestos filter is made of ignited long-fiber amphibole, packed in the bottom of a Gooch crucible to the depth of not over ⅛ in. In filtering no vacuum is to be used and the temperature is to be kept between 20° and 25° C. After passing the liquid contents of both flasks through the filter, the residue on the filter is thoroughly washed and the residues remaining in them are shaken with more fresh carbon disulphide and allowed to settle for 24 hours, or until it is seen that a good subsidation has taken place. The solvent in both flasks is then again decanted through the filter and the residues remaining in them are washed until the washings are practically colorless. All washings are to be passed through the Gooch crucible.

The crucible and both flasks are then dried at 125° C. and weighed. The filtrate containing the bitumen is evaporated, the bituminous residue burned, and the weight of the ash thus obtained added to that of the residue in the two flasks and the crucible. The sum of these weights deducted from the weight of substance taken gives the weight of soluble bitumen.

AMERICAN SOCIETY FOR TESTING MATERIALS
PHILADELPHIA PA., U. S. A.
AFFILIATED WITH THE
INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.

PROVISIONAL METHOD FOR THE DETERMINATION OF THE PENETRATION OF BITUMEN.

ADOPTED AUGUST 21, 1911.

The penetration of bitumen shall be the distance expressed in hundredths of a centimeter that a No. 2 needle will pentrate into it vertically without friction at 25° C. under a stated weight applied for a stated length of time, the factors of weight and time being determined as follows:

The material shall first be tested for five seconds under a weight of 100 grams. If this result is less than 10, the penetration shall be determined under a weight of 200 grams applied for one minute; if between 10 and 300, the penetration shall be determined under a weight of 100 grams applied for 5 seconds; if greater than 300, the penetration shall be determined under a weight of 50 grams applied for 5 seconds. In every case the factor of weight and time shall be stated when reporting the penetration, and whenever possible to obtain both readings, the penetration under a 100-gram weight for 5 seconds and under the modified weight and time shall both be reported. When testing material softer than 100 penetration, a containing receptacle not less than 1¼ ins. in diameter shall be used.

It is recommended that the penetration may be determined at 0° C. (32° F.) and 46° C. (114°.8 F.), in addition to the 25° C (77° F.) test.

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PROVISIONAL METHOD FOR THE DETERMINATION OF THE LOSS ON HEATING OF OIL AND ASPHALTIC COMPOUNDS.

ADOPTED AUGUST 21, 1911.

The loss on heating of oil and asphaltic compounds shall be determined in the following manner: Twenty grams of the water-free material shall be placed in a circular tin box with vertical sides, measuring about 2 cm. in depth by 6 cm. in diameter, internal measurement. The penetration of the material to be examined shall, if possible, be determined at 25° C. and the exact weight of the sample ascertained. The sample in the tin box shall then be placed in a hot-air oven (New York Testing Laboratory oven without fan), heated to 163° C. (325° F.) and kept at this temperature for 5 hours. At no time shall the temperature of this oven vary more than 2° C. from 163° C. When the sample is cooled to normal temperature, it shall be weighed and the percentage of loss by volatilization reported. The penetration of the residue shall then, if possible, be determined at 25° C. as upon the original material, and the loss in penetration found by subtracting this penetration from the penetration before heating. In preparing the residue for the penetration test it shall first be heated and thoroughly stirred while cooling.

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PROVISIONAL METHOD OF SIZING AND SEPARATING THE AGGREGATE IN ASPHALT PAVING MIXTURES.

Adopted August 21, 1911.

The method consists of passing the mineral aggregate through several sieves of the following sizes in the order named:

Meshes per linear inch (=2.54 cm.)	Diameter of Wire.	
	Ins.	Mm.
200	0.00235	0.05969
100	0.0045	0.1143
80	0.00575	0.1460
50	0.009	0.22865
40	0.01025	0.26035
30	0.01375	0.34925
20	0.0165	0.4191
10	0.027	0.6858

REPORT OF COMMITTEE D-5 ON STANDARD SPECIFICATIONS FOR COAL.

While not yet prepared to present a final report, the Committee on Standard Specifications for Coal has, during the year, made decided progress toward the accomplishment of this purpose. The different sections of the Committee have brought together a large amount of data having an important bearing upon this subject, and a number of investigations have been conducted with this same purpose in view. More than one thousand samples have been collected from representative beds of coal in different parts of the country, and chemical analyses have been made of these samples with a view of determining not only their proximate and ultimate analyses, but the nature of the ash and volatile matter in these coals, and the extent to which the slagging of the ash and the behavior of the volatile matter may render necessary the consideration of these two factors in the preparation and use of any specification.

A number of experiments have been conducted to determine which of the different methods of collecting samples of coal would yield the most nearly accurate results; and while considerable progress has been made in this respect, it is nevertheless realized that the collection of reliable samples is still the most difficult of satisfactory accomplishment of any of the factors in the establishment of a reliable specification.

Additional information is being collected continually, and all the new information is being tabulated so that it can be turned over to the various sections of the Committee, in the hope that this matter can be thoroughly considered by each of the sub-committees, and the reports of the sub-committees, made at an early date, be turned over to the general committee for consideration and report to the Society at its next annual meeting.

A brief report on the fuel investigations of the Bureau of Mines, by Mr. A. W. Belden, is appended hereto.

Respectfully submitted on behalf of the Committee,

J. A. Holmes,
Chairman.

APPENDIX.

REPORT ON THE FUEL INVESTIGATIONS OF THE BUREAU OF MINES.

The fuel work of the Bureau of Mines may be divided into three general classes: (1) coal inspection work, (2) laboratory investigations, and (3) tests on commercial equipment.

Coal Inspection Work.—A large percentage of the coal used by the different departments of the Government is now purchased on a specification basis. The Bureau has, this year, sampled, analyzed and reported on over 1,000,000 tons of coal of all kinds, valued at $3,200,000; the work of the laboratories is represented by more than 8,700 samples. The cost of this work, per sample, is about $1.54, equivalent to 1.33 cents per ton or 0.48 per cent. of the cost of the coal. The increasing demand for this method of purchase is shown by the following figures based on contracts for the current year. The number of samples will be increased to about 9,000 and the cost per sample reduced to $1.44 (= 1.12 cents per ton) or 0.39 per cent. of the cost of the coal.

Laboratory Investigations.—The fuel laboratory at Pittsburgh is engaged in analysis and determination of the heat value of samples of fuel submitted by the several sections of the Bureau together with mine samples submitted by the engineers of the Bureau, other departments of the Government and state geological surveys. The investigation work of the several laboratories may be summed up as follows: testing the fusibility and clinkering properties of coal ash to determine the effect of the distribution of the ash in the coal and other influences of the component substances upon the clinkering tendency; investigations of the composition of coal; determination of the nature and quantity of volatile matter of various coals at different temperatures; tests of weathering coal; spontaneous combustion; and the commercial value of the petroleums of the United States.

Tests on Commerçial Equipment.—Supplementing the laboratory investigations, tests with larger apparatus or on commercial equipment are being conducted. These investigations may be briefly summarized as follows:

1. Determination of the briquetting qualities of various fuels, anthracite, bituminous, semi-bituminous, and lignite coals, peat and wood refuse from saw-mills.

2. Gas Producer.—Study of the fundamentals of the gas producer on a specially designed equipment.

3. Long-Combustion Chamber.—Study of furnace problems and combustion processes.

4. Cupola Work.—Study of the results of the coking tests. Subsequent work of the cokes in cupola iron melting on a commercial scale led to further investigations. It was found that under certain conditions as much as 52.5 per cent. of the metal charged into the cupola was lost by burning and other variations of more or less serious nature, depending on whether light or heavy cokes were used. It was fully realized that the use of coke in either cupola or blast furnace was dependent on many factors, some of which are known and can be properly taken care of in the preparation of the coke, and others unknown which it was advisable to investigate. The work at present has to do with the determining of the composition of the gases and the measurement of the temperature at different points in various zones of the cupola. It is hoped that the results of this investigation will throw some light on the actual processes in the cupola as well as furnish additional data for physical tests on coke, especially as regards their action when subjected to continually and rapidly changing gases and temperatures.

Respectfully submitted,

A. W. BELDEN.

REPORT OF COMMITTEE D-8 ON WATERPROOFING MATERIALS.

Sub-Committee A, on the investigation of the waterproofing of mortars and concretes through incorporation of foreign substances, can add nothing to its previously reported findings, namely: that while there may be some practical foundation for the claims of the efficiency of some patented materials, laboratory tests do not generally develop any long-time marked advantage in their use over that of normal materials when properly proportioned; and that if additions are desirable on account of poor proportioning or poor granulometric characteristics, natural materials can be used with satisfactory practical results, without tending as do the general run of proprietary compounds to materially reduce the strength of the mortars and concretes.

As far as the work of this sub-committee is concerned, no further investigations are contemplated or considered necessary at this time

Sub-Committee B, on the investigation of waterproofing methods through the use of bituminous materials applied directly or through mediums, inaugurated a series of tests on concrete tanks treated directly by the representatives of several different materials, as reported preliminarily in 1909 and subsequently in 1910. It was recognized that these tests were only preliminary to further and more extensive ones if any satisfactory general system was to be devised. Since 1909 several meetings of the Committee have been held, from which however no satisfactory conclusion has resulted, excepting the general agreement referred to later. The committee meetings, despite the efforts of the Chairman to obtain better cooperation, have been unsatisfactory; each meeting has been attended by practically the same men, and there has been a general lack of keen interest. It must be confessed that it is apparently beyond the ability or inventive genius of the Committee, and this includes the Chairman, to suggest any practical line of investigation which would be generally applicable to the widely varying conditions calling for the use of bituminous waterproofing material. In other words, one system and method and material might easily prove satisfactory under certain conditions of use, and utterly fail under widely different ones, which might be satisfactorily met by another system and method and material. Attempts have been made through correspondence to obtain information from those in charge of important pieces of engineering construction, where bituminous waterproofing processes have been employed, as to their experience, with the expectation that such data would aid the Committee in its work. Very few replies were received, and even from those it could only be concluded that the varying conditions had so important a bearing that the writers either hesitated to commit themselves generally, or were so guarded in their replies that it was evident that tests of time had either not yet demonstrated real value through success, or if they had indicated failure, it was deemed desirable not to so advise the Committee.

However, at a meeting of those of the Committee who answered the call for April 24, 1911, an inspection of the preliminary test tanks was made on April 25, by the Chairman and Messrs. De Wyrall, Walter, Church, and Benner, the last-named representing Mr. De Knight of the Committee.

As originally described in the report for 1909, the tanks, 18 by 12 by 9 ins. deep, were made of 1:3:5 concrete with four $\frac{5}{16}$-in. drainage holes through their bottoms. After satisfactory ageing, they were waterproofed on their interior with the materials and by the operators furnished by six different manufacturers. About twenty-four hours after the application of the waterproofing, a 1-in. protective coating of porous sand-and-cement mortar was spread over the waterproofing application. Each manufacturer was represented by four tanks, numbered respectively 1, 2, 3, and 4. Tanks No. 1 were filled with Astoria gas drip; tanks No. 2 were filled with seepage water from the sump of the New York Subway at Times Square; tanks No. 3 were filled with this same seepage water containing 5 per cent. of kerosene; and tanks No. 4 were filled with this same seepage water rendered alkaline by the addition of about 23 per cent. of caustic potash (lye).

The tanks were filled December 1, 1908, and no renewal of the liquids was made, the tanks being simply covered to delay evaporation, which, owing to the damp atmosphere and lack of ventilation in the testing room,—the Sub-subway at Times Square,—made the conditions sufficiently satisfactory for comparison. Mr. De Wyrall, Secretary of the Committee, regularly inspected the tanks, first every day and thereafter every week until evidence was furnished by the appearance of any contained liquid, through the drainage holes in the bottom of the tanks, that the waterproofing protections had been markedly attacked. The tanks were open at all times to the inspection of any member of the Committee and the Chairman made several personal examinations.

During the first twelve months and to date of the last examination, April 25, 1911, none of the tanks had failed except those filled with gas drip, and all of these failed except one which will be referred to specifically hereafter. One failed in 24 hours, another in 3 days, another in 6 months and 2 days, another in 6 months and 28 days, and the fifth in 8 months and 16 days. They were examined on April 25. The mortar coating was first broken away, with no particular trouble as the gas drip had greatly impaired its strength. The tanks were later broken in half for detailed inspection.

The tank whose protection failed in 24 hours very naturally showed a more apparent fair condition of the waterproofing material, as it had had time to dry out and did not suffer from any lengthy contact with gas drip, which had completely escaped through the drainage holes. The medium, however, was well rotted. There was poor adhesion between the layers, as well as to the concrete itself, and the bulk of the bituminous material had evidently been carried off by the escaped gas drip, the tanks being practically empty when inspected.

The failure of the second tank, which occurred in 3 days, was caused by the washing away of the bituminous coating, which was not reinforced by any medium. The tank was empty when inspected.

In the third tank, which failed in 6 months and 2 days, most of the waterproofing had disappeared. The medium was a sticky paste with no adhesion and the tank was practically empty when inspected.

The failure of the fourth tank, which occured in 6 months and 28 days, showed the waterproofing reduced to a pulp, both the material and the medium, the latter having no more strength than those previously examined. As the tank when examined contained about 1 in. of the drip, it was apparent that while the waterproofing material had been very seriously attacked, possible clogging had occurred, preventing the entire disappearance of the liquid.

The failure of the fifth tank, which occurred in 8 months and 16 days, showed the material markedly soft with no adhesion between the plies or concrete, and the cloth rotted.

The sixth tank, referred to above as not having failed, contained at the time of inspection considerable of the liquid. Upon careful examination it was found that the drainage holes had for some reason, presumably accidental, been omitted, which was of course recognized as possibly impairing the comparative value of this test, though it certainly is not of the vital importance supposed by some members of the Committee, for the reason that whereas in all the tanks that failed, the concrete bottoms showed more or less infusion of the gas drip throughout from the drainage holes outward, this particular tank did not present this appearance, as the gas drip had not penetrated the waterproofing plies.

While some objection has been made by a minority of the Committee to the above statement as a possible endorsement of the efficiency of material and treatment on the sixth tank, no claim was made by these objectors that any of the other tests showed comparative results, and it was generally admitted that gas drip disastrously affected bituminous materials when in actual contact with them. In order to decide quickly the mooted point caused by the absence of drainage holes, an attempt was then made to obtain agreement by repeating these tests under an accelerated plan, but this plan failed owing to the refusal of at least one manufacturer to agree.

In view of the above facts the Chairman assumes to present the report as originally submitted and accepted by the majority of the Committee.

Tanks No. 3, containing seepage water and 5 per cent. of kerosene, were examined. Two of them showed signs of failure, water being found in the fabric and between the plies. The remainder were in good condition. Owing to the indication that even the 5 per cent. of kerosene added to the seepage water was attacking some of the applications, it was decided to empty tanks No. 2 containing seepage water only, which had produced no effect upon the waterproofing material, and to refill these with straight kerosene oil. This has been done and the results are being noted and will be reported later.

This report can at best be considered only as one of progress. If any further work, except the mere recording of results on the balance of the test tanks now under observation, is to be undertaken in the future, a complete reorganization of the Committee is absolutely necessary in the opinion of the Chairman, although it might be possible under different leadership to excite the desired interest heretofore so markedly lacking in the majority of its present membership. The Chairman recognizes the inherent power of the Committee in this matter, and it is of record that action was taken at Atlantic City last year by dropping from the roll all members failing to show proper interest in the work, as evidenced by absence from called meetings. The result of this ruling has been to practically exterminate the majority of the Committee; such interest as shown by attendance at meetings has been confined to some half dozen members, the only ones ever active.

The Committee should be radically reorganized if further effort is to be made to take up work for which it was constituted and preliminary steps in this direction are being taken. If unsuccessful, a request will be made for the discharge of the Committee.

Respectfully submitted on behalf of the Committee,

W. A. AIKEN,
Chairman.

CYRIL DE WYRALL,
Secretary.

REPORT OF COMMITTEE E-1 ON STANDARD METHODS OF TESTING.

The Committee on Standard Methods of Testing submits herewith Proposed Standard Methods for Transverse Tests of Metals, with the recommendation that they be adopted by the Society and made a part of the Standard Methods of Testing adopted by letter ballot of the Society on September 1, 1910.

Respectfully submitted on behalf of the Committee,

GAETANO LANZA,
Chairman.

NOTE.—The Proposed Standard Methods for Transverse Tests of Metals were adopted by letter ballot of the Society on August 21, 1911, and follow this report. They have been embodied in the Standard Methods of Testing as Part III (see Year-Book, 1911, pages 180-182).—ED.

AMERICAN SOCIETY FOR TESTING MATERIALS
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STANDARD METHODS FOR TRANSVERSE TESTS OF METALS.

ADOPTED AUGUST 21, 1911.

1. In the case of cast metals, when transverse tests are to be used to aid in determining the quality of the material, the specimen used shall be cast vertical, shall be $1\frac{1}{4}$ ins. in diameter, and long enough to use a span of at least 15 times the diameter.

It is important that a definite and uniform standard be adopted so that the results may be comparable with each other; hence the diameter specified above (sectional area corresponding to practically 1 sq. in.). The determination of span is at present the subject of international tests to decide upon a definite distance to replace the present standard of 12 ins. It will probably be 16 to 18 ins.

The circular section will best secure a uniform thickness of skin, and thus avoid this irregularity when other sections are employed.

In the case of ductile materials (except in impact tests) transverse tests shall never be used to determine the quality of the material, tensile tests being those suitable for the purpose.

In small round or square bars of ductile material, both the modulus of rupture and the transverse elastic limit vary considerably with the span.

In the case of tests made for determining constants to be used for designing, the specimen shall conform as nearly as possible with the form and size of the piece to be used. Thus, if I- or T-sections are to be used, the specimens shall be of I- or T-section. In the case of flat springs or plate glass, they shall be flat; in the case of timber, rectangular; etc.

It is well known that the modulus of rupture varies with the shape of the section, being very much greater in the case of round than in I-sections. Hence the modulus of rupture suitable for use for one would be entirely unsuitable for the other.

In rolled sections, the smaller ones are subjected to a more thorough working in the process of rolling than the larger.

2. In the case of the "Arbitration Bar" adopted for cast iron, the span has been fixed at 12 ins., but may be extended as above stated. The bar will serve for cast and brittle materials.

In the case of ductile materials, when the modulus of rupture is desired, the span shall generally be less than 12 or 15 times the depth. Exceptions, however, occur, as in flat springs and in some cases in full-size pieces, when the spans and methods of supporting the ends, etc., shall conform to the conditions of service.

3. In the case of cast and brittle metals, the speed of testing shall not exceed 0.2 in. per minute. For other specimens the speed shall be correspondingly low.

4. The preparation of the specimen shall be such that it truly represents the material itself. The introduction of extraneous influences should be avoided as far as the knowledge of the material will permit. Thus, in cast metals no coupons shall be used; cast materials for tests shall go into dry molds standing vertical.

No specimen shall be machined before testing, except when information is specifically desired regarding the strength of such machined specimens.

5. The transverse yield point for ductile materials shall be noted approximately by the drop of the scale beam.

6. If the transverse elastic limit is to be determined for comparison with that obtained in the tensile test, the successive increments of load in the neighborhood of the transverse elastic limit shall be comparatively small, and after each load has been applied and the corresponding deflection measured by means of the deflectometer, the load shall be removed and the deflection measured again to determine the permanent set.

In those cases where the arbitration bar is used for such cast materials as have an elastic limit, the increment of load used near the transverse elastic limit shall be 250 lbs.

It is well known that when the transverse elastic limit is determined, of course by means of a transverse test, the extreme fiber stress at this transverse elastic limit is not the same as that at the tensile elastic limit of the material; and moreover, that it varies with both the section and the span; hence the desirability of comparing the transverse elastic limit with the tensile elastic limit.

7. In the case of ductile materials, the arrangement of the supports shall be such that longitudinal tension in the specimen due to the rigidity of the supports is avoided.

8. In the case of ductile materials, special care shall be used when determining the ultimate load. For this purpose it will be necessary when approaching the ultimate (i. e., the maximum) load, to make the speed of testing slow enough to enable the observer to note the maximum load.

In many cases, as in I- and T-beams, the maximum load can be easily ascertained, while in others, such as round or flat sections with short spans, it may not be possible to determine it exactly; but it will almost always be possible to determine it with sufficient accuracy for all practical purposes.

LIFE HISTORY OF NETWORK AND FERRITE GRAINS IN CARBON STEEL.*

By H. M. Howe.

PART I.—INTRODUCTION.

1. The traveling salesman who has a proper trunk with suitable subdivisions for classifying the many and varied samples which he must needs show, cannot justly be censured on the ground that he burdens himself needlessly by having to transport a trunk in addition to his wares.

Nature has made the constitution of steel and its behavior under heat treatment so complex that, in order to set them forth in such a way that they can be understood and remembered, a special explanatory diagram and a few special names are imperatively needed, in the opinion of all those who have studied the matter most deeply. We have prepared for you a suitably subdivided trunk to carry the wares you need. It is not we but nature that is to blame for the fact that a mere pilgrim's sack will not suffice, and that such a trunk is needed. You practitioners flinch from the labor of learning how to work this trunk. For those of you to whom the subject is of minor importance it may not be worth while to try; it may be better to content yourselves with carrying your wares in a sack for the rest of your work. But those of you closely interested in the subject, and especially the younger of you, will do well to adjust yourselves to the new condition, and learn to use this new tool, lest haply in a few years you in turn be pushed aside by the still younger, who most certainly will understand it, so thoroughly is its use now taught to the students at our technical schools, and so dependent is our literature on a comprehension of it.

2. *General Survey of the Conditions. Very-Low-Carbon Steel.*—When cold this consists (1) of distinct grains of almost carbonless iron called "ferrite," and (2) whatever further small

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* The Ms. of this article was received September 15, 1911. The references to Ewing and Rosenhain were added later.—Ed.

quantity of carbon is present. This carbon is chemically combined with a small part of the iron in the form of the definite carbide, Fe_3C, called "cementite." When this carbide is first formed it is interstratified with a little of the ferrite, to form a zebra-like conglomerate called "pearlite." In very-low-carbon steel the small quantity of pearlite to which the very small quantity of carbon present gives rise may occur as small islands between adjoining grains of the ferrite; though under favorable conditions the cementite of this pearlite tends to separate itself from the ferrite with which it is initially intermixed, and to form little islands of free cementite, the ferrite with which it was originally intermixed coalescing with the neighboring grains of ferrite.

Thus, dead-low-carbon steel of 0.03 per cent. carbon, under such conditions, consists of 99.55 per cent. of ferrite and 0.45 per cent. of cementite. The small quantity of manganese present in such a steel exists in this cementite, in which it replaces a like quantity of iron.

The characteristic structure of ferrite, as shown in Figs. F and G of Row 12, Plate IV (page 386), is that of irregular polygons. Each polygon or grain is a true but ill-formed crystal. That is to say, all its particles have one and the same orientation and cleavage; and the orientation of the particles changes abruptly as we pass from grain to grain. This is shown clearly in Fig. 12, Plate I[*] (page 386). But the exterior form of these grains is irregular, because they interfere with each others' shape. Each prevents its growing neighbors from reaching the regularity of outward form characteristic of our familiar cabinet crystals.

The structure which pearlite itself assumes under favorable conditions is shown in Fig. 13, Plate I.[†] Its occurrence in high-carbon steel, as a kernel within a shell of white cementite, is shown in Fig. 14, Plate I,[‡] while Fig. 15[§] shows the condition which low-carbon steel readily assumes through the divorcing of the pearlite, its ferrite deserting its mate to coalesce with the free ferrite which forms the great bulk of the metal, and the deserted cementite drawing together in the form of white atolls.

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[*] Stead, *Journal, Iron and Steel Institute*, Vol. I, 1898, Fig. 17, after p. 176, x 130.

[†] Heyn, *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, 1904, Fig. 56, x 1650.

[‡] Professor William Campbell, "Blister Steel."

[§] Stead, *Journal, Society of Chemical Industry*, Vol. 22, 1903, Fig. 10, x 500.

3. *Transformations of Ultra-Low-Carbon Steel in Heating and Cooling.*—When ultra-low-carbon steel is heated, on rising past 770° C. it loses its magnetic properties, changing into a different form called "beta" ferrite. This change-temperature is called A2 generically, Ac2 when it refers specifically to rising temperature, and Ar2 when it refers specifically to falling temperature, "c" meaning "chauffant," and "r" "refroidissant" (Osmond).*

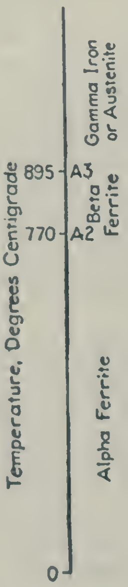

FIG. 1.—Transformations at High Temperatures in Very-Low-Carbon Steel.

When the temperature, in further rising, passes about 895° C., called A3 in Fig. 1, the beta ferrite changes abruptly into "austenite," called also "gamma iron." The properties of this austenite are radically different from those of the ferrite out of which it is formed. Whatever carbon is present dissolves in this austenite, which is what is called a "solid solution." In other words, whereas the steel when cold consisted of a mechanical mixture or conglomerate of ferrite and of the little cementite present, representing the small quantity of carbon present, on rising past 895° C. (A3) these distinct bodies dissolve in each other quite as ice and salt when mixed dissolve in each other.

Most of us are familiar with this melting of ice and simultaneous dissolving of salt, when we bring them into contact with each other in an ice-cream freezer, or by sprinkling salt on the sidewalk for the purpose of cutting the ice there. As the salt and ice merge in each other and so pass from the condition of a conglomerate or mixture to that of a single substance, a solution of salt in water, so do the ferrite and cementite on rising past 895° C. merge and pass from the condition of a conglomerate into that of a single substance, dissolving in each other, with the striking difference that the resultant solution or austenite is solid instead of being liquid as in the case of mixed salt and ice. Save and except that it is solid, it has all the properties which distinguish our common liquid solutions, their complete merging of their several components into a single entity, and their complete indefiniteness of composition, so that different solutions may vary in composition, not like chemical compounds by definite gaps between one another, but by infinitesimal gradations.

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* This change from alpha to beta in heating up, Ac2, and the reverse change from beta to alpha in cooling down, Ar2, begin at nearly the same temperature; but the latter change does not seem to complete itself there.

This austenite remains as austenite with further rise of temperature till the melting point is approached; but on again cooling the change is reversed when the temperature falls past about 895° C., and the austenite splits up into the mixture of ferrite and cementite of which the steel initially consisted. The foregoing applies only to steel very poor in carbon.

4. *Steel Richer in Carbon.*—As the percentage of carbon increases, and with it the percentage of the cementite which contains that carbon, the percentage of pearlite, the fine mechanical mixture of cementite with part of the ferrite, increases extremely rapidly, until when the carbon content reaches about 0.90 per cent. the metal when cold consists exclusively of pearlite, which therefore has a carbon content of 0.90 per cent. And whereas the progressive increase of the carbon content in a series of specimens of steel from 0 to 0.90 per cent. is accompanied by a progressive increase in the proportion of pearlite and decrease in that of ferrite, when the carbon content rises past 0.90 per cent. the steel now consists of a mixture of pearlite with free cementite, as in Figs. C to F of Rows 1 and 2, Plate II (page 386), in which the darker ground mass is pearlite and the white needles and cell walls are cementite. The farther the carbon content rises beyond 0.90 per cent., the greater is the proportion of this free cementite.

5. *The Carbon-Iron Diagram.*—In order to understand the present subject of the effect of heat treatment on the structure of steel, it is unfortunately necessary to understand so much of the carbon-iron diagram as is shown in Fig. 2, in which vertical distances represent temperatures and horizontal ones represent carbon content. In fact, the left-hand axis of this diagram is exactly the same as the vertical line of Fig. 1. Every imaginary

vertical line in the diagram is an isocarb, and the various points on it represent the various temperatures which a specimen of steel of that carbon content passes through or stays at in heating up and cooling down.

Note the distinct regions into which this diagram is divided.

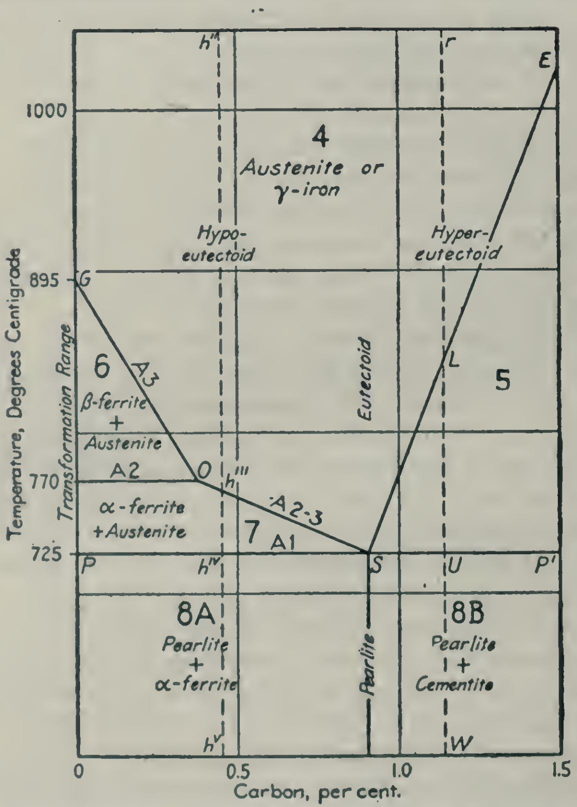

FIG. 2.—The Transformation Range and the Neighboring Parts of the Carbon-Iron Diagram.

In Region 4 the steel normally consists of austenite alone; in Region 8 it consists of a conglomerate or granitic mass, a mixture of pearlite with either ferrite or cementite according to whether it contains less or more than 0.90 per cent. of carbon; whereas in Regions 5, 6, and 7, called jointly *the transformation range*, it consists of a mixture of austenite with either ferrite or cementite.

When steel is heated up, as its temperature rises past the bottom of the transformation range, so that it enters Region 5 or 7, its pearlite changes abruptly into austenite; and with further rise of temperature the austenite thus formed progressively absorbs the ferrite or cementite with which it was initially mixed, completing this absorption at the top of the transformation range, though not instantaneously, so that on entering Region 4 it consists of austenite alone. So when this austenite in turn cools, as it enters the transformation range from above it begins generating ferrite or cementite, according to whether it contains less or more than 0.90 per cent. of carbon, and it continues this generation at such a rate as to bring its own carbon content to 0.90 per cent. when it has reached the bottom of that range. On further cooling, this residual austenite changes abruptly into pearlite, thus completing the cycle, and giving us back the conglomerate or mechanical mixture of pearlite with either ferrite or cementite with which we started.

6. *The transformation*, then, in cooling is the progressive change from the condition of austenite into that of the mixture of pearlite with either ferrite or cementite of which slowly cooled steel consists; and in heating up it is the reverse change to the state of austenite. It may be written thus:

$$\begin{array}{lcl} \textit{Ferrite} + \textit{Cementite} & \rightleftarrows & \textit{Austenite} \\ x\,\alpha Fe + y\,Fe_3C & \rightleftarrows & (x+3y)\gamma Fe\ ,\ Cy \end{array}$$

When *the transformation* is spoken of in this paper, it is this specific transformation that is meant.

In order to understand this diagram better let us follow by means of the line h''-h^V, the cooling of steel containing 0.40 per cent. of carbon in cooling from 1000° C.

At 1000° this steel consists wholly of austenite, the whole of the carbon being dissolved in the whole of the iron. When, in cooling, its temperature reaches the point h''' on the line A2-3, or *OS*, the austenite begins expelling from itself part of its iron in the form of ferrite, preferably into the joints between its adjacent grains, or into its own cleavages, and this expulsion continues as the temperature further falls. As the ferrite thus expelled is nearly or quite free from carbon, and as this residual austenite, thus reduced in quantity, hence contains the whole of carbon present initially, it is of course richer in carbon than the initial austenite. This percentage enrichment of the mother austenite continues with the continuing generation of ferrite, till, when the temperature has fallen to about 725° C., Ar1 or *PSP'*, it reaches 0.90 per cent. which is the carbon content of pearlite. But the austenite still remains austenite. This passage of the steel from h''' to h^{IV} thus has been accompanied by its splitting up from an initially nearly homogeneous mass of austenite alone, into a mixture of austenite with a progressively increasing quantity of ferrite, with consequent progressive enrichment in carbon of that residual austenite, which at h^{IV} reaches the carbon content of pearlite.

The progress of this transformation may be traced in Figs. 24 to 28, Plate I, which show the structure at different points of the same bar of steel on cooling slowly to various points in and near the transformation range.

Figs. 18 to 23, Plate I, show a like series, prepared by heating several separate pieces of this same steel to one and the same high temperature, then allowing each to cool slowly in the furnace to the temperature indicated, and then quenching it in water to preserve the structure reached in the slow cooling. In these cases the cooling was very much slower than in the cases shown in Figs. 24 to 28. As will be explained later, this prolongation of the exposure to these temperatures has allowed the network structure, shown so clearly in Figs. 24 to 28, to break up again.

On cooling past h^{IV} on the line A1, this residual 0.90-per cent. austenite suddenly changes into pearlite, but with no change in the ferrite which it has generated in the passage from h''' to h^{IV}, so that the steel now consists of a conglomerate of (1) ferrite formed in cooling from h''' to h^{IV}, and (2) pearlite formed in cooling past h^{IV}.

This conversion of the austenite into pearlite is accompanied by a great evolution of heat. In an ordinary air cooling, in which because of lag this transformation does not set in till the temperature has passed below that at which it is strictly due, that is, below its equilibrium temperature, this evolution of heat is so rapid as to raise the temperature towards or even to the equilibrium temperature, causing the familiar "recalescence."

With further cooling from h^{IV} down no change occurs which we need notice here, so that the cold steel consists of the familiar mixture of pearlite and ferrite.

When this steel is heated up again, the same changes occur in the reverse order. That is to say, when it rises past h^{IV} the pearlite changes abruptly into austenite; and as the temperature further rises from h^{IV} to h''' this austenite progressively re-absorbs the ferrite which it had expelled in its previous cooling through the transformation range from h''' to h^{IV}, completing this re-absorption on reaching h''', and above this point consisting as before of austenite alone.

7. *Lag.*—This transformation is a lagging one in both its parts, (1) the progressive expulsion of ferrite from the austenite in cooling from A3 to A1, and its re-absorption by that austenite in heating up; and (2) the abrupt change of the residual austenite into pearlite in cooling past A1, and from pearlite into austenite on again rising past A1.

Every such reaction has a certain temperature at which, under existing conditions, it is due to occur, and would occur if equilibrium were exactly complied with. This might be called its normal or equilibrium temperature. But, just as by suitable precautions water may be prevented from beginning to boil at the instant when its temperature reaches 100° C., and from freezing at the instant when its temperature falls to 0°, and may be cooled materially below 0° without actually freezing, so these transformations in solids are very prone to lag. In particular, those which occur in cooling are subject to great "lag," or "undercooling."

Because of this lag it is but natural that the range at which the transformation occurs in cooling is the lower the faster the cooling takes place, and that the range at which it occurs during heating up is the lower the slower the heating. For instance, it is lower in the slow heatings of industrial annealings and of my experiments for this present paper, than in the necessarily rather rapid heatings used for determining the position of the several critical points by means of the retardations which represent them, or in other words the breaks in the time-temperature curve. The presence of any element, like nickel or manganese, which makes the steel sluggish, in like manner lowers the range through which this transformation occurs in cooling.

8. *The hardening of steel* through rapid cooling is a conse-

quence of this lagging tendency. In effect the rapid cooling prevents the transformation from completing itself, so that the rapidly cooled metal, instead of consisting of the mixture of pearlite with ferrite normal for slowly cooled steel, consists of products intermediate between that mixture and the austenite state in which the steel was when in Region 4 before the cooling began. This is proved by an enormous mass of evidence, of which it suffices to mention here (1) that cooling, no matter how rapid, causes no hardening unless, when it begins, the steel is in the condition of austenite, or some intermediate transition form; and (2) that hardening and other effects closely like those caused by rapid cooling are caused by the presence of any element which throws the metal into the condition of the stages intermediate between austenite and the mixture of pearlite and ferrite of which slowly cooled carbon steel consists.

9. *Beta Iron.*—As to the nature of these transition forms opinions are divided. But the great mass of facts before us is explained conveniently by the theory that the beta iron stage, intermediate between that of austenite and of ferrite plus pearlite, is much harder than austenite itself; and that in fact the hardening of common carbon tool-steels, and the stage of greatest red-hardness of high-speed steels, are due to the presence of this intermediate beta iron.

From the facts already before you, that cold carbonless steel consists of ferrite alone; that steel of 0.90 per cent. carbon consists of pearlite alone; and that the austenite of steel of intermediate carbon content in cooling through the transformation range generates within itself enough ferrite to reduce its own carbon content to that of pearlite, 0.90 per cent., you readily infer that the quantity of ferrite thus generated is proportional to the gap between the total carbon content of the steel itself and that of pearlite, 0.90 per cent.

10. *Definitions.*—Steels containing just 0.90 per cent. of carbon, and hence consisting of pearlite alone, are called "eutectoid" steels. Those containing less than this are called "hypo-eutectoid", and those more than this "hyper-eutectoid" steels.

A_1 is the lower boundary of the transformation range.

A_3 is its upper boundary.

A2 is the boundary between the regions in which alpha and beta ferrite respectively exist.

Between 0.37 and 0.90 per cent. of carbon, A2 and A3 occur together; they are therefore spoken of jointly as A2–3.

Grain refining is the breaking up of pre-existing coarse grain, for instance by proper thermal treatment.

11. *The Transformation of Hyper-Eutectoid Steels.*—To fix our ideas let us consider the cooling of a hyper-eutectoid steel containing 1.14 per cent. of carbon, *rLUW*, Fig. 2. When at *r* or any temperature in Region 4, this steel consists of austenite alone. When, in cooling, it reaches the temperature *L* on the line *SE*, this austenite begins generating within itself not ferrite but cementite, and thereby impoverishing itself in carbon because cementite itself contains 6.67 per cent. of carbon. The austenite continues to generate this cementite as the temperature falls further and further, till by the time it has reached temperature *U* on the line *PSP′*, or Ar1, the austenite has reduced its own carbon content to 0.90 per cent., that of pearlite. On cooling past *U*, the residual austenite changes abruptly into pearlite, with great evolution of heat, or in short undergoes the *recalescence.*

No further change which we need notice occurs in the further cooling; and the cold steel consists of a mixture of pearlite together with the cementite which the austenite has generated in the act of reducing its own carbon content to that of pearlite in its cooling from *L* to *U*, that is, in cooling through the transformation range.

When it is again reheated, the pearlite changes abruptly into austenite on rising past the point *U;* and this austenite progressively re-absorbs the cementite which it had generated in the previous cooling down, completing this re-absorption on reaching the temperature *L*, and at higher temperatures in Region 4 consisting of austenite alone.

In short, the behavior of hyper- and hypo-eutectoid steels is parallel, the cementite of the latter replacing the ferrite of the former.

12. *Network or Walled Cell Structure.*—In the act of giving birth to ferrite in cooling through the transformation range, each grain of mother austenite of hypo-eutectoid steel, like mothers

in general, ejects its offspring to its exterior, and thereby concentrates it primarily at its own periphery, that is to say, in and about the joints which separate it from the neighboring grains, and to a less extent in and about its own cleavages. The ultra-microscopic particles of ferrite thus concentrated into these joints later coalesce so as to form a continuous bounding surface or cell-wall, bounding the single grain of mother austenite which had expelled it. Such walled cells are seen in Figs. C, D and E of Rows 5 and 6, Plate III (page 386). Thus, under ideal conditions, the kernel of each walled cell represents a single grain of austenite, and the shell or cell-wall is a party-wall representing the ferrite expelled by contiguous austenite grains during the cooling through the transformation range.

But though much of the ferrite is thus ejected, some of it remains within the mother austenite grains, and gradually coalesces, first into islets large enough to be seen under the microscope, and later into larger and larger islets as time goes on. And some of it is concentrated in the cleavages of the mother austenite, and, later coalescing there into visible masses, discloses the octahedral cleavage or Widmanstättian structure of that austenite. These masses of ferrite within the boundaries of what was a single grain of austenite are easily mistaken for the true party-wall separating true independent austenite grains.

The ferrite network structure is most plainly seen in steel which has cooled at an intermediate rate, as for instance in the air, yet not fast enough to cause true hardening as in quenching in water. (See Principles 12 to 16, Paragraphs 34 to 39, pages 306 to 310, and Proposition 9, Paragraph 62, page 355.)

In slower cooling, the islets of ferrite within the pearlite kernels coalesce more and more, and distract our attention from the ferrite network or cell-walls. Moreover, the ferrite of those walls in turn tends to draw together or coalesce, as drops of mercury at the bottom of a glass of water do, and thus to conceal the network structure; so that in very slowly cooled steel it is often unrecognizable, especially if the quantity of ferrite itself is large. Figs. B to E of Rows 3 to 8, Plates II and III, show us the microstructure of like pieces of like steel cooled at different rates from like temperatures. In those cooled relatively rapidly in air or fire clay from 900° or higher (Rows 5 to 8) the network structure is very manifest; in those cooled extremely slowly in the furnace (Rows 3 and 4) it is hardly distinguishable. Whether it still has a strong influence on the properties of the steel even in such cases remains to be determined. But the probability that it has in those cases in which it stands forth prominently under the microscope is certainly strong enough to warrant a careful study of the question, and, as a first step to that end, a study of the conditions which determine the size of the walled cells, that is, the network size.

In hyper-eutectoid steels this same network structure is plainly seen, shells of free cementite which the austenite has expelled from itself in cooling through the transformation range replacing the ferrite shells of hypo-eutectoid steel, as shown in Figs. C to F of Rows 1 and 2, Plate II.

As this network structure is due to the generation of ferrite or cementite by the mother austenite during its approach to the eutectoid ratio of 0.90 per cent. carbon in its passage down through the transformation range, steel which is initially of this carbon content, and hence generates neither ferrite nor cementite, does not acquire this network structure. It may be in distinct grains, but these grains are not enclosed in a party-wall.

13. *Grains and Walled Cells.*—Here are two units as distinct structurally as a brick and a brick wall, yet habitually confused and given the same name, "grain." Thus many would call each of the several small white *spots* in Figs. B of Rows 3 and 4, Plate II, a "grain," and would also call each of the white-bounded *polygons* in Figs. 24 to 27, Plate I, and in Figs. C, D and E of Rows 5 and 6, Plate III, a "grain"; yet these latter are structurally of a radically distinct order, as is seen on turning to Fig. E of Row 10, Plate IV. The polygons which I have drawn in broken lines in this last figure are evidently the structural representatives of the above-mentioned polygons in Figs. 24 to 27, whereas each of the individual sharp-cornered white islands, "a, a, a," in this same figure is clearly the structural representative of the several white spots in Figs. B of Rows 3 and 4, Plate II. So that those white spots and these white-bounded polygons, though called by the same name, represent structural units of different orders.

Again, one who had before him Fig. 19, Plate I, or Figs. B and C of Row 11, Plate IV, would naturally regard as a "grain" each of the dark masses which is semi-enclosed and well outlined by the discontinuous white spots; but if he had before him only the lower right-hand corner of Fig. C of Row 11, he would be likely to call each of the white spots a "grain"; so that the word "grain" would be applied to structural units of a different order according to whether the observer had the whole or only one part of this figure before him.

For my present purpose it is necessary to distinguish sharply between these units, and hence to propose names that will serve this end at least as a temporary expedient. My first thought was to call the walled polygons "cells" and the spots "grains"; but these grains are themselves in a sense "cells," for though many animal cells like my walled polygons consist of a nucleus and an envelope, yet this envelope is not necessary to cell-hood. Then, again, there are the cells in steel which collectively make up a single grain which has uniform orientation throughout. To these the name "cell" is not only appropriate but already attached.*

To call the walled polygons "phæno-cellular" and the white spots "crypto-cellular" is cumbersome and seems pedantic. For lack of a better plan I propose to call the walled polygons "walled cells" and the white spots specifically "grains."

It is quite true that my white spots may in certain cases have some sort of wall; indeed there may always be some rudiment of an envelope; and hence they might in a sense be called "walled cells." It is also true that my walled cells are derived from the splitting up of what may well be called a "grain" of austenite into the envelope and nucleus or kernel, so that this latter might be called a "grain" of pearlite. I recognize these objections to my plan; but as I cannot express my thoughts without some terminology, and as I have thought of no better, I proceed to use this as a temporary tool, with my gratitude stored up for the inventor of a better.

It remains to be seen whether the need of distinguishing between these entities is going to be urgent enough to warrant burdening our language with special permanent names.

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* Osmond, Les Recherches de G. Cartaud, *Revue de Metallurgie, Memoires*, Vol. 4. 1907, p. 822.

Temporary Definitions.—By a "walled cell" I refer to the composite structure formed during the transformation, with a kernel or nucleus representing a grain of austenite, and an envelope, cell-wall, or shell, representing the ferrite or cementite born of that austenite during the transformation. But we must include here the walled cells which form during solidification, with a cell-wall impoverished in carbon and a nucleus or kernel enriched in carbon.* In the present paper I shall have little occasion to consider these solidification walled cells.

A "network" is an assemblage of such walled cells, and "network size" refers to the average size of the walled cells which make up the network.

By a "grain" I mean specifically one of the individual grains of ferrite or cementite, austenite, martensite, or pearlite, not only in specimens in which "grain" could have no other meaning because of the absence of traceable network-structure, but also in reticular—that is, network-bearing—specimens.

In these latter "grain size" refers to the average size of the ferrite or cementite grains, including both those which occur within the individual kernels and those which form part of the walls of those cells. When, as in Figs. C and D of Rows 5 to 8, Plate III, the cell-walls are continuous or nearly so, the grain size may be very difficult to make out, because we cannot tell readily where the grain boundaries, as distinguished from the walled-cell boundaries, lie in these continuous shells. In other cases, such as that shown in Fig. 19, Plate I, the individual grains of ferrite even in well-marked cell-walls can readily be distinguished.

Strictly speaking, a ferrite "grain" is a mass of uniform orientation throughout. To determine the true size of these grains we cannot rely on the common method of etching with nitric acid, because each of the several boundaries thus outlined may contain several different true grains each with its own orientation. To distinguish these from each other special precautions are needed. In discussing ferrite grain size I have in general confined myself to pre-existing data. We must remember that those data were reached generally without such precautions. Yet for all that we now know the apparent grain size may be as true an index of the quality of the steel as the true grain size.

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* *Annales des Mines*, Vol. 8, 1885, p. 13.

A grain of martensite as in Figs. 19 and 24 to 27, Plate I, or of pearlite as in Figs. C to F of Rows 1 and 2, Plate II, is likely to form the whole kernel of a walled cell; whereas a grain of ferrite is likely to form a small fragment of the wall of such a cell, or of an island of ferrite within the cell, or of such an island in the absence of cellular structure.

As we shall see later, it is the grain growth of the austenite in and more especially above the transformation range that causes the coarsening of the network, or in other words the growth of the walled cells.

The ferrite and cementite interstratified as pearlite I call "pearlitic" to distinguish them from the ferrite or cementite generated by the austenite in cooling through the transformation range, which I call "free." The more accurate name "pro-eutectoid" is too cumbersome for the present purpose. The main subjects of this paper are the network, whether of free ferrite or free cementite, and the ferrite grains, which also are primarily of free ferrite; and hence it is to the free state that I refer in general, unless the contrary is indicated. This seems so clear from the context that I have spared the reader and myself endless repetitions of the word "free."

PART II.—PRINCIPLES UNDERLYING THE STRUCTURAL CHANGES.

14. *Introductory.*—After setting forth in Sections 15 to 48 certain theories and hypotheses as to the causes of the structural changes in carbon steel, in the form of twenty-four principles, I will enunciate in Part III (page 324) twenty-three propositions which seem to govern these changes. I have taken the unpopular step of including among both principles and propositions some which are far from being proved, indeed some which are only conjectures, believing that the formulation even of very imperfect knowledge paves the way for its extension, and that such formulation is harmful only when it conceals the weakness of its foundations, and because it exposes its author to the censure of the superficial and captious, which we may well endure. The

antecedent probability of some of these unproved things warrants using them as working hypotheses.

The matter most difficult to explain is the fact that the distribution of the ferrite and cementite, which form during slow cooling through the transformation range, is so profoundly affected by ante-natal conditions, that is to say, by the temperature reached and the length of exposure above that range, namely, in Region 4, at a time when that ferrite does not exist. I have set forth certain principles which seem to me to explain it reasonably, and under the several laws I have tried to test these principles partly by my own results but chiefly by those of others. These ante-natal conditions affect profoundly the distribution of the ferrite and cementite as cell-walls and as independent islands; the size of this network structure, that is, of these walled cells and of these islands; the quantity of free ferrite which coagulates into visible masses; and the massing of that ferrite into parallel bands.

15. *Principle 1.—Grain growth represents coalescence*, that is to say merging. Whereas before their coalescence each of two adjacent grains had its own crystalline orientation, so that the boundary between them was little more than a line parting two regions of different orientation, different cleavage, etc., after the coalescing the two areas have the same orientation and the boundary between them has ceased to exist. We naturally infer that this identity of orientation is brought about by the conquest of the weaker by the stronger, or by the partitioning of the weaker among its stronger neighbors,* so that the grain which has the more perfectly organized crystalline structure, or which for some other cause has the upper hand, imposes its structure on the other or on the neighboring parts of the other, dragoons the weaker into abandoning its own initial crystalline orientation and accepting instead the orientation of its victor. Or the new grain may have a new orientation of its own, independent of those of the elder grains or grain-fragments by the merging of which it has come into being.

Ewing and Rosenhain's theory,† most seductive in spite of its serious difficulties, explains this merging electrolytically, holding that between grains which are in apparent contact there is still an undetectable partition of solid but somewhat mobile eutectic, which serves as an electrolyte. Through this, because of minute differences of potential, the metal from the conquered grain passes to the conquering grain, so that the boundary line shifts progressively across the conquered grain to its further side, or till it meets the similarly inward traveling boundary of that further side. The transfer may not absolutely need electrolytic help, but may conceivably be due to difference between adjacent grains in their solution pressure toward this undetectable eutectic film, a difference easily explicable by a mere difference in orientation of those grains. The ultimate crystalline particles in grain A may present their apexes (corners) to the eutectic parting, those of grain B may present their flat faces, and this difference may lead to a difference in solution pressure sufficient to account for the extremely slow movement across this ultramicroscopic parting.

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* Guertler, *Metallographie*, 1910, p. 161.

† "The Metallographist," Vol. 5, 1902, pp. 103–109, from *Philosophical Transactions, Royal Society*, Vol. 195, p. 279.

The apparent reason for the existence of this theory is its authors' striking observation that the grain growth of certain metals does not occur in castings, but only in rolled and otherwise overstrained masses. This they explain by supposing that the overstrain breaks the eutectic parting in places, bringing grain against grain, thus setting up an electrolytic circuit, the grain-to-grain contact representing the usual metallic conductor outside the electrolyte, and the film of eutectic representing the usual electrolyte. This theory is supported by their observation that in lead a weld is a barrier which growing grains will not bridge unless it contains eutectic. (See note, page 379.)

Difficulties at once suggest themselves, for instance (1) that the particles of the eutectic itself, for example, the pearlitic ferrite, differ from common electrolytes in being excellent conductors, so that the electrolyte is self-bridging, and that even if the envelopes were not broken by overstrain, local electrolysis ought to go on all along the parting through microscopic circuits. (2) It is rather hard to admit that, even in metals of a high degree of purity and when their grains are initially fine, there is invariably enough eutectic to form continuous envelopes about the grains, a continuity which this theory requires in order to explain the observed absence of growth in unstrained masses. (3) The grain growth on overheating is marked, both in steel castings and in electrolytic cathode iron, such as is shown in Table IV*a* (page 350), in both cases in the apparent absence of overstrain. (4) So, too, with the free ferrite born in cooling through the transformation range, and the austenite born in rising through that range. In both cases the manner of birth seems to preclude the idea of overstrain; they are congenitally strainless; but in both cases grain growth at appropriate temperatures is very marked.

It may be possible to meet these difficulties by taking the somewhat broader point of view which I suggest, that grain growth will occur whenever the conditions permit electrolysis; that though the rupture of very thin envelopes may be one of the conditions favoring electrolysis, it need not be a necessary condition; that, after all, the two main elements are difference of potential and resistance; and that either an increase of the former or a decrease of the latter may suffice.

Though in simple masses like lead the connection between anode and cathode needed to permit electrolysis may be given only by envelope rupture, in composite masses like steel it may be supplied by the usual dendritic structure of the mass as a whole, neighboring twigs being in fact connected, horse-shoe-wise, through the larger boughs. The stronger difference in potential needed for enabling electrolysis to cross the wide spaces present in steel may be supplied by the greater differences in composition. In the cold metal there is the great difference in potential between ferrite and cementite, between alpha iron and the allotropic iron which may survive even a relatively slow cooling, and between neighboring ferrite branches differing in their manganese content. In the hot metal above the transformation range there are the uneffaced differences in carbon content which, aided perhaps by the presence of slag, manganese sulphide, and other inert barriers, have survived diffusion.

Indeed, we may well experiment to learn in what proportion the grain growth induced by overstrain may be due to its rupturing the eutectic envelopes, and in what proportion to its exaggerating the difference of potential by setting up the amorphous hard state of the metal, even in simple masses like lead.

It is in agreement with this conception that the grain-growth of austenite is the slower the less the carbon content, and the less, consequently, is the initial or congenital heterogeneousness of that austenite, and the less the residual heterogeneousness surviving a given opportunity for diffusion. In the nearly pure and congenitally strainless electrolytic cathode iron in which I found decided grain growth at 1300°, congenital heterogeneousness may be supplied in the form of residual concentration of hydrogen or other impurity at the cell boundaries.

In a word, in these congenitally strainless masses uneffaced congenital heterogeneousness may take the place of overstrain in supplying conditions favoring electrolysis and thus inducing grain growth.

We need not fall into the familiar error of excluding all prior causes when we admit a new one. Yet in view of the striking evidence, especially the familiar hastening of grain growth by overstrain, we may provisionally accept electrolytic action as a strong accelerator of grain growth, and as in some cases necessary to it.

Two facts suggest that coalescence is rather by the merging of whole grains than by the partitioning of individual grains among their neighbors. First, such partitioning would be likely to yield nearly equiaxed grains, whereas simple merging would yield grains of most irregular shape, and in particular with very unequal axes. In fact the ferrite grains reached by growth at high temperatures are often very much longer than wide, and of shapes so distorted and fantastic as strongly to suggest erratic merging rather than partitioning. Joisten's Figs. 491, 492, and 493 illustrate this strikingly.[*]

Second, when a polished steel strip of low-carbon (0.125) steel was held by Osmond and Cartaud for a long time in Region 4 in a stream of hydrogen, and again cooled slowly, four distinct sets of network could be traced, of which three were effaced by etching, while the fourth was deepened, showing that it represents the final or existing structure.[†] The surface of this strip after this heating is shown in Fig. 16, Plate I. The existence of the four networks shows that, as each network forms, it causes on the polished surface a certain movement, the traces of which persist. When we consider the distinct transformations which must occur in heating up and cooling down through the transformation range, we are surprised that the number of networks is so small. In this case the polished steel was held in hydrogen for two hours between 1150° and 1200°, approximately.

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[*] *Metallurgie*, Vol. 7, 1910, facing p. 458.

[†] Osmond and Cartaud, *Annales des Mines*, Vol. 18, 1900, p. 142 and Fig. 8, Plate VI. The apparent failure of Ewing and Rosenhain (*op. cit.* p. 86) to set up any pattern on the surface of polished specimens which they heated in a like way. I refer to the fact that their specimen was apparently nearly pure carbonless iron, and hence consisted at all temperatures almost solely of a single constituent; whereas the specimen of Osmond and Cartaud had 0.125 per cent. carbon, and would therefore generate about 13 per cent. of pearlite, or in short would, in and below the transformation range, be a granitic mass with two constituents, differing enough in their coefficient of expansion to set up inequalities on the polished surface.

If we may judge by the very rapid grain growth of the austenite at this temperature indicated by my Figs. E of Rows 2, 4, and 6, Plates II and III, very many successive increments of the grain size must have occurred during these two hours. If the growth is by partitioning, this implies a large number of successive partitionings, each of which should have left its trace in the form of a distinct network, so that a great number of networks should remain, instead of the small number, four, actually found. But if the grain growth was by simple absorption or merging without partitioning, then each step in the growth would occur without setting up any new network, just as the partitioning of Poland set up new boundaries on the map of Europe, but neither the Germanic nor the Italian unification need have created any new ones. From the small number of networks actually present in M. Osmond's specimen after his two-hour heating, I incline to the theory that the successive steps of grain growth occurred for the most part without setting up new boundaries, that is, by simple merging as distinguished from partitioning.

This evidence is at best only suggestive. The growth of a grain according to the Ewing and Rosenhain theory, the progressive shifting of its border across the neighboring grain, would have no distinct stages which could leave their traces; and on this theory even the four networks of Osmond and Cartaud require explanation.

16. *Principle 2. Contact and Migratory Grain Growth.*—When the mass, instead of being a composite or granitic one like medium-carbon steel, Figs. 19 to 28, Plate I, consists of a single chemically homogeneous substance, like the grains of the ice from pure distilled water, or those of nearly carbonless steel, Figs. F and G of Row 12, Plate IV, or of austenite when in Region 4,

or of unalloyed gold, the coalescence is that of grains initially in complete contact with each other. When such a grain loses its existence and is absorbed by its neighbors or partitioned among them, no migration of its particles need occur. Instead, each particle by simple rotation about some axis, that is, by polarization, adopts the orientation of the grain with which it merges. This may be called polarizing or contact coalescence. The theory of Ewing and Rosenhain complicates this conception slightly, by implying, between the abandonment of the initial orientation and the adoption of the final, the intermediate step of crossing the supposed parting which separates even the grains which are apparently in direct contact.

In steels containing any considerable quantity of carbon, the various grains of ferrite are more or less separated from each other by the intervening pearlite; and when the carbon content reaches about 0.75 per cent. this separation may come to look like complete isolation of ferrite grain from grain, somewhat as shown in Fig. 19, Plate I.* That grains which look thus initially isolated do grow, and hence do merge in spite of their apparent isolation, is perfectly certain, because after bringing such steel to the state of an emulsion so fine that its different constituents cannot be distinguished from each other even on the strongest magnification, its particles of ferrite can be caused to coalesce into larger and larger visible islets by long heating below the transformation range. But the merging of different particles initially thus separated clearly implies that they must migrate, that is, be translated. This form of coalescence may be called *migratory* to distinguish it from simple *contact* coalescence. Clearly the presence of even the relatively small quantity of pearlite shown in Figs. A to E of Rows 3 and 4, Plate II, implies that the coalescence of neighboring grains of ferrite occurs through movement over a relatively long distance, and hence it must be in large part migratory.

Though on the theory of Ewing and Rosenhain this difference might, strictly speaking, be called one of degree, yet the difference of degree is usually so great as to be in effect one of kind. The resistance to electrolysis through the wide inter-insular spaces shown in Fig. 19 exceeds so greatly that through the indetectably narrow eutectic partings supposed to separate ferrite grains apparently in contact, that the dominant cause of merging may be surface tension in the former case and electrolysis in the latter.

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* The steel which this micrograph actually represents had 0.40 per cent. of carbon and had been quenched from within the transformation range, and hence the ground mass is not pearlite but probably troostite. But it serves to show this apparent isolation of grains of ferrite by an enclosing ground mass. Such isolation by pearlite itself is shown by Heyn in his Fig. 176, Mikroskopische Untersuchung, *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, 1904, Tafel XIX.

In order that our terms may include this theory, we must understand that the word contact is not necessarily restricted to its strictest sense, but includes the sense in which we say that two men come into violent contact; their bodies are in fact parted by their clothing, yet for most purposes the contact is quite as effective as if their clothing were lacking. Indeed, in most cases of apparent contact there may be an undetectably thin film of air, oxide, etc. *Migratory* coalescence applies to the coalescence of bodies initially parted by a considerable distance, migration, whether of birds or tribes, properly implying a long distance travel, in contrast to the *contact* coalescence of bodies initially in contact, either in its strictest sense or in the looser sense in which the jostling passer-by comes into contact with me.

It is extremely probable that under like conditions migratory is much less rapid than contact coalescence.

17. *Principle 3. Migratory coalescence does not imply that completely isolated particles push aside the intervening matter to reach each other.*—Our first impression is that this migratory coalescence of ferrite particles which, as seen under the microscope, are completely isolated from each other, implies that they must travel through and across the intervening pearlite or cementite, forcing it aside as a swimmer forces the water aside, moved not by surface tension, but by some attraction like gravitation or magnetism, by means of which a body exerts an influence on others which it does not actually touch.

But while we can readily understand that, if two ferrite particles touch even at a single point, surface tension should draw them together into a single islet, and that in thus spheroidizing they should incidentally displace some of the adjoining cementite, it is less easy to understand how two completely isolated ferrite particles can push aside the cementite which parts them. For they have no mechanism comparable with the swimmer's legs which should propel them through so relatively hard and viscous a substance as steel at 650° C., at which temperature this coalescence is relatively rapid. On reflection we incline to dismiss this conception as one which offers a wholly needless difficulty, and to assume as a working hypothesis, that there is no such swimming of initially perfectly isolated ferrite particles through the resistant cementite to unite with each other; but that, on the contrary, all these coalescing ferrite particles are either initially connected directly or indirectly, or else are brought into contact through natural convections due to simple mechanical means.

18. Let a simile explain this. If we were to set a small pine or hemlock tree in a vat, fill the vat with thin opaque molten wax, allow the wax to solidify, and then cut sections at random through it, each section would cut apparently isolated spots of wood. Yet in fact each spot is connected with every other spot by means of the unseen parts of the tree. So with the apparently isolated particles of ferrite. But in fact the conditions in any hypo-eutectoid steel are incomparably more favorable, and the more so the lower the carbon content. For instead of occupying a small fraction of the total volume as our supposed pine tree does, the free ferrite occupies a space which varies from 44 per cent. of the whole in a 0.50-per cent. carbon steel to 89 per cent. in a 0.10-per cent. carbon steel. Hence it is reasonable to suppose that a very large proportion of the apparently separate ferrite islands are in fact connected together dendritically outside of the plane of the microsection.

But the case is still more favorable, because of the fact that the pearlite which separates the ferrite islands is itself nothing but a ferrite sponge, with interstices filled with cementite in the ratio of one of cementite to six of ferrite. Hence, should there be any ferrite islands which are initially out of contact with all others, they are in fact connected with those others by the ferrite which forms the great bulk of the pearlite which alone parts them, the pearlitic ferrite. That the ferrite of the pearlite is disposed in such a way as thus readily to bridge across from one island to another is shown by a remarkable photograph of Heyn's,* Fig.

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* *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, 1904, pp. 355–397, Fig. 56, Tafel VII.

13, Plate I, and also by a famous one of Osmond's * which I do not reproduce.

In this view the gradual coalescence of the ferrite islands may be referred first to the surface tension along the unseen ferrite branches beyond the plane of the microscopic section; and second, to the surface tension between the free ferrite islands and the pearlitic ferrite.

This line of thought explains readily the apparently greater persistence of the cementite cell-walls of hyper-eutectoid steel than of the ferrite cell-walls of hypo-eutectoid steel indicated in Propositions 9 and 10, Paragraphs 62 and 63, pages 355 to 357. The break-down of the network structure of the latter is aided by the solder-like action of the ferrite-sponge of which the pearlite grain-kernel really consists; whereas the break-down of the cementite walls is not so aided, because of the sparseness of the cementite in that pearlite.

Additional convections which aid this coalescence readily suggest themselves. The assumed expulsion of the ferrite from the mother austenite in cooling through the transformation range is such a convection. Where the emigrating particles of ferrite jostle they unite. Again, the drawing together of the ferrite branches under the influence of surface tension, their spheroidizing, is a further cause of convection, like that which occurs when drops of oil suspended in water coalesce. The internal movements which this drawing together involves may well lead to establishing contact between any truly isolated ferrite mass and at least one of its neighbors. Still again, the progressive coalescence of the constituents of the pearlite from their initial state of emulsion to that of lamellar pearlite is another source of convection.

It is in accord with these ideas that the lower the carbon content the more easy and rapid should the grain-growth be, because the carbon is present either as pearlite, or in very-low-carbon steel often as divorced cementite. Each of these acts qualitatively as a foreign body to separate the adjoining grains of ferrite, and thus to prevent the easiest form of coalescence,

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* *Contribution à l'Etude des Alliages*, Paris, 1901, Fig. 2, facing p. 288; *Microscopic Analysis of Metals*, London, 1904, Fig. 42, p. 84. This specimen is of hyper-eutectoid steel, but it serves to show how well the structure of pearlite is adapted to this bridging.

the contact form, that of grains in contact, which consists in one grain's adopting the orientation of its neighbor without the migration of any molecule of either grain. The less the carbon content, the less this mechanical obstruction.

In many cases migratory coalescence may be aided by solution and re-concentration. For instance, when large and small particles of salt lie together at the bottom of a brine, the solvent power of the brine for the smaller particles is greater than for the larger ones.* Hence, though the brine may be saturated as regards the larger particles it may not be as regards the smaller ones, which therefore progressively re-dissolve in the brine, to be re-precipitated upon their larger neighbors. Thus the grain-size progressively increases and the number of grains progressively decreases, till theoretically all should be reduced to a single grain. This indeed reminds us that meteors habitually consist of a single grain. This principle may contribute here, and so may the electrolytic action of Ewing and Rosenhain, even when the separation is wide.

19. *Coalescence of Constituents of Pearlite.*—A special phase of coalescence is the divorcing or disentwining of the twin constituents of pearlite, like that of other eutectoids and eutectics, into separate masses of free ferrite and free cementite. It is perfectly certain that each of the constituents of the pearlite does coalesce in the complete absence of either free ferrite or free cementite, as for instance in steel which is exactly eutectoid. As the pearlite is first generated its particles are so minute as to be indistinguishable with the strongest magnification, and it is then called sorbite or troostite. With longer and longer exposure to temperatures between 600° and say 730°, at which the metal has considerable mobility, this sorbite passes into the stage of granular pearlite. Given sufficient time there can be little doubt that its constituents would eventually form a single mass of ferrite and another of cementite. Goerens gives cases in which this coalescence has gone far.

The ultra-slow cooling after the cementation process affords an excellent opportunity for this divorce of the constituents of pearlite. Thus Arnold's micrograph of blister steel of eutectoid carbon content, 0.90 per cent., shows that a considerable part of the ferrite and cementite respectively of its pearlite have parted company, each to ball up by itself.*

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* Benedicks, *Metallurgie*, Vol. 3, 1906, p. 410; also Hulett, *Zeitschrift für Physikalische Chemie*, Vol. 37, 1901, pp. 385-406.

In a hypo-eutectoid steel the pearlite is in contact with more or less free ferrite; and the observed fact that the ferrite initially present as part of the pearlite, as it disentangles itself from the cementite with which it is intertwined, coalesces with that free ferrite, is only what we ought to expect. Stead's observation† that this disentwining, and the coalescing of the ferrite thus freed with the ferrite initially free, are the more complete the larger the quantity of this initially free ferrite, is only natural; because, as the quantity of this free ferrite increases, so does the size of the pearlite masses between decrease. The smaller they are, and consequently the smaller the distances to be covered, the faster should the disentwined ferrite coalesce with the initially free ferrite, and put an end to its own separate existence.

It is but natural that the disentwining by surface tension should be faster in the immediate neighborhood of the initially free ferrite islands, because here the surface tension between the disentwining ferrite and those free islands is added to the surface tension between the particles of disentwining ferrite in the pearlite.

So, too, do we explain Stead's observation that in hyper-eutectoid steels the presence of much free cementite favors the disentwining of the pearlite, or at least the coalescence of the cementite thus freed into large masses.

20. *Principle 4. Grain Growth Presumably a Function of Time as Well as Temperature.*—We see no reason in the nature of the case why, at a given temperature, grain growth should not continue indefinitely;‡ why the causes which lead strong small

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* The Micro-chemistry of Cementation, *Journal, Iron and Steel Institute*, Vol. 2, 1898, p. 185, Plate XIV.

† Stead, Segregatory and Migratory Habit, *Journal, Society of Chemical Industry*, Vol. 22, 1903, p. 343. Also Fifth Report, Alloys Research Committee, February, 1899, p. 73, excerpt *Proceedings, Institute Mechanical Engineers*.

‡ For simplicity of statement grain growth has often been spoken of as if it depended solely on the temperature reached. But the slightest consideration of the conditions under which, according to our present theories, this growth occurs, leads us to infer that for given temperature, it should increase with the length of exposure. Thus Osmond said, as early as 1893: "It may be admitted, provisionally, that a prolongation of this period is equivalent to a certain elevation of temperature during a shorter period." *Transactions, American Institute*

grains to absorb or partition a weaker small neighbor should not apply to strong large grains also, so that in turn the stronger of the resultant large grains should impose their orientation on the adjacent territory of a like large but weaker neighbor. As this process goes on it might on one hand be retarded (1) by the fact that the gradual elimination of the weaker of the original grains through their absorption by the stronger ones, would leave those stronger ones more evenly matched; (2) conceivably, though not probably, by the greater distances from the centers of individual grains to their outside at which they are doing the work of subduing their neighbors; and perhaps (3) also in the case of low-carbon steels, by their progressively exhausting the ferrite contained in the pearlite which in part separates the ferrite grains; for so long as this ferrite of the pearlite remains, it is a kind of solder, readily divorcing the cementite with which it is initially intertwined to join the great mass of free ferrite which adjoins it. The cementite thus left would act as a parting to obstruct the merging of neighboring ferrite grains. On the other hand, once this cementite was completely divorced, its subsequent balling up under surface tension would leave a correspondingly larger proportion of the surface of the ferrite grains in actual contact one with another, and hence free to merge without migration.

If the process were thus progressively retarded at given temperature this would tend to obscure the influence of time, and thereby to leave the influence of temperature correspondingly the more prominent, and perhaps even to suggest that it is only the temperature reached that determined the grain size. Yet even if such an impression arose on first inspection, on reflection we should expect time to have some effect, and that a sufficiently long exposure to a moderate temperature within Region 4 would lead to the same effect as a short exposure to a higher temperature.

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of Mining Engineers, Vol. 22, p. 256. Yet Beilby seems to hold that for each temperature there is a definite maximum grain size which will not be exceeded on any prolongation of exposure to that temperature. (The Hard and Soft State in Metals, Second May Lecture, Institute of Metals, May 1911, p. 20.) The theory of Ewing and Rosenhain explains such a maximum in part. Only certain ones of the envelopes parting neighboring grains might be ruptured. Grain growth would cease as soon as the grains parted by these ruptured envelopes had merged, the remaining grains surrounded by still unruptured envelopes refusing to merge. But here the explanation stops, for the arrest should be permanent, and further rise of temperature should cause no further grain growth.

To test this point the experiments described in this paper were made. (See Proposition 5, Paragraph 56, page 343.)

21. *Principle 5. Grain Growth of Alpha Ferrite in Very-Low-Carbon Steel in the Regions 7 and 8A, and of Austenite of All Steels above the Transformation Range in Region 4.*—The austenite of all steels, when above the transformation range in Region 4, instead of being a conglomerate of distinct substances like annealed steel of medium carbon content, is a single substance, and in consequence each of its grains is in contact at all points with a chemically like grain. For simplicity I here assume that diffusion has proceeded so far as to cause the re-absorption of any initially free ferrite or cementite. Because of this contiguity of austenite to austenite, grain growth by contact coalescence is rapid, and becomes the more rapid with rise of temperature, because this increases the mobility of the metal.

Very-low-carbon steel in like manner consists, when in Regions 8A and 7, chiefly of alpha ferrite, with very extended contact between grain and grain of that ferrite. Hence its grain growth is rapid in the upper part of this territory, where the high temperature increases the mobility. Here the extent of actual contact between ferrite and ferrite is increased by the fact that, in these very-low-carbon steels, the cementite, instead of remaining intertwined with part of the ferrite in the form of pearlite, is very apt to disentangle itself from that pearlite, and ball up into small compact masses.

One important peculiarity of very-low-carbon steel should be noted here. Though when below and in the lower part of the transformation range (in Region 7 and the upper part of Region 8A), where it consists chiefly of grains of alpha ferrite, its grain size increases rapidly, apparently chiefly by contact coalescence, yet as shown under Proposition 5, Paragraph 58, page 347, the grains of beta ferrite of which it consists when in the upper part of that range (in Region 6), and those of austenite of which it consists when above that range (in Region 4), grow extremely slowly, so much more slowly than those of higher-carbon austenite as to give the impression that they do not grow at all. In short, very-low-carbon steel coarsens rapidly below A2 and very slowly above A3, whereas it is above A3 that the rapid grain growth of higher-carbon steel occurs.

As will be shown later, the cells which we find in cold hypereutectoid steel, consisting of kernels of pearlite encased in shells of cementite, are certainly the larger, the higher and the longer the steel has been held above the transformation range, in Region 4, that is, before the birth of these cementite shells. Inferring as we do that each such cell represents a single grain of austenite formed in Region 4, and that its shell represents the cementite which that austenite has expelled in cooling through the transformation range, we see no escape from the belief that these austenite grains while in Region 4 are in a constant state of such growth as has just been outlined, and hence are in a state of flux, existing boundaries being wiped out as a Bonaparte might wish. This conception of the state of crystalline flux in Region 4 may not have been reckoned with sufficiently by previous students of the crystallography of iron, even by Osmond himself. It is quite possible that some of the four successive networks which he finds on the surface of steel which after polishing he held for two hours at 1150° to 1200° in a current of hydrogen, may be the records of these changes of grain boundary by coalescence in Region 4. The discrepancies between the internal orientation and the cell-walls shown in his wonderful micrograph* which I reproduce in Fig. 16, Plate I, may actually represent a stage in this boundary shifting, after the new has become visible and yet before the old has ceased to be.

Of course there can be no ferrite or cementite grain growth above the transformation range because neither ferrite nor cementite exists there; nor for like reason can there be growth of austenite grains below that range.

22. *Principle 6. Grain Growth of Beta Ferrite, Region 6.*—This normally exists only between A2 and A3, in Region 6, and very-low-carbon steels here consist of this beta ferrite almost solely. As to its grain growth we know so little that the hypothesis which I here offer must be regarded as nothing more than the best speculation which I can frame to fit the recorded facts. It may serve as a stepping stone towards the truth.

In Region 6 the grains of beta ferrite tend to coarsen, but apparently far more slowly than those of alpha iron when in the upper part of Region 7, or those of austenite when in the upper part of Region 4. In general, when the temperature rises into Region 6, the beta ferrite seems to inherit the grain size of the alpha ferrite which existed in Region 7.[*] But if this size is large, the initial grains probably break up into finer ones of beta iron, which in turn coarsen slowly. Thus the assumed natural tendency of beta iron, as of other metals in general, to coarsen by coalescence, may be masked by the tendency of an initial coarse grain inherited from the previous alpha state to break up, the preferred habit of the beta iron apparently differing enough from that of alpha iron to induce this slow break-up.

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* Osmond and Cartaud, *Annales des Mines*, Vol. 18, 1900, Fig. 8, Plate VI.

23. *Principle 7. Grain Growth of Steel of Intermediate Carbon Content (say, 0.10 to 0.80 per cent. of Carbon) in and below the Transformation Range, in Regions 8A, 7, and 6.*—Such steels while in Region 8A are a conglomerate of ferrite with pearlite. As in a series of steels the carbon content increases from specimen to specimen, this pearlite increases in quantity, and thus the more completely isolates the ferrite grains, so that the grain growth of the ferrite, whether by migratory or contact coalescence, becomes increasingly difficult. When such steels rise past Ac1 into Region 7, their pearlite changes into austenite. As the temperature rises through the transformation range, Regions 7 and 6, the ferrite present is progressively absorbed by that austenite, so that the quantity of residual ferrite progressively decreases and that of the austenite progressively increases. This increase in the quantity of austenite progressively obstructs the coalescence of the residual ferrite more and more. Hence, though as the temperature of a given steel rises through Region 8A towards the transformation range, its ferrite-grain growth tends to accelerate because of the increasing mobility, this acceleration is opposed as the temperature rises through that range, Region 7, by the increasing quantity of austenite which forms. This opposition should increase abruptly on entering the beta iron Region 6, because of the apparently slower coalescence of the beta ferrite which here replaces the alpha ferrite of Region 7.

In carbonless steel this retardation in Region 7 caused by the presence of austenite should be nil, that is, the acceleration of ferrite grain growth should continue unchecked through Region

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[†] See quotation from Osmond and Cartaud under Proposition 2, Paragraph 50, p. 327.

7; in steel but slightly hypo-eutectoid the retardation should be great; and in general in a series of steels the retardation should increase as the carbon-content approaches that of the eutectoid.

Turning now to the grain growth of the austenite, the natural retardation which this undergoes as the temperature descends towards Ar3, through Region 4, because of the decreasing mobility, is increased on passing down through the transformation range, Regions 6 and 7, by the progressively increasing quantity of ferrite which the austenite now generates. This ferrite of course tends to insulate the austenite grains. When this generation of ferrite goes so far as to set up a well-marked network structure as in Figs. C, D and E of Rows 5 and 6, Plate III, so that the ferrite cell-walls seem to insulate the enclosed austenite kernels from each other completely, this insulation might be expected to arrest the austenite grain-growth completely. But when these cell-walls are thin, as they must be at temperatures but little below A3, and also in steel but slightly hypo-eutectoid, and when they become discontinuous by the spheroidizing of the ferrite, enough contact between neighboring austenite grains may well arise to cause coalescence. This, beginning in any given case at a contact, may continue as migratory coalescence, the surface tension of the grains which thus merge displacing the ferrite which stands in their way.

In a series of steels of varying carbon content the quantity of this insulating ferrite generated increases as the carbon content recedes from that of the eutectoid; and with this increase the opposition to the further grain growth of the austenite in passing down through the transformation range should increase.

To sum this up, in steels of intermediate carbon content, the grain-growth of the austenite should be retarded progressively as the temperature sinks from A3 towards A1, and should be the more retarded the more the carbon content differs from that of the eutectoid, 0.90 per cent.

The acceleration of the grain growth of the ferrite as the temperature rises towards A1, through Region 8A, because of the increasing mobility, should be retarded as the temperature rises from A1 to A2 through Region 7, by the increasing quantity of austenite which forms, and should be further checked on passing A2 and entering Region 6 by the supposed sluggishness of the beta ferrite. This retardation should be the greater the greater the carbon content. In carbonless steel this retardation in Region 7 should be nil, that is, the acceleration of grain growth should continue unchecked; in steel but slightly hypo-eutectoid the retardation should be very great.

24. *Evidence Supporting this Principle.*—Among the "Propositions" given later in the paper there is none to represent this present principle, because of the scantiness of the evidence on which it is based. It must be regarded as only a hypothesis based on our general knowledge of the conditions. But two pieces of evidence supporting it may be mentioned here.

(1) The specimen shown in Fig. 19, Plate I, which had been cooled slowly from above the transformation range to a point, 765°, within that range, and then quenched in water, was next reheated to a point in the transformation range, 740°, for six hours, and again quenched in order to preserve the structure formed at 740°. It then had the structure shown in Fig. 23, Plate I. At 740° it of course consisted of a conglomerate of alpha ferrite and austenite. As shown under Proposition I, Paragraph 49, page 324, the grain growth of such alpha ferrite when alone or substantially alone, as in very-low-carbon steel, is very rapid at this temperature. But in the present case the individual grains of ferrite forming the discontinuous cell-walls have not increased in size, which agrees with our inference that their separation from each other by the intervening austenite ought to retard their growth.*

(2) Benedicks† found that, on heating a nickel-iron alloy, an 11.7-per cent. nickel steel with only 0.09 per cent. of carbon, to 490°, within the transformation range of that alloy, a network

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* The ground mass between these large grains has a wholly different look in Fig. 23 from that in Fig. 19. The exact nature of this difference I have not determined. In both these figures the area of these large ferrite grains collectively is far too small to represent the whole of the ferrite present, so that the ground mass between must contain in additon to the austenite, much finely divided free ferrite. The long holding at 740° in the case of Fig. 23 seems to have given this ferrite time to coalesce into extremely fine islets, which are readily made out under stronger magnification. This difference between the two micrographs may have been increased by the somewhat slower cooling of Fig. 23. But the purpose of introducing this micrograph is to show that the interposition of this ground mass of mixed austenite and emulsified ferrite between the large cell-wall islets of ferrite has prevented them from growing at the rapid rate at which the grains of ferrite grow at this temperature in very-low-carbon steel, in which ferrite grain is in immediate contact with ferrite grain.

† Benedicks, Synthese du Fer Meteorique, *Nova Acta Regiæ Societatis Scientiarum Upsaliensis,* October, 1910, Series IV, Vol. 2, pp. 15 and 16, Figs. 10 and 11 of reprint.

structure arose. Though the kernel of each cell looked as if it was nearly completely isolated from its neighbors by an extremely thin cell-wall, yet on longer exposure to this same temperature within the transformation range these cells increased in size. This shows that, even in such a conglomerate mass as must exist within the transformation range, one of the two intermixed contituents may assemble into distinct grains, each with its own orientation, in spite of the continued presence of the other constituent within those grains; and further, that the presence of thin cell-walls, even if they look nearly continuous, does not necessarily prevent completely the coalescence of neighboring kernels.

25. *Principle 8. Grain Refining of Low-Carbon Steel.*—When beta ferrite changes into austenite on rising past the top of the transformation range, the old grains of the ferrite, instead of being inherited by the austenite, are shattered, so that the nascent austenite is very fine-grained, indeed so fine-grained as to suggest that it is amorphous. This constitutes the grain refining of low-carbon steel. Yet in such steel, when the grains of austenite have grown to a great size by long high heating above that range in Region 4, this large size appears to be inherited successively by the beta and alpha ferrite into which it changes in passing down through the transformation range, probably for the reasons explained under Principle 10. In short, though the grain of ferrite shatters when it changes into austenite, in heating past Ac3, that of austenite seems to remain unlessened when it in turn changes into ferrite in cooling past Ar3.

This shattering of the existing grains when beta iron changes into gamma iron at Ac3 contrasts strongly with the apparent inheriting of the old grain size when alpha iron changes into beta iron at Ac2. This contrast agrees with the theory that, whereas alpha and beta iron have the same cleavage system, say cubic, gamma iron (austenite) has octahedral cleavage. This is true whether we hold with Osmond[*] that the exterior crystalline form of gamma iron is octahedral, or whether we hold with Le Chatelier[†] that it is rhombohedral, in view of its triangular and hexagonal markings and of its twinnings, which features are in his opinion characteristic of the rhombohedral system.

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[*] The Crystallography of Iron, Osmond and Cartaud, *Journal, Iron and Steel Institute*, Vol. 3, 1906, pp. 444–488.

[†] Private communication, Aug. 25, 1911. In view of the very familiar twinning of the true octahedra of spinell, tetrahedrite, magnetite and other isometric minerals, the argument drawn from twinning needs some elaboration.

To these may be added a third reason, that with the metals as with other crystalline bodies the high-temperature allotropic forms are habitually of a lower order of symmetry than the low-temperature ones, the true or cubic octahedron being symmetrical along all three axes, whereas the rhombohedron is longer or shorter along one axis than along the others. Osmond's evidence, especially his demonstration of the nature of the symmetry of manganiferous gamma iron as shown by the figures developed on it by pressure, seems to me to justify our putting aside Le Chatelier's objections.

On either theory it is but natural that the change from the alpha to the beta form without serious disturbance of the crystalline organization should affect the existing grain size but slowly, whereas the change from the beta to the gamma form at Ac3 with complete change in the cleavage should shatter the existing grains.

26. *Principle 9. Genesis of the Cell Structure.*—The individual grains of austenite which, in cooling through the transformation range, generate ferrite or cementite as the case may be, reject it as they generate it, throwing it primarily to their exteriors and so massing it at the grain boundaries, and secondarily into their cleavages, as explained in Paragraph 12, page 272. But this expulsion is incomplete, so that a large amount of the ferrite thus generated remains scattered through the austenite kernels themselves. This massing of the ferrite at the grain boundaries causes the network structure, each cell representing a single austenite grain. The cell-walls are the ferrite so massed at the grain boundaries, and the kernels the residual austenite, which in cooling below the transformation range becomes pearlite.

As the temperature sinks farther and farther through the transformation range, the kernels of mother austenite continue generating and expelling ferrite, progressively building up and thickening their cell-walls at their own expense, as is shown by the series of micrographs,* Figs. 24 to 28, Plate I.

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* These figures show the structure at various points in a single bar of Steel I (carbon 0.40 per cent.), at a series of points successively lower in the transformation range. The bar

Stages in the network formation roughly corresponding to these may be seen in Osmond's micrographs 77 and 78.*

As the size of the austenite grains, according to Principle 5, Paragraph 21, page 289, increases with the temperature reached above the transformation range, that is, in Region 4, and with the length of exposure to it, it follows that the true network size representing those grains also must increase with the temperature reached in Region 4 and with the length of exposure to it and neighboring temperatures, though it may be masked in hypo-eutectoid steel, as we shall see.

From this theory it follows that this network structure should form in any cooling which includes any part of the transformation range, because everywhere throughout that range the austenite grains must have some size. But in order that the network structure shall be prominent or even recognizable, those austenite grains should be of considerable size, so that the quantity of ferrite or cementite which each expels in cooling shall be enough to form a recognizable cell-wall. Such considerable size we may expect them to reach whenever the temperature is held for any length of time materially above the transformation range, or even in the upper part of that range, for here the quantity of ferrite or cementite present is small, and hence its power of impeding mechanically the austenite grain growth is weakened. Indeed, mere residence in the transformation range, even near its bottom, might set up the cell structure, for here the austenite might well assemble into grains, around which the free ferrite or cementite present would form a network.

27. Steels of 0.90 per cent. carbon, that is, eutectoid steels,

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was first heated nearly evenly within an iron tube in a gas forge to about 1325°. The heating up occupied about 20 minutes. It was then held at this high temperature for about 35 minutes, so as to coarsen the austenite structure. The tube and bar were then drawn forward slowly, through a small orifice in the forge door, till about half their length protruded into the air, the temperature within the forge falling slowly the while. This slow forth-drawing, which occupied about 15 minutes, caused the cooling of the outer end to outrun that of the middle and rear of the bar, and so established a flat thermal gradient along the length of the bar, passing through the transformation range and part of Regions 4 and 8A. Before quenching, the bar was left in this position for about 15 minutes, in order that the several points in it, which at the moment of quenching were in the transformation range, should have reached their several positions in that range by means of a cooling slow enough to build up the cellular structure, yet not so slow as to lead to its degeneration and break-up. The bar was then drawn from the tube and quenched in water, in order to fix approximately the structure thus developed.

* *Microscopic Analysis of Metals*, Osmond and Stead, London, 1904, pp. 136 and 137.

of course generate neither ferrite nor cementite, because such generation is not needed to give them the eutectoid ratio. Hence they may lack the distinct cell-walls. It may prevent confusion to speak of these wall-less cells as grains. When above the transformation range they consist, then, of grains of austenite; when below it they consist of grains of pearlite; and the occurrence or structure of that pearlite may show traces of the grain-structure of the austenite which it succeeds.

What appears to be such a wall-less or very thin-walled grain of pearlite in a eutectoid steel is shown in Fig. 17, Plate I. This shows a steel of 0.92 per cent. carbon, heated to 1300° for one hour and cooled slowly in the furnace.

28. *Principle 10. Effects of High Heating in Region 4 in Addition to Cell Coarsening.*—Now that we have seen how it comes about that high and long heating below A2 in Regions 8A and 7 coarsens the ferrite, and above A3 in Region 4 coarsens the austenite and through it the cell structure, it remains to consider how it is that long high heating above A3 has certain other effects. These are that it (1) opposes the transformation in cooling; (2) increases the stability of the red-hardness of high-speed steel; (3) increases the areas which are free from coagulated ferrite (see Paragraph 35, page 307) in air-cooled steel; (4) causes the austenite to mass into its own cleavages the ferrite which it generates in cooling through the transformation range; (5) makes the cell structure more persistent; (6) increases the size of the ferrite grains in the slowly cooled steel; and (7) retards the coagulation of sorbite into pearlite. As regards (6), note that we are now going to consider the coarsening of the ferrite grains, not by their own direct growth in and below the transformation range, in Regions 7 and 8A, but by pre-natal influences, namely, long high heating above that range in Region 4, when the ferrite does not exist.

That high heating lowers the recalescence, Ar1, was shown by Osmond in 1888[*], and has been shown again lately by Brayshaw.[†] Evidence tending to show that it has the third, fourth, and fifth effects will be shown under Propositions 5, 7, and 12, Paragraphs 53, 60, and 65, pages 337, 352 and 360.

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[*] *Transformations du Fer et du Carbone*, Paris, 1888, p. 35.

[†] *Proceedings, Institute of Mechanical Engineers*, London, April 15, 1910; through *Engineering*, Vol. 89, 1910, p. 526.

Not one of these seven results of heating to a given high temperature above the transformation range in Region 4 occurs at that temperature itself; they occur at far lower temperatures on cooling. Indeed, neither the ferrite nor the pearlite, the occurrence of which is so profoundly affected by such high heating, then exists, and they do not come into existence till the temperature again descends into the transformation range. At this high heating in Region 4 nothing but austenite exists. Hence it is evident that such heating must cause in the austenite itself some immediate change which will in turn lead to these seven ulterior effects. There are two such immediate changes which might seem competent to explain certain of these ulterior effects. These changes do not exclude each other as causes of the ulterior effects. They are (*a*) strengthening the crystalline organization, and (*b*) equalizing the composition, or in other words lessening the initial heterogeneousness. Let us take up (*a*) and (*b*) in order.

29. (*a*) *Strengthening of the Crystalline Organization.*—That this organization has certain remarkable powers is plain from the existence of the cell-walls, for instance in hypo-eutectoid steel. We may reasonably assume that, after a sufficiently long heating in Region 4, above the transformation range, diffusion becomes complete. On slow cooling through the transformation range the austenite begins generating ferrite. Each grain of austenite expels to its outside, at least in part, the ferrite which it bears, thus creating the ferrite cell-walls which enclose kernels first of austenite and later of pearlite. From this theory that the mother austenite thus expels its offspring to its own outside I see no escape. This power of selection, rejection, and ejection, even in the case of crystals growing slowly in an aqueous solution, is certainly a remarkable even if an explicable one. But in bringing about the extremely rapid ejection of ferrite in so viscous a substance as dull-red-hot if not black steel, it is even more remarkable. Thus Osmond[*] found that when a wad of steel of

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[*] *Microscopic Analysis of Metals*, Osmond and Stead, Fig. 63; p. 122.

This is not an isolated instance of extremely rapid crystalline movement. Thus Goerens, and later Hadfield and Hopkinson, show micrographs of cast iron quenched from the molten state which prove that even in this almost instantaneous solidification the constituents have crystallized out prominently. Goerens quenched apparently small lots from the molten state in ice water. His micrographs show a well-marked eutectic structure even under small

0.14 per cent. carbon, only 0.28 in. (7 mm.) thick and 0.48 to 0.52 in. (12 to 13 mm.) in diameter, was quenched in water from 1340°, this ejection of ferrite occurred, as is indicated by the existence of a ferrite network in the quenched steel. This ejection of ferrite must have occurred chiefly during the extremely rapid passage through the transformation range, which must have been lowered by the rapid cooling so that its lower end was near 400°, that is, below visible redness.

Granting the existence of this power, we may suppose that, with either long or high heating and the implied opportunity for molecular rearrangement, the crystalline organization becomes more perfect and hence more stable and efficient. This increase of perfection, stability, and efficiency, explains with a certain degree of ease the seven ulterior effects of long high heating, as follows:

(1) The greater crystalline stability explains the greater opposition to undergoing the transformation on cooling, for this transformation is the abandonment of the organization thus made stable. Hence the greater lag of the transformation, as shown in the lowering of the transformation range.

(2) The greater stability of the red-hardness of high-speed steel caused by long high heating may be explained in like manner, *mutatis mutandis*.

(3) The greater crystalline efficiency explains the reported larger extent of the areas free from coagulated ferrite in air-cooled steel, because it might well lead to a more thorough expulsion of the ferrite, in cooling through the transformation range, from the mother austenite grains into the grain boundaries and into their cleavages, leaving the unexpelled ferrite within the remainder of the mass both less in total quantity and more scattered, and on both accounts less able to coalesce into visible masses in the relatively rapid air-cooling. Hence the large areas free from coagulated ferrite.

(4) In like manner the greater prominence of the octahedral

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magnification (× 50), and under larger magnification a very highly organized crystalline structure. (Vorgänge bei der Erstarrung und Umwandlung von Eisenkohlenstofflegierungen, Halle a. S., 1907, pp. 33 to 35, Figs. 24 to 27; *Metallurgie*, Vol. 4, 1907, Figs. 111 to 114.) Hadfield and Hopkinson show under moderate magnification (× 120) the familiar primary austenite crystals, well oriented, and an easily distinguished eutectic structure. (*Institute of Electrical Engineers*, Vol. 46, 1911, Fig. 9, opposite p. 260.)

cleavage-massing of the ferrite, shown in the Widmanstättian figuring of highly heated steel, explains itself as a natural result of the greater efficiency of the crystalline organization as an agency for expelling the ferrite and concentrating it in the cleavages as well as at the grain boundaries.

(5) So, too, the greater efficiency of the crystalline organization as a mechanism for expelling the ferrite or cementite in the transit down through the transformation range, and massing it in the grain boundaries, would naturally lead to a thicker massing there of that ferrite or cementite. And these masses, because they are so much thicker, would naturally resist the longer the tendencies which break up the network structure, and in particular would be less acted on by the spheroidizing tendency of surface tension, and would the longer persist as traceable cell-walls.

To look at it in a slightly different light, these cell-walls are pseudomorphs in any event below the transformation range. But the stability of even a pseudomorphic structure may increase with the efficiency of the genetic crystalline organization which created it, even after that genetic organization has abdicated.

It is true that, while high and long heating thus preserves the network structure which outlines the boundaries of what have been austenite grains, yet in addition it may tend to obscure those true boundaries by leading to the formation of strong cleavage massings, which cut these areas up in such a way that we do not make out what are the boundaries of austenite grains and what are the cleavages of those grains.

(6) In like manner the effect of such high heating in increasing the size of the ferrite grains which arise during later cooling is seen to be a natural result of this more thorough expulsion of the ferrite into boundaries and cleavages in cooling through the transformation range; for this implies its greater local concentration, and this in turn leads to its coalescing into larger islands.

Very-low-carbon steel towards the end of the transformation consists almost wholly of ferrite which the initial austenite has progressively expelled, thereby reducing itself to an extremely small mass. Here the ferrite thus expelled seems to inherit approximately the grain size of the austenite whence it sprang.

But this seems to be only a special case of this general law, that the several large ferrite islands formed by expulsion from large grains of austenite, remain large.

(7) The effect of long high heating in retarding the coagulation of the pearlite formed on cooling past Ar1, the lower limit of the transformation range, may be referred to such an increase in the stability of the austenite crystalline structure that, even when on cooling past Ar1 it ceases to be the government *de jure*, it may pseudomorphously retain much power *de facto*, and thereby impede the attempt of the material as a whole to break away from the distribution which that crystalline system has set up, thus impeding all re-distribution, including the slight convections which the coagulation of sorbite into pearlite implies.

Once we have looked at the matter from this point of view, we see that here is an additional explanation of effects (4) and (5), so that all of the seven effects except (6) are referable, at least in part, to the greater stability as such of the austenite crystalline system caused by long high heating.

To this hypothesis of the greater perfection of the crystalline organization caused by long high heating it may be objected that, at the instant when a given grain is abandoning its initial crystalline orientation and adopting that of the grain which absorbs it, it does not seem to be in a very stable condition. The rapid grain growth at high temperatures carries with it a suggestion of flux which we do not at first identify with stability.

To this it seems a fair reply that after all the question is one of greater or less stability. The grain growth at higher temperatures is surely an approach towards greater stability. The lack of growth at lower temperatures is evidence not of stability but of frictional opposition to the approach to stability. And if, even after long high heating, crystalline growth is still going on, that simply means that, though the condition is more stable than it was before that growth began, it has not yet approached complete stability so closely that growth ceases.

Having now considered the alleged strengthening of the crystalline organization as an explanation of the seven effects now before us, let us turn to the other explanation, the effacement of initial heterogeneousness through diffusion.

30. (*b*) *Effacement of Initial Heterogeneousness.*—Hypo-eutectoid steel such as is shown in Fig. D of Row 4, Plate II, when it has been coarsened by long high heating, consists of islands of ferrite, *AAA*, Fig. 3, embedded in a ground mass of pearlite, which in turn consists of alternate stripes of cementite, *B*, and ferrite, *C*. If it is heated barely above the bottom of the transformation range and immediately cooled again, these ferrite islands will persist and re-appear. There has been nothing to efface them. It is true that the twin constituents of the pearlite react quickly on each other at Ac1 to form austenite, but at this temperature they do not invade the ferrite islands, *A*. But as the temperature is carried higher and higher, the ferrite islands are progressively re-absorbed by the surrounding austenite, and their re-absorption is due to complete itself at the top of the transformation range, Ac3. This re-absorption in case of hyper-eutectoid steel is so fast that the free cementite may cease to be visible after 4½ minutes at 975°* as is pointed out under Proposition 4, Paragraph 52, page 333.

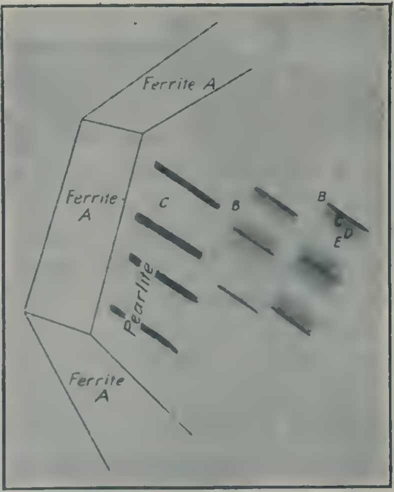

FIG. 3.

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* Jung, *Internationale Zeitschrift fur Metallographie*, Vol. 1, 1911, p. 215.

But re-absorption does not imply equalization; and though no visible traces of ferrite may remain at the end of 4½ minutes at 975°, yet complete equalization may take a very much longer time. It of course occurs through the immigration of hordes of carbon particles from the austenite ground mass into the ferrite islands, and the corresponding emigration of iron particles to make room for them. The distances to be covered are considerable, and the equalizing process spontaneously retards itself as it nears completion.

I wish to direct attention to the probable existence of imperfectly equalized spots, consisting indeed of austenite, but richer in iron than the surrounding austenite; and to the gradual reduction of their excess holdings of iron by long and high heating.

This helps to explain why such heating has the first three and the last of the seven ulterior results enumerated above, but not why it has the rest. Let us again take these up under (1) to (7).

31. (1) The increase of equalization caused by long high heating might increase the stability of the *status quo* and hence oppose the transformation in cooling, and thereby lower the transformation range, by scattering the carbon so that it comes into relations with an ever increasing number of particles of iron. As an essential part of the transformation is the formation of cementite, Fe_3C, so an essential part of it is that each molecule of carbon must pass out of relation with all the iron molecules in excess of three, and restrict its relations to three. We can readily understand that this disentanglement of its relations should be the more difficult the greater the number of iron molecules from which each carbon molecule has to obtain release; in other words, that a result of the diffusion or scattering of the carbon should be an increasing difficulty in the re-forming of cementite, and through this an increasing difficulty in undergoing the transformation, of which the formation of cementite is an essential part.

It is wholly in accordance with this view that the actual observed position of Ar1 is the lower the lower the carbon content of the steel.* But, as the phase rule implies that this reaction

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* Osmond, *Transformations du Fer et du Carbone*, Paris, 1888, Table, p. 41. Carpenter and Keeling, *Journal, Iron and Steel Institute*, Vol. 1, 1904, p. 233 and Plate XVIII.

In Spiecker's cooling curves (*Metallurgie*, Vol. 6, 1909, Fig. 391, facing p. 520), as the is due always at one and the same temperature quite irrespective of the carbon content, this means that the more iron the carbon enters into relation with, the more does the change from austenite into cementite lag; which, if true, agrees with my explanation of the influence of long high heating in lowering the recalescence, Ar1.

It is, indeed, possible that in like manner the progressive effacement of the concentration of carbon into the cementite particles of the pearlite may contribute to the lowering of the recalescence Ar1, caused by long high heating. Minute as these particles are, their conversion into austenite by union with the ferrite with which they are initially intertwined does not necessarily imply instantaneous equalization of composition of the resultant austenite.

In Fig. 3 I sketch this process of equalization. It is quite possible that the space originally occupied by the axis of a given cementite spine *B* of the pearlite, may for some time remain richer in carbon than the space *C* initially midway between two such spines; and that the progress of equalization and the resultant impoverishment of these spaces may here throw an obstacle in the way of the re-formation of that cementite on re-cooling.

32. (2) The increased persistence of red-hardness in high-speed steel may in this same way be explained by increased equalization, *mutatis mutandis*.

(3) Such increase of equalization by long high heating readily explains the increase in the areas free from coagulated ferrite (see Paragraph 35, page 307) in air-cooled steel. During cooling through the transformation range, the quantity of ferrite generated in any spot must increase with the richness of that spot in iron, so that wherever an initial enrichment in ferrite persists, there will the ferrite the more readily coalesce into visible islets. In that equalization lessens such local enrichment, it opposes the re-concentration there of ferrite into visible masses, or in other words increases the areas free from coagulated ferrite.

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carbon content decreases from 0.89 per cent. there is a progressive lowering of Ar1. It is naturally rather irregular, because in any individual specimen the lag varies with the attendant conditions. Wust says that this descent exists only among those of Spiecker's steels which have less than 0.40 per cent. of carbon, but this seems to be a typographical error. (*Idem*, p. 524.)

(7) Like considerations may conceivably explain the effect of high heating in retarding the coagulation of the pearlite on cooling below the transformation range. As long as the scattering of the carbon falls ever so slightly short of completion, so that the spine *B*, of the area where a cementite layer of the previous pearlite had been, remains ever so slightly richer in carbon than the layer *E* midway between spines, on the re-birth of the pearlite on cooling past Ar1 the slight extra richness of that spine may suffice to cause a rapid concentration of the new-born cementite towards *B*, and so give rise to well-marked cementite centers, about which coagulation would be rapid. Thus there would be relatively few dominant lines *B*, representing the previous cementite spines, the attraction of which would so far outweigh that of the intermediate lines like *C*, *D*, and *E* that coagulation about *B* would be relatively rapid. Still higher heating and further scattering of the carbon from *B* would proportionally weaken it as a center for coagulation in the ensuing cooling, giving *C*, *D*, and *E* a chance to set up their minor local centers of coagulation. The result would be that coagulation about a much larger number of nearly evenly balanced centers of attraction would be much longer in developing aggregations large enough to be visible, or in other words that the coagulation of the irresoluble sorbite into resoluble pearlite would be retarded.

(4), (5), and (6). But I do not see how effects (4), (5), and (6) are to be explained by this increase of equalization. Thus I fall back on (*a*), the strengthening of the crystallizing organization, as the only hypothesis by which I can explain these phenomena as a whole. It is to be hoped that some more cogent explanation may be discovered.

33. *Principle 11. The Network Structure in Very-Low-Carbon Steel.*—The transformation of very-low-carbon steel in cooling generates so great a quantity of ferrite, and reduces the residual austenite to so small a mass, that the ferrite walls of the resultant cells have the appearance rather of a ground mass in which the residual kernels of austenite, and of the pearlite into which it transforms on passing below the transformation range, are suspended. And the eating away of these kernels as they progressively shrink through generating more and more ferrite, may take place so unevenly as to leave them as long

narrow islands, so that they suggest a network structure in which they form the cell-walls and the ferrite groundwork forms the kernels. The disentwining of the pearlite, the absorption of its ferrite by the ferrite cell-walls, and the balling up of its cementite, further conceal the network structure.

In interpreting the structure its genesis should be borne in mind. In its light the structure often seems to be truly reticular, but with extremely thick ferrite cell-walls and very small distorted pearlite kernels. In this view, coarse austenite grains in very-low-carbon steel should, in passing through the transformation range, yield coarse grains of ferrite.

34. *Principle 12. The development of cell-walls requires a certain length of time.*—During the cooling through the transformation range the ferrite is generated, if not in the form of single atoms quickly grouping themselves into molecules, at most in the form of particles so much smaller than the wave lengths of light that to make them visible is probably far beyond the powers not only of our present microscopes but of all future ones relying on such light as we now know. They become distinguishable from the surrounding pearlite, that is, visible, only through coalescence, which in so sluggish a solid as red-hot steel may take an appreciable time. Hence a certain degree of slowness of cooling is needed to enable these ferrite particles to coalesce into visible masses, and thus to reveal the whole of the network structure. This is in accord with Dr. Boynton's observation that the network structure was lacking in his hyper-eutectoid steels when air-cooled and that it became prominent when they were furnace-cooled. It is true that even an air-cooling may afford the time needed for the ferrite in hypo-eutectoid steel to coalesce into visible masses, but we readily understand that such a specific difference in behavior between the ferrite and the cementite in these cases might exist, whether because the coalescence of cementite is habitually slower than that of ferrite, or because the cementite in this particular hyper-eutectoid steel was slow to coalesce into visible masses because it was so scanty.*

This, indeed, is an important difference between ferrite and cementite in general. The largest quantity of free cementite which can be generated in any steel in cooling through the transformation range should be about 14 per cent. of the whole mass; whereas the ferrite generated in such cooling should form 56 per cent. of the whole mass even in a 0.40-per cent. carbon steel, and 89 per cent. in a 0.10-per cent. carbon steel.

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* Compare Paragraphs 12 and 17, pp. 271, 283; Proposition 10, Paragraph 63, p. 357; and Boynton, *Iron and Steel Magazine*, Vol. 7, 1904, p. 477.

As regards coagulation, what is thus true of the free ferrite and cementite generated in cooling through the transformation range, is true also of the pearlitic ferrite and cementite. A cooling at a properly chosen intermediate rate allows the transformation to complete itself in very large part or perhaps completely, without allowing time for the resultant ferrite and cementite to coalesce into visible masses, the free ferrite or cementite into a network, etc., the pearlitic ferrite and cementite into pearlite. These uncoagulated irresoluble conglomerates are called "troostite" if the transformation still falls materially short of completion, and sorbite if it is nearly or quite complete.

35. *Definitions: Coagulated and Uncoagulated Ferrite and Cementite.*—Such iron and such cementite as have thus ceased to be dissolved but have not yet coalesced into masses which can be detected microscopically, I call *uncoagulated;* those which have coalesced so far that they can be detected I call *coagulated.* The uncoagulated might indeed be called "sorbitic" or "emulsified" or "invisible." Though this difference is purely one of degree, and though a magnification of 1,600 diameters detects some ferrite which a magnification of 40 diameters cannot, yet the distinction is useful for the reason which makes that between mountains and hills, beef and veal, giants and dwarfs, and men and boys useful, though each of these differences is in like manner one purely of degree.

36. *Principle 13. Progress of Structural Development.*—The hypothesis (Principle 9, Paragraph 26, page 295) that the massing of the ferrite is densest at the grain boundaries, next in density at the grain cleavages, and least dense in the remainder of the grain, naturally implies that, during the coalescence of that ferrite so distributed, that about the grain boundaries should be the first to form masses thick enough to be seen, that in the cleavages should be next, and that scattered at random through the grain should be last. Hence, as the coalescence

proceeds during slow cooling, the true network structure should be the first to become manifest; the grain cleavages second; and the irregular islets of ferrite representing that which has not been thus distributed but remains scattered at random through the grains, should be last.

37. *Principle 14. Spheroidizing and Granulation.*—When the ferrite in the grain boundaries first coalesces sufficiently to form visible masses, it forms continuous cell-walls. But, as they grow thicker, those walls in turn tend to break down by a spheroidizing process, the tendency of the ferrite to break away from the form of thin sheets and to assume that of small spheres, just as when three drops of oil standing in line are brought into actual contact, their surface tension tends to draw them together so as to form a single sphere. The cell-wall ferrite can obey this spheroidizing tendency, this tendency to break away from military line into skirmishing groups, without strong opposition from the austenite grains which it encloses. This spheroidizing of the cell-walls is the beginning of the breaking up of the network structure.

38. *Principle 15.—The break-up of the network structure,* thus started, continues through the coalescence of the ferrite, scattered through the pearlite grains or cell-kernels, into irregular islets. These mask and distract attention from the cell-wall ferrite, which at the same time further breaks away from its initial continuous shell arrangement, both by further spheroidizing and by coalescence with these irregular intra-mural ferrite islets. As a result the network structure gradually becomes unrecognizable.

The progress of this break-up is illustrated by the series of micrographs, Figs. 18 to 22, Plate I, which show the structure of Steel I, cooled to different points in the transformation range slowly enough to allow the break-up of the network structure,*

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* Five wads of Steel I (0.40 per cent. carbon), ⅜ in. in diameter and about ½ in. long, were heated in an atmosphere rich in carbonic oxide, in the closed porcelain tube of a Heræus furnace, to 1000° for 10 minutes, cooled slowly in the furnace to the temperatures indicated, and then quenched in cold water, except No. 22, which was cooled slowly to room temperature. Nos. 18, 19, and 22 were treated in one operation, and Nos. 20 and 21 in a second. No. 19 was then reheated to 740° under like conditions, held there for 6 hours, and then quenched in cold water, when it had the structure shown in Fig. 23. In the interval between quenching any given specimen and quenching the preceding, the thermo-couple lay exactly above that specimen

Thus the two series of specimens, Figs. 12 to 22 and 24 to 28, are of the same steel after in contrast to the parallel Series 24 to 28 of the same steel cooled slowly enough to allow the network to form, yet not slowly enough to allow it to break up again. An early stage of the spheroidizing of the cell-wall ferrite is shown in Fig. 19, in which the network structure is still evident; but the cell-walls, instead of consisting of thin, moderately regular sheets, as occurs when the cooling is moderately rapid, as for instance in the air or in cold powdered clay (Figs. 24 to 28, Plate I, and Rows 5 to 8, Plate III), have drawn together into spheroidal masses. Figs. 20, 21 and 22, Plate I, show the structure of the same steel cooled very slowly within the furnace to points still lower in the transformation range or below it, and then quenched in water to fix the structure then formed. By the time that the very slow cooling had reached 730° the cellular structure had become very vague; and by the time 650° was reached it had become unrecognizable.

Again, comparing (1) Figs. E and F of Row 10, (2) Figs. B, C, D of Row 11, and (3) Figs. B, C, D of Row 12, Plate IV, of which (1) represents the slowest and (3) the fastest cooling, we note the early stage of development in Row 12, with its thin well-marked lines of ferrite; a later stage in Row 11 in the slower cooling of which the cell-walls have indeed thickened greatly, only to begin their course of degeneration into globular masses; and a still later stage in E of Row 10, cooled still more slowly, where these globules, though leaving the network structure still traceable, have traveled farther on the downward path, assuming the granular shape common in fully de-reticulated specimens.

This granular stage shows us that some influence in addition to surface tension is here at work, apparently the attempt of the ferrite grains to build into idiomorphic crystals, perhaps both pushing aside the pearlite in which they are enclosed, and drawing on its ferrite. This angular ferrite structure may be connected with the large manganese content of this steel.

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treatment which was in general alike, save that the specimens shown in Figs. 24 to 28 underwent a retarded air-cooling, which was very much less slow than the furnace-cooling of the specimens shown in Figs. 19 to 22, the faster cooling preventing and the slower cooling allowing the break-up of the network. Beyond this, each of the latter specimens was a separate piece, drawn and quenched separately when it had cooled as far as its individual quenching temperature.

39. *Principle 16.—Thus the network structure represents an intermediate stage of the general structural development,* when the ferrite massed about the austenite grain boundaries has had time to coalesce into visible cell-walls, and before it has had time to obey surface tension so as to spheroidize, and break ranks. Hence the facts (1) that though a very rapid cooling, as by quenching in water, so far restrains coalescence that whatever free ferrite is generated remains invisible; (2) yet a somewhat slower cooling, as in the air, while it gives time for the coalescence in the grain boundaries (shells) to go far enough to mass the ferrite there into visible cell-walls, does not enable the more scattered ferrite within the bodies of the several grains to coalesce into visible masses, with the consequence that the network structure is very striking; and (3) a very slow cooling, as in the furnace, allows the ferrite within the individual grains to coalesce to such an extent as to distract attention from the cell-walls and to mask them, and the cell-walls themselves to break ranks, as more fully set forth under Principles 14 and 15, Paragraphs 37 and 38, page 308. Sauveur and Boynton* called attention in 1903 to this fact, that whereas steel cooled moderately slowly, as for instance in the air, had a network structure, "very slow cooling . . . from similar temperatures was always accompanied by the formation of a granulated structure."

A rapid cooling which sets in above the transformation range and hence before the generation of the ferrite cell-walls has begun, seems to preserve the grain boundaries of the austenite into which the ferrite would shortly have begun massing if the cooling had remained slow till the transformation range was entered. At least so I interpret the dark grain boundaries in Fig. 18, Plate I, which shows the structure of a 0.40-per cent. carbon steel so treated. The polygons here I take to be wall-less cells or grains, each representing what was a grain of austenite while the metal was in Region 4.

40. *Principle 17. The Cleavage Massing of the Ferrite.*—(See Principle 10, Item 4, Paragraph 28, page 297). This is represented by the well-marked parallel white lines in Figs. E and F of Row 4, Plate II, where they co-exist with a very strongly

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* *The Metallographist,* Vol. 6, 1903, p. 148. But I have not noticed that they give any explanation of these facts, such as I here offer.

marked network structure; by the parallel offshoots of white ferrite which run from the cell-walls into the kernels themselves in Figs. C, D, and E of Row 5, and D and E of Row 6, Plate III, where too they co-exist with the network structure; and in general in the familiar Widmanstättian figuring like that of meteoric iron, in the parallel white ferrite lines in Fig. 22, Plate I, and Fig. F of Row 10, Plate IV, in which the network structure has passed away.

The life history of these cleavage massings seems to be like that of the cell-walls themselves, first a coalescing into lines thick enough to be seen, and then a breaking up of those lines through spheroidizing. Thus the presence of these lines, like that of a well-marked network, represents an intermediate stage in the structural development, a stage at which these lines have had time to form but not yet time to break up again into skirmishing groups. Because the massing is less strong in the cleavage planes than in the grain boundaries, the stage in which they are well marked should be a later stage than that in which the network structure is well marked. Moreover, as the cleavage massing differs in density from place to place, so the cleavage lines may be found in very different stages of the structural development. Look, for instance, at those in Figs. E and F of Row 4, Plate II, which co-exist with a strongly marked network structure, that is, in a relatively early stage; and again at those in Fig. 22, Plate I, which are well marked long after the network structure has become unrecognizable. These latter cleavage lines we may take to represent a relatively sparse massing, which passes through the stage of clearly marked lines at a late stage of the general structural development.

The break-down of these parallel cleavage massings is probably the easier because they are pseudomorphs. That is to say, they represent the massing of ferrite, generated during the passage down through the transformation range, in the octahedral cleavages of the then existing austenite which bore that ferrite. The ferrite thus massed is pseudomorphous, that is, actually distributed octahedrally though it is not itself octahedral by nature. When the cooling passes the bottom of the transformation range, and the residual austenite changes into pearlite, that government which has caused this octahedral cleavage massing abdicates, thereby removing an impediment to the re-distribution of the ferrite according to its natural habit.

That the cleavage massing does tend to break up is indicated not only by its frequent absence from the micrographs of slowly cooled steel in which it should once have existed, but also by its apparent destruction by Benedicks, by means of an extremely slow cooling below the transformation range and its preservation in a somewhat faster cooling below that range.*

That it can be made extremely persistent by long high heating is shown primarily by its habitual presence in meteoric irons in spite of their presumably extremely slow cooling,† and secondarily by some experiments of mine. The cleavage massing of Figs. E and F of Row 4, Plate II, fixed by holding at 1200° and 1300°, neither spheroidized nor gave any other sign of breaking ranks after later holding at 650° for 6 hours and again at 700° for nearly 6 hours.

We can readily understand that these cleavage masses might, under favorable conditions, persist not only for a long time but forever. For instance, if particles of inert non-spheroidizing slag should be scattered through them, their adhesion to these might be stronger than their spheroidizing tendency due to surface tension, and thus they might resist spheroidizing permanently. (See end of Paragraph 42, page 316.)

41. *Principle 18. Natal and Post-Natal Influences.*—We have now seen that, though the ferrite which forms the cell-walls, and the ferrite grains scattered within the cells, do not come into existence until cooling reaches the transformation range, and though in consequence those walls and grains are formed in and below that range, nevertheless their size, shape,

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* Synthese du Fer Meteorique, *Nova Acta Regiæ Societatis Scientiarum Upsaliensis*, Series IV, Vol. 2, 1910, p. 23 of reprint.

† Osmond reasonably says that "for meteorites, which we regard as fragments of planets, the coolings are habitually infinitely slow compared with those which we call slow." (Sur les Fers Meteoriques, *Revue de Metallurgie, Memoires*, Vol. 1, 1904, p. 78.

It is true that the presence of the Widmanstättian or octahedral cleavage massing in meteoric iron suggested to Benedicks (*op. cit.* p. 22) that the cooling of this iron below its transformation range must have been relatively rapid, because, had it been very slow, this cleavage massing would have broken up. But he probably overlooked the fact that the long very high heating might fix this structure so that it would persist even through an extremely slow cooling. Certainly his later writings accept the slowness of cooling from the transformation range down (Das Ovifaker Eisen, *Metallurgie*, Vol. 8, 1910, p. 66, and Ein Synthese von Meteoreisen, *Internationaler Kongress fur Bergbau*, etc., Dusseldorf, 1910. Berichte, Abteilung Theoretisches Hüttenwesen, pp. 3-7).

and general arrangement, as we reveal them by means of our micrographs, are profoundly affected by the pre-natal conditions, namely, the temperature reached in Region 4 and the length of exposure to it. From this point of view the disposition of the ferrite masses, whether in cell-walls, cleavages, or erratic islets, and the size and shape of those masses, are dependent on two radically distinct things, (1) pre-natal and (2) natal and post-natal conditions. First, the crystalline structure of the austenite formed during the sojourn in Region 4, its growth into larger or smaller grains, with greater or less power of expelling, distributing, and massing in its own boundaries and cleavages the ferrite as it generates it later, that is, the pre-natal condition. Second, the natal and post-natal conditions, the opportunity for the ferrite so distributed and concentrated during its birth by the mother austenite, to coalesce during the life and after the death of its parent. The beginning of this coalescence of the ferrite so concentrated in the cell boundaries, leads to the development, as by a photographic developer, of the outlines of the austenite grains, revealing the network structure. This is the natal and post-natal coalescence of the ferrite concentrated at its birth into the cell boundaries. The coalescence continues in certain cases by developing in like manner the pre-natal cleavages which had existed in the austenite, the trace of their existence being the parallel ferrite bands and the Widmanstättian figuring revealed by the natal and post-natal coalescence of the ferrite concentrated there in its birth. The coalescence further continues by the break-up of the network structure, as the ferrite masses which constituted that structure further coalesce into masses irrelated to the now past pre-natal conditions.

42. *Principle 19. Grain Refining of Steel Other than Ultra-Low-Carbon Steel.*—We have considered under Principle 8, Paragraph 25, page 294, the grain refining of ultra-low-carbon steel; and under Principles 9, and 12 to 16, Paragraphs 26 and 34 to 39, pages 295, 306–310, the genesis of the walled cells and ferrite grains of steel containing any important quantity of carbon; in short, the genesis and coarsening of its heterogeneousness. It remains to consider the breaking up of this coarse heterogeneousness, or grain refining.

Whether the network structure remains manifest or is masked, it is evident that the grain refining of a steel of any considerable carbon content is more complex than that of simple dead-low-carbon steel. This latter consists practically of ferrite alone; and its grain refining consists simply in breaking up the coarse grains of ferrite and replacing them with fine grains without implying any migration of any molecule.

But a glance at Fig. E of Row 4, Plate II, shows that this would not suffice to refine a coarsened steel, say of 0.40 per cent. carbon. Suppose for the moment that each islet of ferrite in it is composed of a single coarse grain; and that by some means each of these islets, though remaining intact as an islet and without change of its size or shape as a whole, has its crystalline structure changed in such a way as to convert it into a vast number of minute grains, the molecules of each of which remain in place during the change. That would be indeed a grain refining of the ferrite, but not of the conglomerate of which the steel as a whole consists. For that steel would still retain its coarse conglomerate structure, with coarse islets of fine-grained ferrite in a ground mass of pearlite. Such a conglomerate would still be coarse grained, just as a concrete made with coarse lumps of quartzite would remain coarse-grained, even though each of those lumps of quartzite were itself composed of myriads of ultra-microscopic grains of quartz.

In order to remove this feature of the grain-coarseness these islets of ferrite must be effaced. This is true whether the network structure in the coarse-grained steel is still manifest or has been masked; and it is true not only of the ferrite of the cell-walls but also of the ferrite islets scattered through the kernels themselves. This effacement can occur only through the re-absorption of the ferrite islets by the austenite which surrounds them, as the steel rises through and out of the transformation range, and can complete itself only on rising past the temperature Ac3, as explained in Paragraphs 5 and 6, pages 265, 267. Thus Ac3 is the grain-refining temperature.

That this re-absorption cannot be instantaneous is self-evident.

To fix our ideas let us assume that there is a well-marked network structure. Re-absorption here implies the equalizing of the composition, giving the places initially occupied by the ferrite (or cementite) cell-walls the same composition as the kernels which they enclose; and this equalizing can take place only through a double process of diffusion. In the case of hypo-eutectoid steel these walls contain more iron than the kernels, and they lack carbon nearly or quite completely. The process of equalizing then necessarily implies (1) the diffusion of enough carbon from within the kernels into the cell-walls to give the whole a uniform carbon content; and (2) a simultaneous outflow of enough iron from those cell-walls into the kernels to give a uniform iron content throughout. Such uniformity moreover implies a movement which extends from the center of each kernel to the middle of each cell-wall, and the actual migration of certain of the molecules of carbon through a distance at least equal to half the thickness of the cell-walls. It is true that this process of equalization through re-absorption and diffusion is going on through the whole of the passage up through the transformation range; so that, if that passage is only slow enough, a very large part of the equalization will have occurred by the time the upper limit Ac3 of that range is reached. Hence the length of sojourn above Ac3 needed to effect the last touches of equalization should be inversely as the time occupied in reaching Ac3.

I wish to emphasize my opinion that thorough grain refining requires a complete or very nearly complete equalization of carbon content, or the effacement of the initial impoverishment in carbon along the cell-walls and ferrite islets. As long as this local impoverishment in carbon remains uneffaced, the mass has in a sense a coarse structure. That is to say, there lie within it regions poor in carbon, separated from each other by wide regions richer in carbon; and the very fact that these higher-carbon regions separating the lower-carbon ones are wide, in and by itself means a coarseness of heterogeneousness, and in that sense a coarseness of structure, the existence of large regions of greater tenacity and less ductility, alternating with other regions of less tenacity and greater ductility. This coarse heterogeneousness is harmful.

This conception seems to me very important, and a mass of our phenomena fall into line if seen from this standpoint. A steel which has been imperfectly grain-refined by too short a holding above Ac3, when again reheated, for example to 650°, for annealing, very readily develops milky ways. These are strong evidence that the effacement of the cell-wall and ferrite-grain impoverishment is incomplete. It means that, where these milky ways arise, there the carbon content as a whole is lower than in the rest of the mass, with the result that there the ferrite is in greater excess over the pearlite, or that there the sorbite is richer in ferrite and poorer in cementite, both of them to be sure indistinguishable under the microscope. And where the ferrite is in greater excess, there it the more readily coalesces into the visible masses of these milky ways. We have seen that the coarse heterogeneousness, the coarse network and granular structure arise through the coarsening of the austenite grains in Region 4, followed by the fixing of their coarseness by the concentration of ferrite in their boundaries in cooling through the transformation range, which concentration of ferrite is in effect a local impoverishment in carbon. When the steel thus coarsened is reheated above Ac3 into Region 4 for the purpose of grain refining, these carbon-poor regions are immediately invaded by carbon from the surrounding austenite, and if held in Region 4 long enough this invasion will restore a uniform carbon content. But until this diffusion has become complete, the steel when again cooled will retain this local impoverishment in carbon, with the consequence that milky ways and ghost lines readily reappear there.

From this point of view the influence of slag in impeding grain refining may well be in large part mechanical. It impedes mechanically the invasion of the impoverished regions, the ferrite cell-walls and ferrite grains, by the migrating carbon, and thus tends to perpetuate the local impoverishment. Hence, because the slag-bearing areas when re-cooled past Ar1 into Region 8A remain impoverished in carbon, and hence richer in ferrite, than the surrounding areas, there the ferrite, because more abundant, the more quickly coalesces into visible masses. In addition the slag may have the precipitating effect generally ascribed to it, provoking the coalescence of the local ferrite into visible masses somewhat as a crumb of bread provokes the evolution of gas in a glass of champagne. Further study must determine which of these two possible causes is the more important. Slag may conceivably act in a third way, impoverishing the mass locally in carbon by oxidizing that carbon.

43. *Principle 20. Normal and Local Ac3 or Grain-Refining Temperature.*—In passing we may note that the grain refining of the ferrite islands themselves ought not to be expected to occur at the Ac3 of the given steel, say 825° C. in our present case of 0.40-per cent. carbon steel, so long as they remain unabsorbed ferrite islets. This is seen most readily by considering the case of the interior of one of those islets to which we may for the moment assume that no carbon at all has penetrated. Steel in which such unabsorbed islands exist is in effect a conglomerate, in this case and at this high temperature of austenite with unabsorbed pure ferrite islands. As regards undergoing the Ac3 transformation from beta ferrite into austenite, these islands are a law unto themselves. They will transform when they reach the temperature at which they, taken apart and in and by themselves, are due to transform, allowance of course being made for lag, or at about 895°. The fact that neighboring masses with a higher carbon content are transforming when the mass as a whole reaches the temperature which corresponds to Ac3 for their particular carbon content, say 750° if their carbon content is about 0.75 per cent., does not cause these unabsorbed ferrite islands to transform before they reach the higher temperature, 895°, at which in virtue of their lack of carbon they are due to transform.

Thus we must distinguish between the normal Ac3 temperature which applies to the steel as a whole after its carbon content has become uniform throughout by diffusion, and the local Ac3's of its different constituent parts, so long as its carbon content varies from place to place because of incomplete diffusion. In those parts where unabsorbed ferrite islands remain, the local Ac3 is above that of the steel as a whole; in the remaining parts, forced to be richer in carbon than the normal by the very fact that those ferrite islands are poorer than the normal, the local Ac3 is below the normal Ac3. And, following the same reasoning, as the carbon gradually diffuses into each spot which was initially a ferrite island, and the carbon content of that spot correspondingly increases, that rise of carbon content causes a corresponding descent of the temperature Ac3 needed for converting it into austenite, and hence presumably needed for breaking up its coarse grain.

Indeed it follows from the phase rule that a given spot where a ferrite island has been, cannot change into austenite at the normal Ac3 temperature of the given steel, and thereby undergo the grain shattering, unless its carbon content is at least as high as that for which this temperature is the Ac3 point, which is equivalent to saying, unless it has the carbon content of the given steel as a whole; which in turn is equivalent to saying that it cannot so change and be refined till equalization is complete, because not till then can such a spot reach that carbon content.

44. Up to this point we have considered the grain refining by the re-absorption of cell-walls and of the ferrite islets alone. It remains to consider that of the remainder of the mass. In the slowly cooled steel this remainder consists of pearlite. In heating up, this pearlite changes into austenite when the temperature rises past Ac1, and this change is probably rapid because the two constituents which have to reach and diffuse into each are in general so extremely fine and so intimately mixed. It is doubtful whether the pearlite can be said to have any true, that is, idiomorphic, grain or crystalline structure of its own; we have rather to suppose that it consists of extremely minute crystals of ferrite and others of cementite, each with its own independent crystalline structure. Even the fine structure of these is probably shattered when they merge and change into austenite at Ac1.

To sum this all up, the grain refining of steel of any considerable carbon content consists (1) in the shifting of its pearlite into austenite, which occurs spontaneously on rising past Ac1; (2) in the effacement of the islets of ferrite by diffusion, which can complete itself only at or above Ac3. This completion of the diffusion is preceded by a completion of the change of that ferrite into austenite, which change, as stated in Principle 8, Paragraph 25, page 294, is probably accompanied by a complete effacement of whatever grain size that ferrite had.

A natural consequence of this condition of affairs is that the grain refining of such steel requires heating to the Ac3 of the steel as a whole, but in order to effect it at Ac3 the sojourn there must be long enough to permit complete equalization by diffusion.

Exposure to any higher temperature would naturally hasten diffusion, but at the cost of coarsening the austenite grains in accordance with Principle 5, Paragraph 21, page 289.

45. *Principle 21. Fine-Grain Annealing.*—This grain refining has to be fixed by a cooling as rapid as is consistent with avoiding undue cooling stresses.

In such a rapid cooling much of the transformation is actually suppressed, so that the steel is actually hardened; that is to say, instead of consisting of a conglomerate of pearlite and ferrite as slowly cooled steel does, it consists in large part of substances intermediate between austenite and that mixture. Steel in this hardened state is unsuited to most uses; hence this hardening is usually undone by annealing, reheating to between 500° and 650°, at which temperatures the metal is mobile enough to allow the arrested transformation to proceed, so as to convert the whole, or nearly the whole, into a mixture of alpha ferrite plus pearlite, or more accurately sorbite, which is an uncoagulated conglomerate of (1) the components of this pearlite with (2) either this ferrite, or in the case of hyper-eutectoid steel, the corresponding uncoagulated cementite.

The advantage of thus substituting a rapid cooling plus a moderate reheating for a simple slow cooling from the grain-refining temperature is this: A slow cooling through the transformation range implies a long sojourn at the high temperatures of that range; and at those high temperatures the metal is so mobile that the ferrite which is generated progressively in passing down through that range can readily coalesce into harmfully large grains, and even into objectionably coarse network if the sojourn above that range had been high and long enough to re-form coarse austenite grains. But in a quenching or hardening followed by a reheating to between 500° and 650°, first whatever small quantity of ferrite is generated in the rapid passage down through the transformation range has but insignificant time given it to coalesce at the high temperature of that range; and when the mass is reheated to between 500° and 650°, and the suppressed generation of ferrite now occurs, the temperature is so low, and the metal therefore so rigid, that the coalescence of the ferrite into harmfully large masses is prevented.

So, too, the coalescence of the constituent ferrite and cementite of the pearlite itself, the ferrite particles flocking by themselves into rods or sheets, and the cementite particles by themselves into rods or sheets parallel with those of ferrite, is so far restricted that the resultant masses of ferrite and cementite respectively are indistinguishable under the strongest magnification, and form an ultra-microscopic emulsion, which Osmond has called "sorbite," after the illustrious founder of microscopic metallography.

We should recognize distinctly the part played by these several thermal movements. The grain refining is brought about by the sojourn just above the top of the transformation range, Ac3. The fineness of grain is fixed by a rapid cooling, which prevents the re-coarsening that would have occurred during a slow cooling through that range. The hardening which this rapid cooling incidentally causes is undone by reheating to a temperature (500° to 650°) high enough to allow the suppressed transformation from austenite into the constituents of pearlite plus ferrite to proceed, yet not high enough to allow that ferrite to coalesce into harmfully coarse particles. Hence the proposed name *fine-grain annealing.*

46. *Principle 22. Influence of Rate of Cooling on Network Size.*—We have seen under Principles 5, 7, and 9, Paragraphs 21, 23, and 26, pages 288, 291, and 295, that the true walled-cell or network size represents the size reached by the austenite grains; that this size increases with the opportunity for coalescence; that this opportunity does not exist below the transformation range; that growth is obstructed in the transformation range by the presence of the ferrite or cementite with which the austenite there forms a mechanical mixture; that this obstruction increases as the temperature descends through this range, and also, from specimen to specimen, as the carbon content departs in either way from the eutectoid ratio of 0.90 per cent.; but that coalescence becomes more rapid as the temperature rises farther above the transformation range, so that the advantageous opportunity for coalescence increases with the length and height of the heating above that range.

From these considerations we infer that the rate of cooling should affect the austenite grain size, and its successor the walled-cell size, only in so far as it affects the total opportunity for coalescence; and that in general an increased length of time spent in cooling should be less effective in increasing the walled-cell size than a like increase of the time spent at the highest temperature reached, because of the greater mobility at this higher temperature. In particular the rate of cooling through the transformation range should affect the true walled-cell size relatively little, though it may affect the apparent size, by affecting both the plainness with which the ferrite marks out those walled cells, and the degree to which the cleavage massings counterfeit the true grain outlines.

This principle of course must be applied with intelligence. In case of a very rapid heating to a high temperature followed immediately by a slow cooling, the opportunity for coalescence may be very much greater during the slow cooling than during the rapid heating and the momentary stay at the peak temperature; whereas, after a long holding at a high temperature, slow cooling should have little effect on the network size, because it increases the total opportunity for austenite grain growth by so small a fraction.

47. *Principle 23. Influence of Rate of Cooling on Grain Size.*—From the facts (1) that the size of the ferrite grains represents the degree to which the ferrite particles have coalesced, and (2) that this coalescence cannot begin till they come into existence in cooling through the transformation range, it follows that the ferrite grain size ought to increase with the slowness of cooling from the top of the transformation range down.

After a very short holding at the peak temperature a slow cooling above the transformation range may increase the ferrite grain size materially, because this slowness is an effective increase in the opportunity which the austenite has to perfect its crystalline organization, and through this to increase the local massing of ferrite as explained under Principle 10, Paragraph 29, page 300, which shows how long high heating comes to increase the ferrite grain size.

In the case of very-low-carbon steel, the ferrite grains of which seem to inherit the grain size of the austenite, slow cooling both above and below the transformation range may be expected to increase the ferrite grain size, above by increasing the austenite grain size (with the limitations set forth under Principle 22, Paragraph 46, page 320), and below by lengthening the opportunity for direct ferrite grain growth.

48. *Principle 24.- The effect of manganese and nickel,* severally and jointly, is to increase lag and otherwise to retard; in short to sluggardize. Look, for instance, at Osmond's original diagram of the nickel steels, Fig. 4. The gradual descent of Ac1 with rising nickel content shows, indeed, that nickel lowers the true or equilibrium position of A1; but the rapid widening of the gap between Ac1 and Ar1 with rising nickel content shows that the effect of nickel in increasing lag, in making the metal sluggish, is still more pronounced. We may imagine that, closely related as these three metals are, they are yet sufficiently unlike not to act interchangeably in the transformation; that the atoms of nickel and manganese are as it were foreign bodies, diluting the iron atoms all the more efficiently because they are

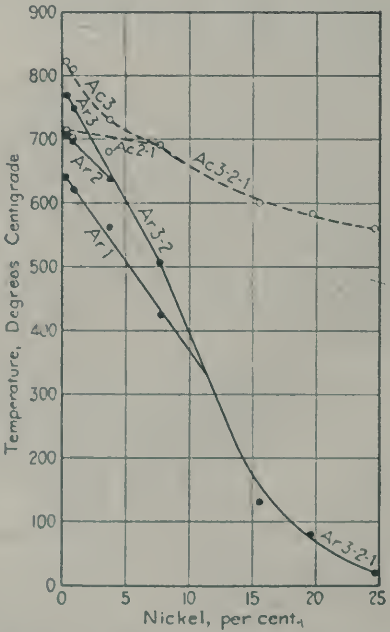

FIG. 4.—Osmond's Nickel-Iron Diagram.
(*Proceedings, Institution of Civil Engineers,* Vol. 138, 1898–99.)

so closely alike that they mix freely with them in the austenite, cementite, etc.*

It is as if ice had exactly the density of water at 0°, so that particles of ice and of water could remain intimately mixed. The resultant mixture would be very sluggish. Much less water could pass through a given canal if it were mixed in equal parts with fine particles of ice under these imaginary conditions, than under the actual conditions which leave the ice at the top and the water beneath free to run.

Twenty-five per cent. nickel steel and 12-per cent. manganese steel are both austenitic, or in other words have when cold the structure which common carbon steel has when above the transformation range in Region 4, because this large quantity of these retarding elements so retards the transformation that, even in usual slow cooling, it fails to occur. Steel of an intermediate content of nickel or manganese is martensitic when cold. that is, has the structure and properties characteristic of common carbon steel when it is in a state intermediate between that of austenite normal to Region 4 above the transformation range and that of pearlite normal to Regions 8A and 8B below that range, because this intermediate quantity of these retarding elements, though not enough to arrest the transformation completely, suffices to check it when it has gone as far as the martensite state. I do not here discuss the precipitation of cementite in the slow cooling of Hadfield's commercial manganese steel.

This same retarding effect in the case of manganese restrains the coalescence, whether of austenite when in Region 4, thereby lessening the coarseness of the network; of ferrite, during and after the transformation, into grains large enough to be visible, thereby lessening the ferrite grain size; or of the twin constituents of pearlite into lamellæ. Further, in restraining the coalescence of ferrite within the bodies of the grains as distinguished from the cell-walls, it restrains the break-up of the network structure to which that coalescence leads. The direct retarding effect of manganese on the coalescence during cooling is reinforced by its indirect effect in increasing the lag of the transformation, and so lowering the range of temperature through which that transformation occurs, so that the birth of the ferrite in passing through the transformation range and of pearlite at its end, Ar1, occurs at a lower temperature, at which of course the metal is more sluggish. In brief, sluggishness from lowering of the birth-temperature of ferrite and pearlite reinforces sluggishness due directly to the presence of manganese.

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* Osmond pointed out as early as 1888 certain of these retarding effects of manganese, namely, that it not only lowered the transformation range as a whole but made the transformation incomplete, so that even after slow cooling some of the iron was still in the beta state. (*Transformations du Fer et du Carbone*, pp. 48–51.)

PART III. PROPOSITIONS GOVERNING THE STRUCTURE OF CARBON STEEL.

49. *Proposition 1. Low-carbon steel (say 0.12 per cent. of carbon or less) coarsens progressively and without limit at temperatures between 600° and say 770°*, that is, in Region 7 and in the upper part of Region 8A. This proposition is amply proved by Stead's classical experiments,* in which he found not only that the ferrite grains might grow to very great size in this range, but that the size reached increased with the length of exposure.† Joisten has lately shown that this grain size increases not only with the temperature reached but also with the length of exposure.‡ Indeed, a prolongation of the exposure in certain of his experiments accelerates the grain growth to an extraordinary degree. His photographs and diagram, as well as Stead's results, show how very capricious the grain growth is. Concerning the influence of length of exposure see Paragraph 56, page 343.

Fay and Badlam also found a very slight grain growth in this region in their 0.07-per cent. carbon steel.§ The slightness of their growth is readily explained (1) by the probably brief sojourn in this range of temperature, and (2) by their very small number of observations, only one at 647° and one at 703°. All their others were at temperatures above the alpha ferrite range, that is, above A2, and hence in Region 4 or 6.

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* The Crystalline Structure of Iron and Steel; *Journal, Iron and Steel Institute*, Vol. 1, 1898, pp. 145-189. Brittleness Produced in Soft Steel by Annealing; *Idem*, Vol. 2, 1898, pp. 137-154.

† Dr. Stead specifically says of this grain growth, which occurs in the 48-hour annealing of sheet steel containing between 0.05 and 0.12 per cent. of carbon, "These changes are not effected rapidly. Time is an important factor." *Idem*, Vol. 2, 1898, p. 145.

He further says, "On slowly heating up carbonless iron to between 600° and 750° C., it develops an exceedingly coarse structure; in passing from 750° to 860° it appears to change very little, the coarse structure remains, but as soon as the heat rises above 870°, or thereabouts, it is replaced by one of fine degree." *Idem*, Vol. 1, 1898, p. 166.

‡ Einfluss der thermischen Behandlung auf die Korngrösse des Eisens. *Metallurgie*, Vol. 7, 1910, pp. 456-458.

§ The Effect of Annealing, etc.; *Technology Quarterly*, Vol. 13, 1900, pp. 295-313.

Stead reported in one case a grain growth to 4,000,000 times the initial normal size, by a heating extending through several years; but it is not very clear whether, during this heating, the steel was in the alpha or beta ferrite state, or in that of austenite.*

Charpy† found that the grain growth of low-carbon steel in this range of temperature was greatly accelerated by previous overstrain, for instance by cold deformation. Two like pieces of very-low-carbon and moderately phosphoric steel, one wire-drawn and the other not, were held in this range of rapid growth between 650° and 800°. After this the grains of the wire-drawn piece were about 10 times as wide as those of the other, implying a difference in volume of 1000 to 1. Steel which when wire-drawn could be bent considerably without cracking, after later heating at about 650° became so brittle that it broke when dropped on the ground. This is a striking example under Ewing and Rosenhain's law that overstrain hastens grain-growth. (Principle 1, Paragraph 15, page 277.) It is to provide against this deterioration that the last paragraph of the "Practice Recommended for Annealing," page 86 of this volume, is given.

This grain growth of ferrite at temperatures below A2 is in accord with Principle 5, Paragraph 21, page 289.

50. *Provisional Proposition 2. Behavior of Beta Ferrite.*— It is probable that the grains of the beta ferrite of which very-low-carbon steel chiefly consists when in Region 6 between A2 and A3, grow like that of other metals, by coalescence. But this growth is evidently very much slower than that of alpha ferrite in Region 7. It is further probable that when coarse grains of alpha ferrite of such steel are heated up into Region 6 and thus become beta ferrite, though now pseudomorphous, they yet persist for a time. But they are so unstable that they gradually break up into finer grains fashioned after the true beta structure.

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* *Journal, Society of Chemical Industry*, Vol. 22, 1903, p. 343.

† *Revue de Metallurgie, Memoires*, Vol. 7, 1910, p. 655; also *Metallurgical and Chemical Engineering*, Vol. 9, 1911, p. 80.

It is further probable that these fine beta grains, like those initially fine, gradually grow by coalescence.

The conditions may be summed up in the Provisional Proposition 2, as follows:

Between 770° and 895° (A2 and A3, Region 6) very-low-carbon steel, consisting as it then does almost solely of beta ferrite, (A) if initially fine-grained coarsens though only very slowly; (B) if initially coarse-grained refines slowly, (C) to coarsen again on longer exposure.

This proposition is almost a re-statement of Principle 6, Paragraph 22, page 290, which see. Taking up the evidence, whereas Stead found rapid grain growth to an enormous size, often with extreme embrittling, on long heating to temperatures between 650° and 750°, that is, in Regions 8A and 7, Heyn[*] found no such embrittling effects on heating his 0.07-per cent. carbon steel even for 14 days to temperatures between 700° and 890°. As this embrittling seems to be closely connected with the coarsening, from the fact that Heyn's steel did not embrittle we naturally infer that it probably did not coarsen. On the other hand, he found that low-carbon steel which had been embrittled by coarsening it when in the austenite state, lost its brittleness extremely slowly "at temperatures between 700° and 850°." Even 5 hours did not suffice for this; but a heating of several days did.

At the first glance Heyn's results seem to disagree with Stead's. I have attempted to reconcile them by trying to rectify and limit their temperature intervals, assuming that Stead's region of rapid coarsening is Region 7 between A1 and A2, and that the reason why Heyn's steel did not coarsen as Stead's did is that Heyn's temperatures lay so much of the time in Region 6 between A2 and A3, as to efface whatever coarsening he caused in Region 7. If we make this very simple and reasonable assumption, that Stead refers to Region 7 and Heyn to Region 6, and that their disagreement is due to their failure to discriminate between these two regions, the discord resolves itself, without even having to call on the difference between the special brittleness test of Heyn and the grain coarsening to which Stead chiefly addressed himself.

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[*] *Journal, Iron and Steel Institute*, Vol. 2, 1902, p. 108.

An examination of Stead's paper supports this explanation. Thus he repeatedly gives the upper limit of the range of rapid coarsening as 730° or 750°, but nowhere, so far as I find, at any higher temperature.* The last passage quoted from him in the footnote to Proposition 1, Paragraph 49, page 324, here and in general supports this present proposition, save that he did not note the slow refining in Region 6 between A2 and A3, of previously coarsened steel to which Heyn's results point, nor the gradual coarsening of initially fine-grained steel in Region 6, to which Joisten's results point.

Joisten's evidence tends to support this proposition. Thus, whereas the ferrite grain of his 0.07-per cent. carbon steel grew with great rapidity, but most irregularly, at 600° and 700°,—that is, at temperatures within the alpha ferrite range, below A1, Region 8A,—yet at 850°, which is within the beta ferrite range, Region 6, the growth was extremely slow, so that the grain was but little larger after a 10- or 20-hour sojourn at 850° than after a 5-hour† one. The apparently more rapid growth per hour during the short than during the longer exposures to 850° is readily explained by the rapid growth which would occur in each case during the passage up through Region 7 between A1 and A2, in order to reach 850°. In other words, whether the sojourn at 850° was short or long, a rapid growth would occur in reaching 850°, followed by an extremely slow one at 850°; and the inevitable effect of this would be to give the impression of a progressive slackening of the growth at 850° unless the rapid growth in reaching 850° was recognized.

The contrast between the persistence of the previous alpha grains on heating up past A2 into Region 6, and the shattering of the grains existing in Region 6 on heating past Ac3 into Region 4, is shown by these words of Osmond and Cartaud, based on their examination of isolated crystals: "a crystal of alpha iron heated in the range of beta iron, and cooled to the ordinary temperature, becomes again the same crystal of alpha iron—the system persists beyond transformation A2.

"This is not the case with the passage of the point A3, provided it has been sufficiently exceeded, and for a sufficiently long time. It is quite possible, if the heating and the cooling down are both done rapidly, to heat a crystal of alpha iron up to 900° without destroying it; but if it is maintained at this temperature, the crystal resolves itself into little grains, with the formation of elongated lamellæ, which appear to be twin crystals."*

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* *Journal, Iron and Steel Institute*, Vol. 1, 1898, pp. 158, 159, 160, 161, and 166.

† Or perhaps 2.5 hours. There is an apparent contradiction in the original. *Metallurgie*, Vol. 7, 1910, p. 457.

The results of Osmond and Cartaud, and those of Stead and Heyn, as I here interpret them, certainly show that if there is any coarsening of initially fine-grained beta ferrite in Region 6 between A2 and A3, it is very slow. Heyn's loss of brittleness on very prolonged heating here goes to show that initial coarseness is here lost slowly, and Joisten's results indicate that an initially fine grain here coarsens slowly. If this is true it is hard to resist the inference that a fine grain caused by the breaking up of a coarse one inherited from the previous alpha state, also would coarsen slowly. Thus, while the evidence makes it clear that whatever coarsening occurs here is very slow, and that there is no rapid break up here of previous coarse grain as there is on heating up past Ac3 into Region 4, yet both the slow coarsening which I have postulated under A and C, and the refining under B, are based rather on inference than on direct evidence. Hence the proposition in its present form is only tentative.

51. *Proposition 3. The grain refining of very-low-carbon steel and carbonless metallic iron occurs quickly at Ac3.*—This is practically a re-statement of Principle 8, Paragraph 25, page 294.

Evidence Supporting this Proposition.—It is certain that this grain refining takes place abruptly at a certain high temperature, and that this temperature is in the neighborhood of Ac3, allowance being made for lag. It is true that the exact identity of Ac3 and the grain-refining temperature of such metal has not been demonstrated rigorously. But the identity is so probable antecedently, and is so fully in accord with the evidence, that it may reasonably be adopted as a working hypothesis.

The normal or equilibrium temperature of Ac3 for carbonless metallic iron is probably very near 895°. By the equilibrium temperature I mean that at which the underlying change from beta to gamma iron is due to occur, and would occur were there no lag. In the slow rise of temperature of heat-treatment operations, both industrial and laboratory, the lag is less, and the change therefore occurs closer to the equilibrium temperature, than in the relatively rapid heatings. Therefore we are here concerned with the equilibrium temperature of A3 as inferred from the data at hand taken as a whole, rather than with the temperature of Ac3 as observed directly by means of heating curves. My reasons for taking the equilibrium temperature of A3 provisionally at 895° will be given elsewhere, because they lie beyond the natural scope of this paper.

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* Osmond and Cartaud, *Journal, Iron and Steel Institute*, Vol. 3, 1906, p. 451.

That the grain-refining temperature rises as the carbon content falls is so familiar that it may here be assumed.

That this temperature for very-low-carbon steel is close to 895°, and hence that for pure carbonless iron it is close to or a very little above 895°, is shown by abundant tests of Stead's,* and less positively by those of Heyn.† The fact that Fay and Badlam's steel refined between 850° and 890° is in full accord with this, because both the 0.07 per cent. of carbon and the 0.32 per cent. of manganese in their steel would lower the equilibrium temperature of A3.‡

For instance, Stead found that the extreme coarse grain induced in steel of 0.035 per cent. of carbon by a 48-hour annealing was completely removed by heating for one minute to 900°. Very many other observations of his showed this grain refining at 900°, and Heyn found that about half an hour at 900° sufficed to break up coarse grain.

It is true that such assertions as that there is a "breaking up or refining of the structure between 850° and 950°" are occasionally found, and that on their face they suggest that this grain refining may occur below Ac3. In particular Stead gives the grain-refining temperature at "870° or thereabouts."§

But, first, an assertion that this refining occurs somewhere "between 850° and 950°" does not imply that it can occur

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* *Journal, Iron and Steel Institute*, Vol. 2, 1898, pp. 147, 152. *Idem*, Vol. 1, 1898, pp. 173, 181, 183, 185.

† *Idem*, Vol. 2, 1902, pp. 91, 108.

‡ *Technology Quarterly*, Vol. 13, 1900, pp. 307, 311.

§ *Journal, Iron and Steel Institute*, Vol. 1, 1898, p. 166.

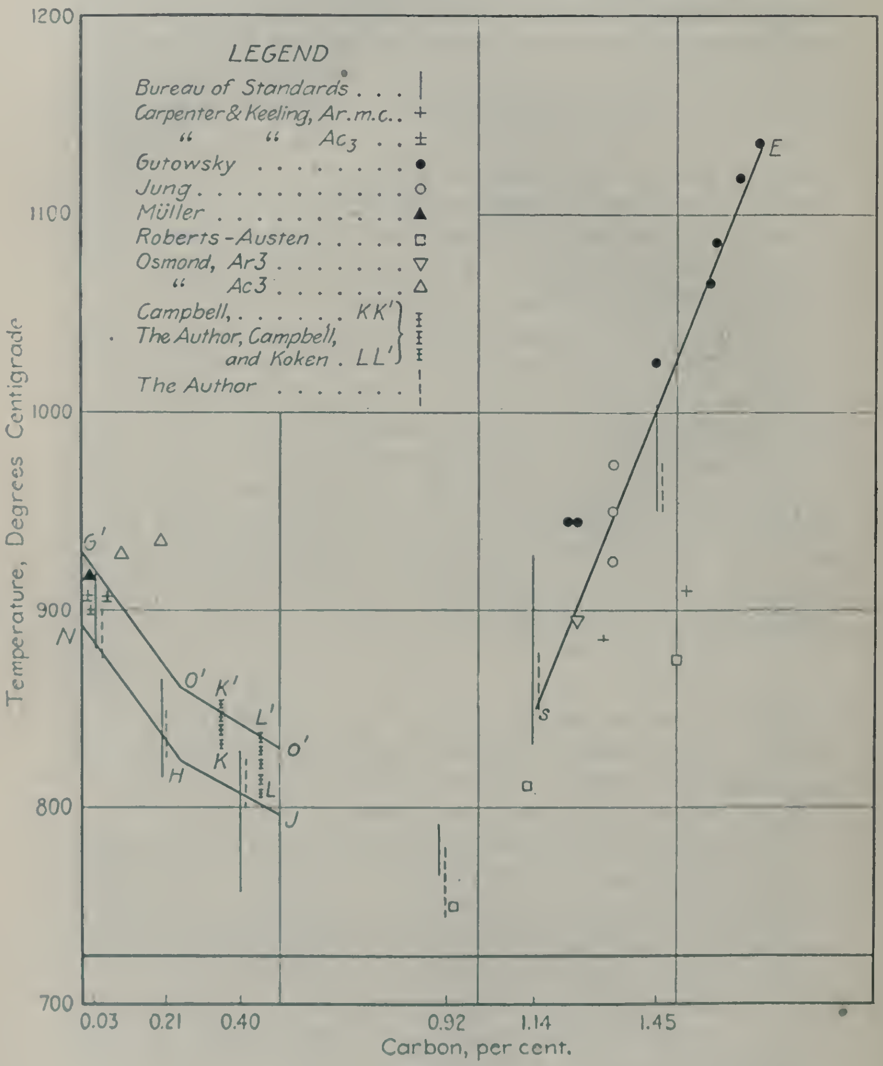
LEGEND
Bureau of Standards . . . |
Carpenter & Keeling, Ar.m.c. . +
" " Ac3 . . ±
Gutowsky . . . . ●
Jung . . . . ○
Müller . . . . ▲
Roberts-Austen . . . . . □
Osmond, Ar3 . . . . ▽
" Ac3 . . . . △
Campbell, ... KK'
The Author, Campbell, and Koken . LL'
The Author . . . . ¦
1200
1100
1000
900
800
700
Temperature, Degrees Centigrade
0.03
0.21
0.40
0.92
1.14
1.45
Carbon, per cent.
G'
N
O'
K'
K
L'
L
O'
H
J
S
E

FIG. 5.

NOTE TO FIG. 5.

Dr. G. K. Burgess, of the United States Bureau of Standards, Washington. Thermal determinations on the author's steels. The upper end of each solid line represents the end of Ac3, the lower end represents the beginning of Ar3, except that the lower end of the 0.92-per cent. carbon solid line represents the maximum and end of Ac1. Ar3 for this steel was not detected.

Carpenter and Keeling (*Journal, Iron and Steel Institute*, Vol. 1, 1904, p. 232). Temperature of separation of massive cementite, Ar.m.c., and of Ac3, determined thermally.

Gutowsky (*Metallurgie*, Vol. 6, 1909, p. 739). The total carbon taken into combination by pure iron, at various temperatures, or retained in combination by pure iron, at various temperatures, or retained in combination there by pure iron initially richer in carbon; hence apparently the equilibrium content.

Jung (*Internationale Zeitschrift Metallographie*, Vol. 1, 1911, p. 214). Temperatures of completion of re-absorption and beginning of generation of cementite, on heating and cooling respectively, determined microscopically.

Müller (*Metallurgie*, Vol. 6, 1909, p. 158). Ac3 of iron, determined thermally in 29 specimens of electrolytic iron.

Roberts-Austen (*Fifth Report to Alloys Research Committee*, 1899, Plate 3). Ar3 determined thermally.

Osmond (*Transformations du Fer et du Carbone*, 1888, pp. 27, 76, and 81). Ac3 and Ar3 determined thermally. Osmond thought that the Ar3 retardation of the 1.25-per cent. carbon steel shown on this diagram was accidental, but as it occurs at the same temperature in the records of both of his coolings of this steel and agrees so very closely with the most trustworthy later determinations of the line *sE* in cooling, we may assume that "the dice were really loaded." Because of the then wrong datum points I follow him in adding 35° to his determinations at about 850° (*Journal, Iron and Steel Institute*, Vol. 3, 1906, p. 451).

Campbell (*KK'*) (*Proceedings*, Vol. IX, 1909. p. 370). Range in which re-absorption of ferrite network becomes complete in heating up, determined microscopically.

The Author, Campbell, and Koken (*LL'*) (*Proceedings*, Vol. VIII, 1908, p. 185). Range corresponding to that described under Campbell (*KK'*).

The Author. Range corresponding to that described under Campbell (*KK'*). A series of specimens of each steel, after coarsening, was heated to a series of temperatures in about this range, quenched in water, and examined microscopically. The lower end of each dotted line represents the highest temperature after heating to which the steel still contained visible unabsorbed free ferrite or cementite. The upper end represents the lowest temperature after heating to which the steel lacked visible free ferrite or cementite.

TABLE I.—COMPARISON BETWEEN MICROSCOPIC AND THERMAL DETERMINATIONS OF Ac_3 IN THE AUTHOR'S STEELS.

Presented by	Number of Steel	Composition, per cent.					Analyzed* by	Thermal. Bureau of Standards			Microscopic. The completion of reabsorption of ferrite network occurred			The end of Ac3 determined microscopically lay below the end of Ac3 determined thermally by —° C.
		C	Si	Mn	P	S		End of Ac3, ° C.	Beginning of Ar3, ° C.	Range, ° C.	below —° C.	but above —° C.	Range, ° C.	
LOW-MANGANESE STEELS.														
Carpenter Steel Company....	6	0.027	....	0.259	0.005	0.024	B. and M.	920	882	38	900	875	25	20
	4	0.214	....	0.053	0.013	0.010	B. and M.	865	814	51	859	825	25	15
	1	0.40	0.103	0.161	0.014	0.012	B.	828	754	74	825	800	25	3
Halcomb Steel Company.....	7	0.92	0.14	0.14	0.009	0.011	M.	790	†	.....	775	740	35	15
	8	1.143	0.124	0.239	0.016	0.009	B.	930(?)	830(?)	100	875	850	25	55
	9	1.45	0.14	0.16	0.009	0.006	B. and M.	1,005(?)	950(?)	55	975	950	25	30
HIGH-MANGANESE STEELS.														
Carpenter Steel Company.....	3	0.16	.....	1.16	0.020	0.018	M.	878	780(?)	98	875	850	25	3
	2	0.46	0.095	1.215	0.017	0.015	B.	820(?)	750(?)	70	890	775	25	20

* In this column, B. denotes Messrs. Booth, Garrett and Blair, and M. the makers of the steel.

† Maximum of Ac_1 is 768°. Its end is not given. Ar_3 was not detected.

everywhere within those limits; second, such assertions may well have been meant to include steel with enough carbon and manganese to lower the position of A3 materially, though that just quoted from Stead refers immediately to carbonless iron; third, the older determinations of such high temperatures were subject to a considerable error, because of wrong datum points, and, except in the most skilful hands, from the neglect of certain precautions in detail; and fourth, there is probably a slow breaking up of previous coarse grain in Region 6, below Ac3, though this is in striking contrast with the rapid breaking up or refining at 895°. In interpreting such quotations as I have just made, we may well note how their vagueness contrasts with Stead's more precise words: "When pure iron, made granular by long heating at a dull red heat, is heated between 750° and 870°, as a rule the structure is not altered to any material extent, but as soon as the temperature rises to about 900°, the granules again become small."*

The words quoted from Osmond and Cartaud under Proposition 2 show that, rapid as the shattering of the grains is on rising past Ac3, it is not instantaneous.

52. *Proposition 4. The grain refining of higher-carbon steel occurs at Ac3; but it takes an appreciable and sometimes a very considerable length of time, unless the carbon content is very near to the pearlite or eutectoid ratio of 0.90 per cent.*

This proposition is in accordance with, and indeed a necessary consequence of Principle 19, Paragraph 42, page 313.

That the temperature needed to complete this re-absorption of ferrite is near Ac3, is indicated by my own unpublished experiments. I coarsened a series of steels of varying carbon content, by overheating them to 1350° for one hour. On reheating a series of specimens of each steel so coarsened to various temperatures for one hour, then quenching, and then seeking for traces of residual unabsorbed coarse network, I found that the grain-refining temperature, as indicated by the disappearance of the ferrite or cementite network, agreed fairly with the preliminary determinations of the end of Ac3 in these same steels by Dr. C. K. Burgess of the United States Bureau of Standards. This agreement is shown in Fig. 5 and Table I.

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* *Journal, Iron and Steel Institute*, Vol. 1, 1898, p. 183.

In the hypo-eutectoid steels the ferrite network disappeared slightly below the end of Ac3 as determined thermally, and in the hyper-eutectoid steels the cementite network disappeared at a temperature considerably below the thermal end of Ac3, if I may use this term provisionally to apply to the line *SE*, Fig. 2 (*sE*, Fig. 5), which is to free cementite what *GOS* (Fig. 2) or Ac3 is to free ferrite.* This agreement is not such as to demonstrate the identity of the end of Ac3 and the grain-refining temperature, but it is in accord with the belief in that identity. It is but natural that the microscopic results should be lower than the thermal ones, first because my very long sojourns at these temperatures should have greatly lessened the lag; and second, because the ferrite or cementite might cease to be recognizable under the microscope at a time when the re-absorption was still going on at a rate which would retard the rise of temperature measurably.

TABLE II.—INDUSTRIAL ANNEALING TEMPERATURES FOR THE RE-ABSORPTION OF THE COARSE FERRITE NETWORK (GRAIN-REFINING).†

Range of Carbon Content.	*Range of Annealing Temperature.*
Less than 0.12 per cent.	875 to 925° C. (1607–1697° F.)
0.12 to 0.29 " "	840 " 870 " (1544–1598 ")
0.30 " 0.49 " "	815 " 840 " (1499–1544 ")
0.50 " 1.00 " "	790 " 815 " (1454–1499 ")

This proposition is further supported by the experience in industrial grain refinings, in which (1) the temperatures actually found necessary as indicated in Table II correspond approximately to the probable equilibrium temperatures of A3; and (2) the length of time needed is so much more than can be explained by the mere lag of the heating of the interior of the objects as to indicate that the grain refining proper takes "an appreciable and often a very considerable time."

That the grain refining of hyper-eutectoid steels takes place on reaching Ac3 is further indicated by the experiments of M. Osmond, by those of F. Göransson made under my directions, and by those of Jung. On heating a steel of 1.57 per cent.

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* The lines *GOS* and *SE* together may be called the liquidoid and *PSP'* the solidoid.

† From "Practice Recommended for Annealing Miscellaneous Rolled and Forged Carbon-Steel Objects," this volume, p. 86.

carbon to 1000° and quenching it, M. Osmond* found that the initial cementite, which we may take with some confidence to be the equivalent for our present purpose of the cell-wall cementite, had not been fully re-absorbed during the heating; but when the heating and quenching temperature was raised to 1050°, that is, nearly to the line Ac3, only traces of that cementite remained unabsorbed.

Göransson† found that a 1.20-per cent. carbon steel, coarsened by over-heating to above 1270°, lost its coarse grain and simultaneously had its cementite completely re-absorbed on reheating to 887°, which is close to this line.

Jung‡ found, by microscopic examination of steel of 1.33 per cent. carbon quenched from divers temperatures in and near the transformation range, that the absorption of the free cementite in heating up became complete between 925° and 975°, and that its generation in cooling began between 975° and 950°.

He further found that the re-absorption of free cementite "takes an appreciable time." On holding this same steel at 975° for varying lengths of time, he found that between 3 and 4½ minutes were needed to complete the re-absorption of its cementite to such a degree that it ceased to be visible under the microscope.

So too, Campbell, Koken, and I found that a de-ingotized steel casting of 0.43 per cent. carbon, and a rolled steel of 0.38 per cent. carbon, after coarsening by heating to 1377°, had their coarse ferrite network effaced by heating to 840°, but not by heating to 804°§, and Professor William Campbell found that even an un- de-ingotized casting of 0.35 per cent. carbon, if free from a network of manganese sulphide and its equivalents, lost its coarse ferrite cell-walls on heating to 855° but not fully on heating to 830°.‖ In all these cases heating to temperatures apparently above Ac3 removes the coarse ferrite network, though heating to below that point does not.

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[*] Osmond and Stead, *Microscopic Analysis of Metals*, London, 1904, p. 150; *Etude des Alliages*, Paris, 1901, p. 318.

[†] *Transactions, American Institute of Mining Engineers*, Vol. 33, 1903, p. 113; *The Metallographist*, Vol. 5, 1902, p. 223.

[‡] *Internationale Zeitschrift für Metallographie*, Vol. 1, 1911, pp. 214, 215.

[§] *Proceedings*, Vol. VIII, 1908, pp. 185–189.

[‖] *Idem*, Vol. IX, 1909, pp. 370–377.

In the case of un- de-ingotized steel castings, that is, those which retain the initial network structure formed during solidification, Professor Campbell showed that if manganese sulphide or its equivalent is present in the form of a coarse network, the temperature must be raised much higher in order to break up that network before the coarse ferrite network can be permanently removed.*

It is true that Brinell† says, in his second law, "very coarse crystalline steel, hardened or unhardened, loses its texture (fracture) completely, as soon as the heating is pushed fully to the point which suffices for the change of the carbon from cement to hardening." To explain, the change from cement to hardening carbon is one feature of the change from pearlite to austenite, which, in such steel as he then wrote of, occurs wholly at Ac1.

Here he contradicts the present proposition. But I find that he is in error; for, however marked the fracture-change is which occurs at Ac1, it is not till Ac3 is passed that the refining proper becomes complete, that is, the change from a coarse to a fine fracture. My procedure is described in Appendix II, page 382.

We know positively that the temperature at which the carbon condition changes from cement to hardening in hypoeutectoid steel is A1, and that this temperature is nearly constant, and hence independent of the carbon content. Further, it has been universally recognized from the days of Tschernoff‡ that the grain-refining temperature rises rapidly as the carbon content falls, as shown by the line *GOS* in Fig. 2. Neither of these things is open to question. Indeed they form part of our everyday industrial processes. The former temperature, because it is nearly constant, cannot be identical, as Brinell implies, with the latter which varies widely. That Brinell's extraordinary powers of observation should have been deceived in this single detail still leaves his classical work a wonderful example of what a trained mind and eye can do, even under the conditions of those pioneer days long before the coming of our present instruments of precision.

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* *Proceedings*, Vol. IX, 1909, pp. 370–377.

† *Stahl und Eisen*, Vol. 5, 1885, p. 620.

‡ *Remarks on the Manufacture of Steel*, translation by W. Anderson, 1880, pp. 6, 7. In particular Tschernoff pointed out that in low-carbon steel these two points, the grain refining temperature and the temperature Ac1 at which the hardening power is acquired, draw apart, till in the lowest-carbon steel the grain-refining temperature "corresponds to white heat."

53. *Proposition 5. The coarsening caused by long or high heating in Region 4. Though holding at Ac3 for an appropriate length of time breaks up pre-existing coarse grain and coarse network, yet a prolonged sojourn at any higher temperature leads to a re-coarsening of the ferrite grains and of the network structure, a re-coarsening which increases with the length of sojourn and with the temperature.* This re-coarsening of the network represents the coarsening of the austenite grains. The re-coarsening of the ferrite grains I refer to the perfecting of the austenite crystalline structure, which enables it to mass the ferrite locally the more thoroughly, in cooling through the transformation range.

As pointed out in the third paragraph of Principle 5, Paragraph 21, page 289, this coarsening is extremely slow in the case of ultra-low-carbon steel. (Paragraph 58, page 348.)

This proposition is in harmony with Principles 4, 5, 8, 9, 10, and 11, Paragraphs 20, 21, 25, 26, 28, and 33, pages 287, 289, 294, 295, 297, and 305, and could indeed be deduced from them.

It is in accordance with Principles 4 and 5, Paragraphs 20 and 21, pages 287, 289, that the austenite grains should coarsen progressively through coalescence while in Region 4, and that this coarsening should increase both with the length of sojourn and with the mobility of the metal, that is, with the temperature.

It is in accord with Principles 8, 9, and 11, Paragraphs 25, 26 and 33, pages 294, 295, and 305, that this grain coarseness of the austenite should be followed by coarseness of the ferrite grains of ultra-low-carbon steel when that austenite changes into ferrite in cooling.

It is in accordance with Principle 10, Paragraph 28, page 297, that *high and long heating*, by perfecting the crystalline organization of the austenite, enables it the more thoroughly to expel into the joints between its grains, and into its cleavages the ferrite which it generates in cooling through the transformation range, thereby massing the ferrite there, and thereby in fine leading to the formation there of larger accumulations of coagulated ferrite, which later break up into correspondingly *larger ferrite grains.*

It is in accordance with Principle 9, Paragraph 26, page 295, that in the case both of hypo-eutectoid steels with any considerable carbon content, and of hyper-eutectoid ones, the coarse grains of austenite formed by coalescence in a long sojourn in Region 4 above the transformation range should give rise to *coarse network structure* in cooling through that range by expelling into the joints between themselves, or into their cleavages, the ferrite or cementite which they generate in cooling through that range.

54. *Experimental Evidence.—A steel containing at least a moderate quantity of carbon,* as distinguished from nearly carbonless steel. Though the general law has long been recognized, that so-called grain size increases with the temperature reached and with the length of exposure, yet in general the older writings leave us in doubt as to whether this grain size refers to the size of the grains of ferrite or the size of the walled cells. Osmond's micrographs show that the ferrite grain size of steel of 0.14 per cent. carbon, heated and cooled slowly, increases with the temperature reached, to 750°, 1015°, and 1330°.[*]

Fay and Badlam, besides recording the grain refining of their 0.07-per cent. carbon steel, which occurred between 850° and 889°, recorded a ferrite grain growth, slow up to 1125° and then faster with further rise of temperature up to 1247.[†]

R. G. Morse,[‡] working under my directions, found that the grains of ferrite in slowly cooled steel of 0.343 per cent. carbon increased in size both with the temperature to which the steel had been heated in Region 4, above the transformation range, and with the length of exposure to those temperatures.

55. *Turning now to network size* (size of the walled cells) an examination of some of the older micrographs shows that it, as distinguished from ferrite grain size, is increased by high heating. This is shown directly by M. Osmond's and by Professor William Campbell's micrographs, and indirectly by

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[*] *Bulletin Société d'Encouragement*, 1895, pp. 26 and 27, and Figs. 157, 159 and 160 of reprint; *Contribution à l'Étude des Alliages*, Figs. 17, 18, and 19, and p. 305; and *Microscopic Analysis of Metals*, pp. 117-124.

[†] *Technology Quarterly*, Vol. 13, 1900, pp. 311, 312.

[‡] *Transactions, American Institute of Mining Engineers*, Vol. 29, 1899, pp. 731, 745.

Professor Sauveur's. Thus, after heating and slow cooling, the network of M. Osmond's steel* of 0.45 per cent. carbon was very much coarser when the temperature to which the heating was carried was 1390° than when it was 1015° and also much coarser when it was 1015° than when it was 750°. So, too, the network of Campbell's steel of 1.61-per cent. carbon was much coarser after slow cooling from 1200° than when cooled slowly from 1070°.†

Here Osmond's earlier results,‡ shown graphically in Fig. 5*a*, are of the first moment. In a series of specimens cut from each of three steels of 0.14, 0.45, and 1.24 per cent. carbon respectively, heated to a series of high temperatures and cooled slowly, the network size (?) increased continuously with the temperature reached, from a temperature which he takes to be about Ac2, and this increase accelerated at two points, one slightly above

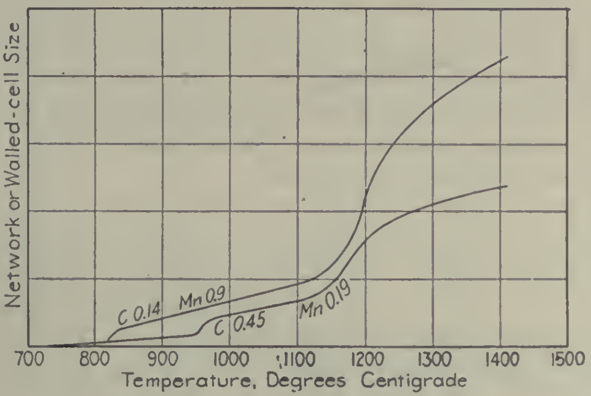

FIG. 5*a*.—Increase of Network Size (?) with Temperature Reached. (Osmond, 1888.)

NOTE.—Heated (in Leclerq and Forquignon Furnace) in 25 to 30 minutes to peak temperature, then at once cooled in furnace.

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* *Bulletin Société d'Encouragement*, May, 1895, p. 28 and Figs. 179 and 190 of reprint; *Contribution à l'Etude des Alliages*, Figs. 27 and 28, and p. 308; and *Microscopic Analysis of Metals*, pp. 127–8.

† *Proceedings*, Vol. VI, 1906, Figs. 43, 44, 47, 48, Plate XIII, following p. 238.

‡ *Etudes Metallurgiques*, Dunod, Paris, 1888, from *Annales des Mines*, July–August, 1888. It was not till this present paper was in type that I found these important data in this work of M. Osmond's, which is too little known.

Ac3 and the other between 1115° and 1230°, that is, in the neighborhood of Ball's point. Between 1330° and 1390° but little further growth occurred. Though he tried varying the length of exposure, he does not report the effect; but we may guess it from his giving as his opinion that grain growth (he calls it "transformation") occurs during heating up, cooling down, and holding at high temperatures, and that it is a function of the maximum temperature reached and *probably* of the length of exposure to it (page 78 of reprint).

As he gives no photographs we cannot be confident that the growth which he refers to is strictly that of a network system similar to what is discussed in this paper, though his language certainly suggests this. But in view of the fact that his specimens were cooled in the furnace it is surprising that a network such as my specimens show, or one which would correspond to his description, should have persisted.

Professor Sauveur's micrographs of rail structure show that in the section of a given rail, the network is the coarser the higher the temperature at which the rolling is finished.* This is in harmony with this present law, as is seen on the least reflection. When a piece like a rail, which is highly heated, is rolled with such heavy reduction as to distort or inequiaxe the austenite grains greatly, the distorted and hence unstable grains immediately shatter, and their remains immediately begin growing again by coalescence. This is repeated as often as the piece is greatly reduced by the rolling. Each of the grains of austenite formed by coalescence after the last of these reductions, in cooling through the transformation range, gives birth to a walled cell by ejecting to its outside the ferrite which it generates. Hence it is the size which those austenite grains reach after this last effective reduction that determines the network size of the cold steel, and the only opportunity which they have to grow by coalescence is in cooling from that last effective reduction down into the transformation range.

Finishing Temperature.—Hence as regards the opportunity for network growth, the finishing temperature of rolled and forged objects is the equivalent of the peak or highest tempera-

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* *Transactions, American Institute of Mining Engineers*, Vol. 22, 1893, pp. 546–557, and especially Plates IV and V.

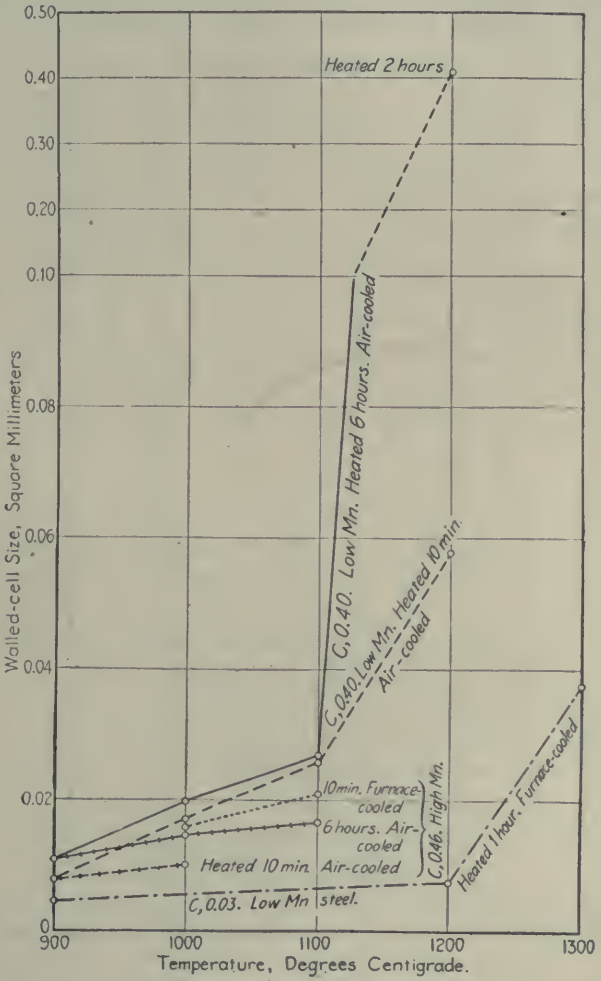

FIG. 6.—Increase of Network or Walled-Cell Size with Length and Height of Heating. Hypo-Eutectoid Steel.

NOTE.—The low-manganese steel contained 0.398 carbon and 0.161 manganese; the high-manganese steel, 0.458 carbon and 1.215 manganese; and the 0.03-per cent. carbon steel, 0.029 carbon and 0.259 manganese.

ture reached by objects not later rolled or forged. And the fact that the network size of the former increases with the effective* finishing temperature tends to support the law that the network size of the latter increases with the temperature reached.

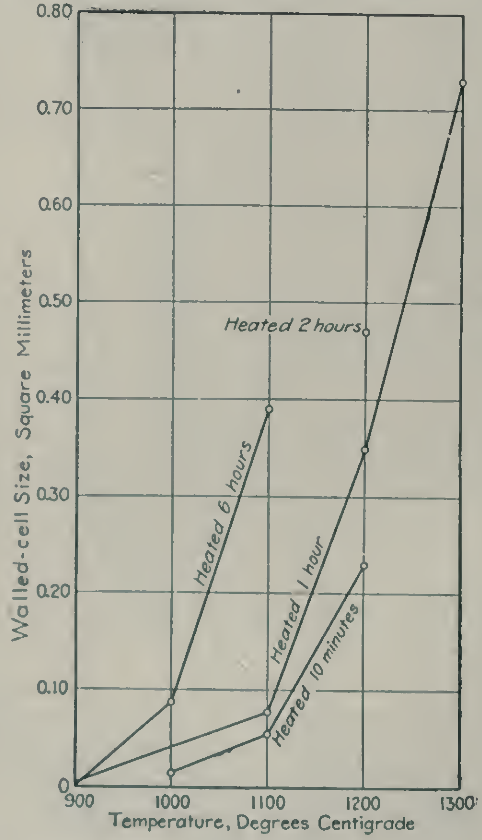

FIG. 7.—Increase of Network or Walled-Cell Size with Length and Height of Heating. Hyper-Eutectoid Steel.

NOTE.—The steel contained 1.143 carbon and 0.239 manganese.

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* I say effective finishing temperature, because the opportunity for austenite grain growth and through it for network growth, clearly dates from the last reduction important enough to shatter the pre-existing austenite grains by setting up a degree of inequiaxing sufficient to cause that shattering.

56. *New Results.*—Finding the support of this proposition so fragmentary and indirect as regards the influence of time on network size, I undertook the experiments which led me to write this paper. The results show that the network size, like the grain size, increases with the temperature reached, between the limits of 900° and 1300°, and, at least in the case of hyper-eutectoid steel, with the length of exposure to those temperatures. The experimental work is described in an appendix, and the progressive growth is readily seen by means of Figs. 6 and 7, Table III, and the micrographs of Plates II, III, and IV.

In Figs. 6 and 7, though every rise of temperature from 900° upwards hastens the network coarsening, this acceleration is the greater the higher the temperature. Thus in general there is a sharp upward break at 1100°. In the case of the hyper-eutectoid (1.14-per cent. carbon) steel, heated for 6 hours, this break occurs at 1000°.

There are suggestions to the effect that the rate of growth accelerates with the size of the network, that is, of the austenite grains, somewhat as the growth of a rolling snowball accelerates, with the further suggestion that the rate of growth increases rather with the mass of the coalescing grains than with the extent of surface exposure between grains. The fact that the upward break in the 6-hour curve of the 1.14-per cent. carbon steel occurs at 1000°, whereas in the 10-minute heating it occurs at 1100°, is one of these suggestions; and another is that the effect of a first five-fold lengthening of the exposure of this steel, from 10 minutes to 1 hour, is much less than that of a second five-fold increase, from 1 to 6 hours. But no such relation exists among the curves of the hypo-eutectoid steels.*

Professor Sauveur has kindly verified these present results in part, by measuring independently the network size of two of these present specimens of this hyper-eutectoid steel, with results agreeing fairly with those which I give here. Yet the network size of this same steel, given in Fig. 7, is very much greater than that which he and I gave earlier for steel of roughly like composition.* After careful examination of the conditions I am at a loss to explain the difference.

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* That there should be great irregularities in network size is not strange in view of the great irregularities in ferrite grain size which are reported. Thus Stead found that the diameter of the "granules" or ferrite grains in one-half of one and the same sample, apparently after identical treatment, was more than eight times as large as in the other half. (*Journal, Iron and Steel Institute*, Vol. 1, 1898, p. 159.) An even greater difference in grain size is seen between two parts of one of Joisten's micrographs of a steel of 0.07 per cent. carbon after 20 hours' exposure to 700° (*Metallurgie*, Vol. 7, 1910, Fig. 491, facing p. 458), and in my Fig. G, of Row 10, Plate IV, of a 0.40-per cent. carbon steel heated at 900° for 6 hours, under potassium chloride and cooled in air, so that the various layers, though heated alike, were cooled at different rates. See Paragraph 62, p. 355.

Influence of Surroundings on Network Growth.—In order to prevent decarburizing, the later heatings were made under fused barium or potassium chloride, except the 1-minute and 3-minute 1100° heatings of Steel I, made under molten copper. But in many of the earlier heatings the less thorough precaution was used, of surrounding the specimen with an atmosphere rich in carbonic oxide within a closed vessel, and confining the microscopic examination to the inner parts of the specimen, where little if any decarburization was to be expected. In studying the effect of time on the network size of Steel I of 0.40 per cent. carbon, I found great anomalies, reminding me that Professor William Campbell found no grain growth in steel of 0.43 per cent. carbon when heated to 1180°.† On further examination I found that the anomalies were most striking after those heatings made without a molten cover. Therefore these unprotected heatings of Steel I were repeated under fused chloride, with results which are fairly concordant as regards the influence of temperature.

I have set the results reached with this steel without molten cover in a separate part of Table III, and in making the curves for this steel in Fig. 6, I have disregarded all of them except that of the 1200° 6-hour heating. This result I have retained because of the difficulty of making such a heating under fused chlorides, which at 1200° are very destructive to the enclosing crucible and furnace. The cause of the discrepancies is considered in Paragraph 57.

57. *Results Less Concordant With Medium-Carbon Steel.*—In general my results are much less concordant with these medium-carbon steels than with the hyper-eutectoid steel. Thus, while their curves in Fig. 6 are in general of the same family as those of the hyper-eutectoid steel of Fig. 7, the slight

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* *Engineering and Mining Journal*, Vol. 60, 1895, p. 537. This steel contained 1.10 per cent. carbon, 0.02 per cent. silicon, and 0.41 per cent. manganese. See also the Author, *Iron, Steel and other Alloys*, p. 246.

† *Proceedings*, Vol. IX, 1909, p. 370.

difference between the 10-minute and 6-hour curves for the 0.40-per cent. carbon steel, and the fact that the 10-minute furnace-cooled curve lies above the 6-hour air-cooled curve for the 0.46-per cent. carbon steel, are certainly anomalous.

So with the network growth for the four different exposures of the 0.40-per cent. carbon steel to 1100°, in Table IV. Here, though the network size increases with the length of exposure from 1 to 10 minutes, the increase is jerky and there is but little further growth in the 6-hour exposure. Yet all four of these exposures were under molten coverings.

At first sight one is inclined to refer the discrepancies to the influence of decarburizing in the longer heatings, having in mind Proposition 6, Paragraph 59, page 351, that the network growth increases with the carbon content. The two striking discrepancies in Fig. 6 are that the network growth is insufficient in the 6-hour heatings, and of course it is precisely in these 6-hour heatings that decarburization would occur. But this explanation is not in itself enough. For instance, the growth in the first 10 minutes of the 6-hour curve of the 0.40-per cent. carbon steel should be quite as great as in the 10 minutes of the 10-minute curve; but after these 10 minutes the 6-hour specimens had a further heating of 5 hours and 50 minutes; yet this very great prolongation of the exposure, even in these specimens covered with fused salt, caused no important further network growth. But it is inconceivable that, immediately after the first 10 minutes, decarburization should have caused so complete an arrest of the grain growth of the austenite that none of importance occurred in the following 5 hours and 50 minutes. Hence this explanation fails.

TABLE IV.—WALLED-CELL SIZE OF STEEL OF 0.40-PER CENT. CARBON INCREASES WITH LENGTH OF EXPOSURE TO 1100° C. ±
(CONDENSED FROM TABLE III.)

Length of Exposure.	Cell Size, sq. mm.
1 minute*	0.009
3 " *	0.010
10 " †	0.026
6 hours†	0.027

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* Total time of heating. The very brief heatings were carried out by immersing small fine-grained pieces of steel, for the time indicated, in a bath of molten copper in which the thermo-couple lay, then withdrawing them, and allowing them to cool in air.

† Exclusive of time of heating up and cooling down.

Nor can errors in manipulation and observation explain the discrepancies, because if such errors existed they ought to cause discrepancies in all directions, whereas within the limits to be expected under the conditions, the results are concordant in every respect save and except only that the lengthening of the exposure of the medium-carbon steel does not lead to the expected network coarsening.

This concordance is shown by the facts (1) that in all three steels for given length of sojourn every elevation of temperature is followed by an increase in network size; (2) that in the hyper-eutectoid steel every lengthening of exposure to a given temperature increases the network size, with the single exception of the exposure to 900°, at which the growth is so slight and slow that it might well fail to efface initial variations in network size; and (3) in brief that as far as the data go, all the curves for all three steels are of the same family, with the sharp upward break near 1100° recorded previously by Osmond but unknown to me till after these curves were drawn. That the position of this break should vary with the composition and attendant conditions is not surprising.

Turning from these relatively concordant results, we find in both the medium-carbon steels very serious discrepancies which suggest strongly that some perturbing cause is stubbornly at work here, masking the expected coarsening influence of lengthening the exposure to a given high temperature, in itself antecedently most probable, and strongly reinforced by the coarsening of the hyper-eutectoid steel with every such prolongation of exposure. Thus, whereas lengthening the exposure from 10 minutes to 6 hours, or 35-fold, increases the network size of the hyper-eutectoid steel in one case 5-fold, in the other case 6-fold, it increases that of Steel I in the heatings from 900° to 1100° inclusive, in five cases by only between 4 and 37 per cent., and in the two remaining cases it actually lessens it very much. (This refers to the average network size for the several temperatures.)

Again, in the high-manganese medium-carbon Steel II, the furnace cooled specimens after 10 minutes exposure to 1000° and 1100° respectively are coarser than those exposed 36 times as long (6 hours) to those temperatures and air cooled.

In view of these results we understand readily the old habit, common among those who were familiar chiefly with rail and other medium carbon steels, of speaking of coarsening as if it depended on temperature only and were independent of time.

Such a perturbing cause is at hand in the specific effect which long and high heating both have in increasing the coalescing of the ferrite into visible masses in general and into the cleavages in particular (Principle 10, Paragraph 28, page 297). In these medium-carbon steels, especially the low manganese steel, we have already seen that th network structure is so quickly obscured by coalescing and by the appearance of independent ferrite grains within the walled cells that air cooling has to be resorted to in order to preserve the network structure in recognizable state. With this tendency of the network itself to become thus obscured, it is not surprising that discrepancies in network measurements should arise. It is rather a matter of surprise that the network growth with rising temperature should be as constant and regular as it is.

With these facts before us, it is easy to understand that prolonging an exposure to a given high temperature, and thereby increasing the coalescing of the ferrite in the subsequent cooling, should lead us to mistake these resultant masses and especially the cleavage massing for true grain boundaries, that is, the true network structure, and to count fragments of true walled cells as whole walled cells.

In short, I refer the failure of a great lengthening of sojourn at a given high temperature to reveal the expected coarsening of network in medium carbon steel, and its even lessening the apparent network size, chiefly to its incidental action in increasing, during the following cooling, the coalescing of ferrite into visible masses in general, and into the cleavages in particular.

In view of these conditions, we cannot look at even the relatively regular coarsening of the hyper-eutectoid steel as conclusive evidence of the coarsening of the austenite grains themselves with time and temperature. I therefore propose to test this by quenching steels of various carbon contents after various lengths of sojourn at various high temperatures, and examining the grain size of the resultant martensite. Should this follow the behavior of the network of hyper-eutectoid steel and coarsen with time and temperature, it would strengthen materially the already probable theory of a like coarsening of the austenite grains themselves.

58. *Very-Low-Carbon Steel.*—If we assume that the ferrite grains of very-low-carbon steel inherit the grain size of the austenite reached in Region 4, as seems probable, then the fact that both Osmond and Stead found no ferrite grain growth in dead-low-carbon steel after high heating, in spite of their noting great grain growth and coarsening of network in the case of higher-carbon steel, indicates that the grain growth of austenite in Region 4 is very slow in the case of such low-carbon steel.

Osmond, experimenting on steel of 0.02 per cent. carbon, says: "Annealing and hardening at various temperatures including whiteness did not change very appreciably the structure reached on long annealing;" and, tentatively, "It seems to result from this first examination that the structure of pure iron, once fixed by a prolonged heating at about 900°, is nearly independent of the subsequent temperature of heating and the rate of cooling."* This behavior of such very-low-carbon steel is in marked contrast with the great network growth which he records in the case of steel of 0.45 per cent. carbon on heating successively to 1015° and 1330°. In the same way, though Stead† found that the "grain size" (probably network size) of steels of 0.21-, 0.47-, 0.90-, and 1.14-per cent. carbon increased with the temperature reached in Region 4, yet, after noting the grain-refining at 900° of his ultra-low-carbon steel with only 0.01 per cent. carbon, he says "heating to 1200° does not apparently produce any difference in their [the grains] size."

This sentence seems to be based on inspection without direct measurement of the grain size. But beyond this he measured the grains of steel of 0.01 per cent. carbon after heating it either very briefly to temperatures rising to 1300° and immediately cooling it, or after a 4-hour exposure to temperatures reaching as high as 1000°, without material increase of the grain size in either case.

Their observations should, I believe, be interpreted as meaning that the grain growth of their ultra-low-carbon steel was too slight to be detected by mere inspection in the cases in which direct measurements were not made; and that in the cases in which Stead actually measured the grain size, the heatings were either too brief, or to too low a temperature, to induce a material coarsening. I interpret them thus because both Heyn and I find a material increase in ferrite grain size after heating very-low-carbon steel highly in Region 4 above the transformation range. Indeed, in 1904, Osmond and Stead evidently inclined to this opinion, modifying the sentences just quoted from Osmond by adding at their end "but such a conclusion would be premature . . . All that can be said is that the transformations of structure of pure iron are slower than those of carburized irons,"* referring of course to the grain growth in Region 4 above the transformation range, and not to the rapid grain growth of low-carbon steel in Region 7, between A1 and A2.

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* *Bulletin, Société d'Encouragement*, May 1895, pp. 25 and 29 of reprint; *Contribution à l'Étude des Alliages*, 1901, pp. 302 and 308; *Microscopic Analysis of Metals*, pp. 112, 113.

† *Journal, Iron and Steel Institute*, Vol. I, 1898, pp. 183, 184.

Heyn, on actually measuring the ferrite grain size of steel of 0.03 per cent. carbon, found that it increased materially with the time occupied in cooling from 1100°, that is, with the length of exposure to high temperatures in Region 4.† When heated in half an hour to 1100° and cooled either with moderate rapidity or by quenching, the grains were much smaller than before the heating; but when the cooling from 1100° lasted in one case about three-quarters of an hour and in another case about 7.5 hours, the grains were materially larger, in one case much larger than initially. The inference from this is that the grain growth of this very-low-carbon steel is so slow that during a short exposure to temperatures near and below 1100° it does not grow enough to make up for the grain shattering which occurred at Ac3, say 900°; whereas on greatly prolonging the sojourn at high temperatures in Region 4 the grain size, after the shattering which occurred at 900°, later approached and in one case grew beyond the initial size.

The grain growth, slow between 889° and 1125°, and then more rapid up to 1247°, noted by Fay and Badlam,‡ can hardly be used to show that such growth occurs in ultra-low-carbon steel, because their steel contained as much as 0.07 per cent. carbon.

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* *Microscopic Analysis of Metals*, London, 1904, p. 113.

† *Journal, Iron and Steel Institute*, Vol. 2, 1902, pp. 80, 105.

‡ *Technology Quarterly*, Vol. 13, 1900, pp. 311, 312.

My results in Table IV*a* and Fig. 6 show that the ferrite grain size, not only of steel of 0.03 per cent. carbon but even of electrolytic iron, increases materially with the temperature to which the metal has been exposed. I here condense these results, recognizing the fact that these grains may prove, on further examination, not to be grains in the strict sense of the word. Two independent heatings of unforged electrolytic iron agreed in showing grain growth of the same order of magnitude.

The slowness of the ferrite grain growth of all these ultra-low-carbon steels may possibly be due to their containing oxygen, which, assembling at the grain boundaries, may interfere with the contact of metal with metal necessary to contact coalescence. But this would hardly apply to the electrolytic iron.

TABLE IV*a*.—INCREASE OF FERRITE GRAIN SIZE ON HEATING ULTRA-LOW-CARBON STEEL AND ELECTROLYTIC IRON.

Heating.		Grain Size, sq. mm.		
Temperature reached, °C.	Time held there, hours.	Carbon,* 0.023 per cent. Electrolytic Iron.	Carbon,* 0.029 per cent. Electrolytic Iron.	Carbon, 0.027 per cent. Steel VI.
900	1	......	......	0.0046
925	1	0.0061	0.0035	......
1,200	1	......	......	0.0069
1,300	1	......	......	0.0372
1,300	2	0.0095	0.0098	......

For the reasons given in Principle 1, Paragraph 15, page 277, the slow grain growth of nearly carbonless austenite harmonizes with the theory of Ewing and Rosenhain. The mass as a whole being so nearly free from carbon, different dendritically connected parts would differ initially less in carbon content from each other and from the intervening metal than like parts in higher carbon steel would. After a given opportunity for diffusion, the residual differences in carbon content would be less than in a higher-carbon steel, with consequent less difference in potential and slower electrolytic grain growth.

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* The carbon content of the electrolytic iron was determined after heating.

More than twenty years ago I found enormous growth of the grain of commercially pure copper and platinum at high temperatures; and Heyn has found that, at 1000°, the grain of copper grows from 98 to more than 100,000 μ^2 in 26 minutes, including the time of heating up.*

The comparison of the assertions of different writers concerning this very-low-carbon steel must be made very cautiously, because we are not sure when they say "grain size" they mean the same thing. Andrews called attention to the different orders of grain size, grains and subdivisions of grains. For instance, it is striking that the grains which Stead found in steel of 0.01 per cent. carbon after heating to temperatures varying from 700° to 1300° and cooling slowly lay between the limits of 0.11 and 2.52 mm. in diameter, or with areas of from 0.01 to 6.4 sq. mm. Yet the largest of the areas of Heyn's grains of his 0.03-per cent. carbon steel after heating to 1100° and cooling at various rates, was 0.004 sq. mm., that is to say of a wholly different order of magnitude.

59. *Proposition 6. (Provisional.) The Rate of Network Coarsening Increases with the Carbon Content.*—This is supported by a comparison of the curves for the 0.03-, 0.46-, 0.40-, and 1.14-per cent. carbon steels of Figs. 6 and 7, pages 341, 342, if we allow for the retarding effect of the 1.21 per cent. of manganese in the 0.46-per cent. carbon steel. Moreover, it is in agreement with the results of Osmond and of Stead, which seemed to show that there was no grain growth of austenite in their very-low-

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* *Journal, Iron and Steel Institute*, Vol. 2, 1902, p. 103, Table VII. μ equals 0.001 mm. It is true that there are apparent exceptions to this general law, for instance that the grains of certain gold alloys, when gently heated, have been found to break up into smaller grains. (See Osmond and Roberts-Austen "On the Structure of Metals," *Proceedings, Royal Society*, June 18, 1896, p. 430.) So far as I have noticed, these disintegrations are of grains which for some special reason have been thrown out of equilibrium. In the case of the gold alloys just referred to, the break-up is readily understood. Before the heating which induced it; these pieces were not only in the cast state, with the unstable columnar structure which the rapid cooling during solidification induces, but further had been distorted by tensile test, and hence were inequiaxed, and hence had an additional element of instability. That these unstable grains should break up on reheating to between 200° and 250° and yield new equiaxed smaller grains, and indeed that larger grains which are unstable for any reason should break up, and that the new stable ones formed from their wreck be smaller, is only what we should expect.

Like breaking up of inequiaxed grains of steel on gentle heating is most familiar. Heyn found that it began at 400° and completed itself at 600° (Ueber die Nutzanwendung der Metallographie, *Stahl und Eisen*, Vol. 26, 1906, pp. 580 to 596), and Stead found that it completed itself at the melting point of aluminum, say 657°. (*Journal, Iron and Steel Institute*, Vol. 1, 1898, p. 157.)

carbon steel in Region 4 above A3, and with the moderate grain growth of the ultra-low-carbon steel and electrolytic iron of Table IV*a*. Further what appears to be the network size in Osmond's early measurements,[*] after heating to ten different high temperatures, is uniformly much larger in steel of 0.45 per cent. carbon than in that of 0.14 per cent.

A natural inference is that for a given temperature the mobility increases with the carbon content, which harmonizes with the fact that the solidus falls rapidly as the carbon content increases, so that for a given temperature in Region 4 above A3 the amount by which the steel lies below its temperature of incipient fusion is the less the higher the carbon content. Another explanation given under Principle 1, Paragraph 15, page 277, is that the congenital heterogeneousness of the austenite increases with the carbon content, and with it increases the difference of potential residual after a given opportunity for diffusion, and with this the rapidity of grain growth.

This proposition is in marked contrast to Provisional Proposition 22, Paragraph 73, page 370, which holds provisionally that the rapidity of ferrite grain growth is inversely as the carbon content.

60. *Proposition 7. The Proportion of Coagulated Ferrite Increases With the Slowness of Cooling.*—This is a necessary consequence of Principle 12, Paragraph 34, page 306, that ferrite becomes visible only through the coalescing of initially invisible particles into visible masses. This ferrite does not exist above the transformation range, but comes into existence during the cooling down through that range. Hence it is only during the cooling from the top of that range down that any coalescence of this ferrite is possible; and hence in turn the slower the cooling the greater the opportunity for coalescence into visible islets.

So far as I know this proposition was first indicated by Dr. Boynton when he implied that the quantity of coagulated ferrite was the less the faster the cooling and the smaller the sample, small size of course implying faster cooling under like conditions.[†]

It is true that he looked at the matter from the opposite point of view, namely, that rapid cooling increases the proportion of sorbite in his steels. But, as we now believe, this sorbite is simply a mixture, an emulsion too fine to be resolved by the microscope, of pearlite and ferrite. At a temperature high enough to permit coalescence to go on, the ferrite and pearlite of this emulsion severally coalesce into visible masses; so that the presence of sorbite is from this point of view substantially a shortage of recognizable ferrite and pearlite.

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[*] *Etudes Metallurgiques*, Plate III, Fig. 7, and pp. 62–71. From *Annales des Mines*, July–August, 1888.

[†] *The Iron and Steel Magazine*, Vol. 7, 1904, p. 472.

This proposition is verified by my micrographs. Thus the quantity of coagulated ferrite is very much greater in each of the micrographs of the furnace-cooled Rows 3 and 4, Plate II, than in the corresponding micrograph of the air-cooled Rows 5 and 6, Plate III.

61. *Proposition 8.—Long high heating retards the coagulation of sorbite into pearlite plus free ferrite or cementite*, thus increasing, *in air-cooled steel*, the quantity of sorbite and the areas free from coagulated ferrite. These are Items 3 and 7 of Principle 10, Paragraph 28, page 297. Dr. Boynton enunciated* along with Proposition 7, the proposition that long and high heating increases the quantity of sorbite in air-cooled steel, a proposition which is embraced in the present one. He informs me that his proposition was based on five concordant sets of observations, and that he has often verified it since; and Professor Sauveur, in whose laboratory the observations were made, informs me that he accepts it.† Most of what was said under Proposition 6 applies here, *mutatis mutandis*.

Turning now to the effect of long high heating in increasing the areas free from coagulated ferrite, Morse's micrographs agree with this proposition as well as can be expected in view of his not having it in mind, and hence not selecting spots specially suited for illustrating it.‡ Comparing his simple heatings to 900°, 1100°, 1200°, and 1300°, followed immediately by relatively rapid cooling (bedded in lime), we find certainly a decided increase in the size of the individual pearlite plus sorbite areas between his ferrite grains, and apparently an increase in their total area, as the temperature reached rises. Again, comparing the specimen heated to 900° and withdrawn immediately with those held at 900° for one-half and one hour; and also that heated to 1200° and withdrawn immediately with that held for half an hour at 1200°, we note that prolonging the exposure to a given temperature in Region 4 above the transformation range, increases the individual size and the collective area of the parts free from coagulated ferrite. But in such coarse-grained specimens the structure varies so much from spot to spot that, unless the spots are selected expressly for the purpose of testing this proposition, they cannot be accepted as conclusive evidence.

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* *Iron and Steel Magazine*, Vol. 7, 1904, p. 472.

† Private communications, August 5 and 7, 1911.

‡ *Transactions, American Institute of Mining Engineers*, Vol. 29, 1899, pp. 729–750.

My own micrographs tend in a general way to comply with this proposition. Like Morse's they were not selected with this proposition in mind. That prolonging the heating lessens the proportion of coagulated ferrite or cementite can be seen in every case in which it should be expected, namely, on comparing Figs. B, C, D, and E of Row 1, Plate II, with the corresponding figures of Row 2; on comparing Fig. E of Row 3 with E of Row 4; and on comparing Fig. C of Row 7 with C of Row 8. The exceptions ought to be, and are, reasonably explained. Thus in Figs. A of Rows 1 and 2, the quantity of visible cementite cannot be distinguished. In Figs. A, B, C, and D, Rows 3 and 4, the slow cooling seems to have afforded time for the practically complete assembling of the ferrite into visible masses even after the longer heating. In Rows 5 and 6 the influence of the longer heating was all too likely to be masked by that of unintentional variations in the rate of air-cooling because this proposition was not before me at the time.

As regards the influence of higher heating in increasing the areas free from coagulated ferrite or cementite, the indications of my micrographs are much less clear. This may be partly because the general look changes so much with the temperature reached that a comparison of the quantity of coagulated ferrite or cementite is not easy; partly because the etchings were not made with a view to bringing out this effect; and partly for the reasons given in the preceding paragraph. All that can be said is that, in Rows 1 and 6, the highest heated members have the least coagulated ferrite or cementite; and that most of the exceptions are capable of a reasonable explanation. Yet, in Row 4, though the highest heated but one, E, has indeed the least coagulated ferrite, the very highest heated, F, has more coagulated ferrite than any other.

We may accept this proposition as regards the quantity of sorbite. As regards the areas free from coagulated ferrite the proposition is antecedently probable, but its experimental support, though encouraging, is not strong enough to be conclusive.

62. *Proposition 9.—The network structure is most marked in specimens which have been cooled at an intermediate rate,* (1) slow enough to permit the massing of much ferrite into the grain boundaries where it forms cell-walls, yet (2) not so slow as to allow the spheroidizing of the ferrite in those cell-walls and the coalescing of the scattered ferrite within the kernels themselves to break it up.

This proposition is in accord with Principle 16, Paragraph 39, page 310, indeed almost a re-statement of it.

Experimental proof of it is given by my present micrographs. Thus comparing Rows 5 to 8, Plate III, with Rows 3 and 4, Plate II, the network structure is very clearly seen in every one of the relatively fast-cooling air-cooled and clay-cooled specimens, but in none of the much-slower-cooling furnace-cooled ones, excepting those (E and F of Row 4) in which very high heating has fixed the network structure, according to Proposition 13, Paragraph 66, page 362. In Fig. E of Row 3, the short exposure to 1200° has fixed this structure to an intermediate extent, as pointed out in the same place.

Again, in the relatively fast-cooled series, Figs. 24 to 28, Plate I, the network structure is very prominent, whereas in the more slowly cooled series, Figs. 18 to 23, the structure of the same steel has passed through the network stage, and shows in Fig. 20 the break-up of the network or cell-walls through spheroidizing. Indeed, in preparing this series, Figs. 24 to 28, so as to show the progressive thickening of the cell-walls in the passage down through the transformation range, I had to rely on this principle that an intermediate rate of cooling develops the network structure without destroying it.

Two micrographs of Dr. Boynton's illustrate this proposition that the development and preservation of the network structure require an appropriate intermediate rate of cooling.* This structure is very marked in a specimen of hyper-eutectoid 1.11-per cent. carbon steel cooled in the furnace from 1100°, but is absent from a like specimen treated in the same way except that it was air-cooled. Again, it is very prominent in the hypo-eutectoid 0.52-per cent. carbon steel of Sauveur and Boynton* air-cooled from 1100,° but not in a specimen treated in exactly the same way save that it was cooled in the furnace. In the case of Dr. Boynton's hyper-eutectoid steel the air-cooling has not given the time needed for the small quantity of free cementite to coalesce into a visible network, though the slower furnace-cooling has; and accordingly the air-cooled specimen is free from coagulated cementite, whereas the furnace-cooled one has a well-marked cementite network.

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* *Iron and Steel Magazine*, Vol. 7, 1904, p. 477, Figs. 7 and 8.

In the case of my hypo-eutectoid steel the air-cooling has given the abundant free ferrite time thus to coalesce, but the slower furnace-cooling has given time for the additional ferrite scattered through the kernels to coalesce into islands which mask the network, and has given the cell-walls themselves time first to spheroidize and then to break up; and accordingly we find well-marked network or cell-walls in the air-cooled specimens, but an abundance of irregular scattered ferrite islets without traceable network structure in the furnace-cooled ones shown in Figs. B to D of Rows 3 and 4, Plate II.

A difference in the rate of cooling of the various layers is probably the cause of the great difference in the size of the network of the two halves in Fig. G of Row 10, Plate IV. This figure represents Steel I (carbon 0.40 per cent.), heated for 6 hours at 900° under potassium chloride, and cooled in an air blast. Here the conditions of heating must have been closely alike throughout the specimen, but of course air-cooling would be far more rapid in the outside than in the inside. It is probable that, had the cooling of the right side been somewhat slower, it would have developed a network like that of the left side. These two networks are evidently of different orders. The coarser one may represent the persistence of the initial network of solidification.

63. *Proposition 10. The intermediate rate of cooling needed for catching the network structure during its transitory life is more*

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* *Transactions, American Institute of Mining Engineers*, Vol. 34, 1904, p. 150.

rapid in the case of hypo-eutectoid steel than in that of hyper-eutectoid steel.—In other words, cementite, whether in the cell-walls or scattered within the kernels themselves, coalesces, spheroidizes, and breaks ranks less rapidly than ferrite under like conditions. A beginning of this spheroidizing of cell-wall cementite is plainly seen in Figs. D and F of Row 9, Plate IV.

Three cases may be cited in support of this proposition.

First, taking jointly the cases just cited from Professor Sauveur and Dr. Boynton, though air-cooling suffices to bring out the ferrite cell-walls in hypo-eutectoid steel, those walls cease to be traceable after the slower furnace-cooling. On the other hand, the cementite cell-walls of hyper-eutectoid steels in their cases have not time to form in an air-cooling, yet not only form but persist unbroken in a furnace-cooling.

Second, much the same thing can be traced in my micrographs. The cementite cell-walls persist unimpaired through the furnace-cooling of my hyper-eutectoid steels, Rows 1 and 2, Plate II; but the ferrite cell-walls of hypo-eutectoid steel, though very plain after air-cooling, have broken up and cease to be visible after furnace-cooling, as is readily seen by comparing Rows 3 and 4, Plate II with Rows 5 to 8, Plate III.

Third, Professor Arnold's micrographs of blister steel show that, in spite of the extreme slow cooling which follows the cementation process, lasting 14 days, the cementite network persists in some cases but little broken. Thus it is very perfect in the micrograph of his steel of 1.6 per cent. carbon (his Section 8), and is prominent in at least part of each of his other micrographs of hyper-eutectoid blister steel.* Yet it is evident that even cementite may at last break ranks and thus break up the network system, for in parts of five of his micrographs of hyper-eutectoid blister steel, those with 1.3, 1.4, 1.5, 1.8, and 1.9 per cent. of carbon, the cementite network is either absent, or much less perfect than in my much less slowly cooled specimens, Rows 1 and 2, Plate II.

64. *Proposition 11. True network structure habitually arises through one of the two processes of differentiation, (1) solidification, and (2) transformation.*—In other words, it arises in a sojourn

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* The Micro-Chemistry of Cementation. *Journal, Iron and Steel Institute*, Vol. 2, 1898, pp. 185–199.

either in the freezing range or in the transformation range, so that a slow cooling which begins very far below the top of the transformation range does not generate the network structure.

This is in accord with Principle 9, Paragraph 26, page 295.

It is in general accord with this law that steel ingots and other castings, before annealing, generally have a marked cellular structure, due to the differentiation which occurs during solidifying, and the formation of a network poorer in carbon than the meshes which it encloses. Further, that once this initial network has been removed, whether by mechanical or thermal treatment, no characteristic network structure is again set up except by slow cooling through at least part of the transformation range, after an exposure to a temperature in Region 4 high enough, and for a time sufficient, to set up in the austenite a coarseness of grain sufficient to cause, with the ferrite which it rejects in cooling through the transformation range, a visible network structure.

For instance, the hot-finished parts of rails habitually show a true network; whereas the cool-finished edges of the flange may show none. This agrees with the above proposition, for the hot-finished parts have presumably cooled with little mechanical distortion from a temperature high enough in Region 4 to generate austenite grains of considerable size, which in turn generate a corresponding network structure in passing through the transformation range. But the cold-finished parts have had their austenite grains which formed at a high temperature broken up by the rolling, and this process has been repeated either till the final austenite grains which have formed after the mechanical distortion ceased are so fine grained as not to give rise to the characteristic network,* or till the temperature has fallen to within the transformation range.

Further, heating followed by slow cooling does not give rise to a network unless it includes a sojourn long enough and high enough above the bottom of the transformation range to give rise to austenite grains of appreciable size. Thus in my micrographs the network is plain in all those members of Rows 5 and 6, Plate III, which have cooled from well above the top of the transformation range, B to E; but it is not recognizable in A, the temperature of which did not rise above the transformation range.

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* Sauveur, *Transactions*, *American Institute of Mining Engineers*, Vol. 22, 1893, pp. 546–547, and especially Plates IV and V.

This proposition is supported by an analysis of the remarks of M. Osmond and the micrographs of Professor Campbell. Thus M. Osmond's words lead us to infer that, in the case of a steel of 1.25 per cent. carbon cooled slowly, the network was lacking when the cooling was from 750°, that is, from a point low down in the transformation range, but present when it was from 1015°, or from above that range.[*]

Professor William Campbell's hyper-eutectoid steels had initially the rather fine network structure due to the low finishing temperature of their rolling by the maker. He then heated a series of pieces of each steel to a series of temperatures, and cooled them slowly. Though we cannot interpret the results with complete confidence, yet it appears that in those cases in which the temperature rose above the transformation range, and in the case in which his 1.61-per cent. carbon steel rose nearly to the top of that range, a well-marked network structure was developed in cooling down through that range, but not in the cases in which the heating fell far short of the top of that range.[†]

Some of my unpublished results show that this assembling of cementite into a network is accomplished by a sudden loss of ductility, as we should naturally expect, because during overstrain these large, sharp, hard plates would easily shear into the neighboring metal gashes too deep to heal by flow.

The remark of Sauveur and Boynton that, in the case of

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[*] *Microscopic Analysis of Metals*, Osmond and Stead, pp. 144–145. Here one has to read between the lines.

[†] *Proceedings*, Vol. VI, 1906, pp. 211–239. For his several hyper-eutectoid steels, the highest temperature which failed to cause the network structure, and the lowest which did yield that structure were as follows:

Carbon content, per cent.	Peak temperature.		Probable position of A_3.
	Network lacking.	Network present.	
2.04	1070°	1200°	1130°
1.94	1070	1200	1130
1.72	950	1200	1130
1.61	855	1070	1090

It is true that, in those heatings of all but the last of these steels which yielded a network structure, there was an excursion above the eutectic melting point; but as in none of these cases did it go as much as one-third way towards the liquidus, it is less probable that the network structure formed in that excursion than that it formed in cooling through the transformation range.

steel of 0.52 per cent. carbon, air cooling from temperatures exceeding 1000° always gives rise to a "network" structure,* is in general agreement with this proposition. In my micrographs a peak temperature of 900° suffices, under favorable conditions, to set up a network structure which persists through cooling, as in the furnace-cooled B of Row 2, Plate II (hyper-eutectoid) and the air-cooled B of Rows 5 and 6, Plate III (hypo-eutectoid, $C=0.40$).

The interpretation of the evidence is complicated by the considerations that, other things being equal, high and long heating tend to fix the network structure, and that slow cooling tends to break it up. Hence the proposition must be applied cautiously, and indeed the evidence, though encouraging, is far from conclusive.

In order that the reader may the more readily see how the numbers just given bear upon this proposition, the probable position of part of the line *SE* is sketched in Fig. 5.

65. *Proposition* 12. *The assembling of ferrite in parallel bandings is favored by long and high heating in Region 4.*—This proposition accords with Principles 10 and 17, Paragraphs 28 and 40, pages 297, 310.

That the thoroughness of crystalline organization caused by very high heating enables the austenite, in later cooling through the transformation range, to expel into the cleavages of the individual austenite grains, as well as into the boundaries between grains, the ferrite which it then generates, is suggested by the micrographs of my highly heated specimens, for instance E and F of Row 4, Plate II, E of Row 6, Plate III, and F of Row 10, Plate IV, and by Morse's Figs. 24 and 25, all of which represent heatings to 1200° or higher. These micrographs indicate strongly a concentration of ferrite in the octahedral cleavages of the mother austenite.

Perhaps the most striking case of all is the parallel banding of Belaiew's steel of 0.55 per cent. carbon, cooled extremely slowly from the molten state, with a sojourn of some 16 hours in Region 4, including of course the extremely high temperatures just below the melting range.† He seems to show conclusively that this banding is in the octahedral cleavage planes of the austenite. We do not know positively whether the network in this case represents the persistence of the initial network formed during solidification; or whether, as is more probable, the initial network was broken up by diffusion during the long sojourn in Region 4, so that the present one represents a new network formed in cooling through the transformation range. But in either case it is probable that this cleavage massing of ferrite arose in cooling through the transformation range; and we naturally refer its extraordinary prominence to the length and height of the sojourn above that range, in Region 4.

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* *Transactions, American Institute of Mining Engineers*, Vol. 34, 1904, pp. 150-158.

† *Revue de Metallurgie, Memoires*, Vol. 7, 1910, pp. 510-521.

After this concentration has thus been made clear to us, we readily interpret the parallel off-shootings of the ferrite tendrils, for instance in my Figs. E of Rows 5 and 6, Plate III, as a result of this same local concentration; and then in turn we refer to this same cause the Widmanstättian figuring for instance of my Figs. E and F of Row 4, Plate II, and Morse's Fig. 27. Osmond, Morse, and Fay and Badlam called attention to the parallel banding of the ferrite in relatively low-carbon steel which had been highly heated. The ferrite in Osmond's 0.14-per cent. carbon steel tended "to lengthen into groups of juxtaposed bands" when heated to above 1000° and cooled slowly.* His micrographs show this parallel banding strikingly on slow cooling from 1330°, but not markedly on slow cooling from 1015°.

Morse's† 0.343-per cent. carbon steel had banded ferrite after holding half an hour at 900°, and this banding increased progressively with the temperature and length of exposure. The ferrite in Fay and Badlam's‡ 0.07-per cent. carbon steel was already changing to the banded type at 1075°, and with further rise of temperature the banding became much stronger.

Like cleavage massing of the cementite of hyper-eutectoid steel is seen in Figs. E of Row 1, and C of Row 2, Plate II; and much more strikingly in Osmond's micrograph§ of blister steel cooled in the air direct from the cementation furnace. Here the very long heating of the cementation process seems to have caused a very strongly marked cleavage massing of the cementite.

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* *Bulletin Société d'Encouragement*, May, 1895, p. 26, Figs. 159 and 160 of reprint; *Etudes des Alliages*, p. 305, Figs. 18 and 19; *Microscopic Analysis of Metals*, p. 117, and Figs. 58 and 59.

† *Transactions, American Institute of Mining Engineers*, Vol. 29, 1899, pp 739, 746, 747.

‡ *Technology Quarterly*, Vol. 13, 1900, p. 312.

§ *Microscopic Analysis of Metals*, Fig. 86, p. 149; *Etudes des Alliages*, Fig. 46, p. 317.

The cleavage massing of the ferrite in very highly heated specimens is so striking that it might easily suggest that it represented heating to Ball's point, estimated at about 1300°. But Morse's micrographs * show that a structure substantially like this arises after a sufficiently long heating at 900°, and my Fig. 22, Plate I, shows that heating to 1000° may cause it. Indeed we question whether Ball's point is not a misnomer, and whether it is not rather Ball's effect that ought to be spoken of, an effect reached quickly at 1300° and more slowly at lower temperatures. This effect is a sudden dropping of the tensile strength of quenched low-carbon steel when the quenching temperature rises past a point estimated at about 1300°.†

66. *Proposition 13. The network structure is made more persistent by long high heating in Region 4.*—This proposition is in accordance with Item 5 of Principle 10, Paragraph 28, page 297. Four facts are offered in support of it.

(1) *Hypo-Eutectoid Steel.*—In my own micrographs high heating invariably has this effect. The opportunity offered by furnace-cooling for the break-up of the network structure sufficed to remove all clear trace of that structure in the micrographs of Rows 3 and 4, Plate II, when the heating was to any temperature below 1200°. But when the temperature rose to 1200° or 1300° and was held there for 1 or 2 hours (Figs. E and F of Row 4) the ferrite network successfully resisted the tendency to break up in the succeeding furnace cooling, and it stands forth prominently. Further, with a shorter stay at 1200° (10 minutes, Fig. E of Row 3) the network has such an intermediate degree of persistency as corresponds roughly with this intermediate strength of the fixing conditions, for though it is much less marked than in the higher or longer heated E and F of Row 4, yet, as I have sketched in broken lines, traces of it persist such as cannot be detected with like confidence in the lower-heated specimens of this steel. Looking through a large number of my own micrographs, I find this proposition always complied with, though it was unsuspected when they were made.

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* *Transactions, American Institute of Mining Engineers*, Vol. 29, 1899, pp. 739, 746, 747.

† *Journal, Iron and Steel Institute*, Vol. 1, 1890, p. 88, and Plate VI.

(2) The observation of Sauveur and Boynton, cited under Proposition 11, Paragraph 64, page 359, that steel of 0.52 per cent. carbon when air-cooled from temperatures exceeding 1000° always has the network structure, supports the present proposition.

(3) In a general way the persistence of the network set up during solidification, familiar to us all in the steel castings, tends to support this proposition. This structure is in a general way like that which arises in cooling through the transformation range. While it is true that the long or high heating needed for breaking it up in the annealing of steel castings* has been shown by Professor William Campbell to be due to the presence of foreign matter,† such as manganese sulphide, in the absence of which a heating to just above Ac3 suffices; yet the persistence of the network in such a case as that of Belaiew's casting, whether the network there is of solidification or of transformation, tends to support the present law.‡ The cooling of this casting from the top of the transformation range (Ac3) to 500° lasted about 18 hours, and so gave abundant opportunity for the break-up of the network; nevertheless that structure remained extremely prominent. The force of this illustration is weakened by our ignorance as to whether this particular casting had or lacked manganese sulphide and its equivalents.

(4) *Hyper-Eutectoid Steel.*—This proposition is strongly supported by the contrast between the persistence of the cementite network during Professor Arnold's extremely slow cooling of blister steel referred to under Proposition 10, Paragraph 63, page 357, with its breaking ranks and balling up in the reheating of Professor Campbell's hyper-eutectoid steels, described under Proposition 11, Paragraph 64, page 358. Briefly, Professor Arnold's hyper-eutectoid blister steels, even after the 8 to 11 days' heating followed by the 14 days' cooling of the cementation process, still preserved their cementite network to a greater or less degree; whereas the initial cementite network in Professor Campbell's steels, evidently formed in cooling from the relatively low temperature at which the rolling ceased, broke

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* Howe, Campbell, and Koken, *Proceedings*, Vol. VIII, 1908, pp. 185–189.

† *Proceedings*, Vol. IX, 1909, p. 370.

‡ *Revue de Metallurgie, Memoires*, Vol. 7, 1910, pp. 510–521.

down sometimes completely, sometimes partly, on re-heating to 650°.

Between these unstable networks of his, formed presumably at a relatively low temperature, and the stability of my cementite network formed after heating to 1300°, there is a like contrast. The network shown in Fig. F of Row 2, Plate II, formed in furnace cooling after 1 hour at 1300°, showed no sign of breaking up on reheating to 650° for 6 hours, and at most a first beginning of a break-up after a further holding at about 700° for nearly 6 hours.

The opportunity for thus breaking ranks was much longer and apparently much better in Professor Arnold's case than in Professor Campbell's if we consider the total opportunity, through exposures to temperatures slightly below the transformation range, where the austenite crystalline structure, which set up the network during the cooling through the critical range, has now abdicated, yet the temperature is still high enough and the metal hence still mobile enough to facilitate the break-up of the network. Some influence other than mere opportunity for break-up is here at work. The difference in treatment, that Professor Campbell's steels after slow cooling were reheated to below the transformation range, whereas Professor Arnold's were not, does not explain readily the difference in behavior, the persistence of Professor Arnold's cementite walls during his greatly retarded cooling, and their break-up in the reheating of Professor Campbell's on reheating after a far less slow cooling. So we naturally refer the striking persistency of Professor Arnold's network to the fixing effect of the very long high heating of the cementation process.

Here we are reminded of certain micrographs of Goerens's, which show that the eutectic structure in cast iron is extremely persistent. This eutectic of course consists of alternate plates of austenite and cementite, of which the former resolves itself into pearlite and pro-eutectoid cementite during slow cooling. Sharp unrounded blades of eutectic cementite, which seem to have drawn to themselves the whole of the pro-eutectoid and eutectoid cementite, persisted after heating for 12 hours at 680° in the case of a white cast iron of 4 per cent. carbon all combined, quenched from the molten state in ice water; and well-marked remains of the eutectic structure persisted after 50 hours' heating at 650° to 680° in the case of a cast iron of 2.10 per cent. combined carbon and 1.50 per cent. graphite which had been cooled slowly.*

67. *Propositions 13 to 19. Influence of Manganese.*—These seven propositions may well be considered together.

The presence of manganese:

Proposition 13, retards the formation; and

Proposition 14, retards the break-up of the network; and

Proposition 15, increases the area free from coagulated ferrite.

It decreases:

Proposition 16, the size of the ferrite grains; and

Proposition 17, the size of the lamellæ of pearlite; and

Proposition 18, the size of the grains of austenite formed in Region 4; and

Proposition 19, of the network based on them.

The presence of manganese thus has a retarding effect, and in general is equivalent to a lowering of the temperature reached in Region 4, or to a shortening of the sojourn at that temperature.

These propositions are in accord with Principle 24, Paragraph 48, page 322, and indeed could be deduced from it. The evidence supporting it may be considered under these seven heads, Propositions 13 to 19.

68. *Roberts-Austen's Data*, covering Propositions 15, 16, 17, and 18.—In his fifth report to the Alloys Research Committee, compare (16) the striking network, (17) the small quantity of coagulated ferrite, and (18) the small size of the ferrite grains in his rail steel with 1.009 per cent. of manganese, in his Fig. 14, with the corresponding features of his steel of like carbon content but only 0.69 per cent. of manganese, in his Fig. 13. Again, as in his Figs. 57, 58, 63, and 64 the manganese content rises

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* *Ueber die Vorgänge bei der Erstarrung und Umwandlung von Eisenkohlenstofflegierungen*, Halle a. S., 1907, Figs. 35 and 38; *Metallurgie*, Vol. 4, 1907, Figs. 122 and 125; and *Mitteilungen aus dem Eisenhüttenmannischen Institut*, Aachen (Wüst), Vol. 2, 1908, Figs. 123 and 126. In the latter case the initial eutectic structure can still be traced, though obscured (1) by the balling up of its own constituents, (2) by the coalescence of its own cementite with the free and the pearlitic cementite, and (3) by the balling up of the pearlitic ferrite. The exact conditions of the reheating in this latter case are somewhat uncertain. See item 6, p. 32, note to Fig. 38, and first line, p. 40, of reprint.

from 0.50 to 0.70 and 0.90 per cent., note (16) the corresponding increase in the clearness of the network, the shrinking (17) of the total ferrite area, and (18) of the separate ferrite grains. His Fig. 59 is an exception unexplained by his data. I do not reproduce his figures.

In like manner compare these high-manganese steels with any and all of the pure low-manganese ones which the rest of the report gives; or with Heyn's most admirable series of 258 micrographs, all of low-manganese and all but a few of ultra-low-manganese carbon steel.* In none of Heyn's hypo-eutectoid steels is there (16) any development of the cellular structure comparable with that of these divers rail steels which I have cited. Moreover, in Heyn's steels of about this carbon content (17) the total quantity of coagulated ferrite and (17) the size of the ferrite grains is much greater than in Roberts-Austen's high-manganese rail steels. Of course I except the network formed in unannealed steel castings during solidification, and the steels quenched from within or above the transformation range.†

Proposition 19.—That manganese lessens the size of the lamellæ of pearlite was reported and insisted on by Roberts-Austen as early as 1899.‡

69. *The Author's Results* cover Propositions 13, 14, 15, 17, and 19, as is shown by a comparison of the low-manganese steel in Rows 3, 4, and 6, Plates II and III, with the high-manganese steel in Rows 11 and 12, Plate IV.

Proposition 13.—That manganese has retarded the formation of the network is seen clearly by comparing C and D of Rows 5 and 6, with C and D of Row 12, all relatively rapidly cooled in the air. In the low-manganese specimens the ejection of the ferrite has, even in this rapid cooling, gone so far as to set up strong cell-walls; whereas in the high-manganese specimens so little ferrite has been massed in these cell boundaries that the cell-walls (network) are still extremely thin.

Proposition 14.—That manganese has retarded the break-up of the network is seen by comparing B, C, and D of Row 11,

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*Mikroskopische Untersuchung von Eisenlegierungen. *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, 1904, pp. 355–397.

† As regards the size of the ferrite grains see Heyn's Fig. 151, the magnification of which is nearly that of Roberts-Austen's. In support of these other assertions see Heyn's Figs. 54, 55, and 140.

‡ Fifth Report, Alloys Research Committee, pp. 61, 62.

with B of Row 4, and C and D of Row 3, all of them cooled slowly in the furnace. In this slow cooling the network of the low-manganese specimens has become masked by the great precipitation and coalescence of the ferrite; in the high-manganese ones it is still manifest, the slow cooling having resulted in the spheroidizing of the ferrite in the network, yet without in general masking the network.

But that even in this higher-manganese steel the network will break up during a sufficiently long exposure to temperatures just below the transformation range is indicated by Fig. E of Row 10. In this much more slowly cooled specimen, though the network can still be traced, not only is it far less clear than in the faster cooled B, C, and D of Row 11, but much of the ferrite has already taken on the granular outline so often seen in such steels when they have cooled so slowly as to lose the network altogether.

Proposition 15.—That manganese has restrained the formation of coagulated ferrite is seen at a glance on comparing any one of the micrographs of the high-manganese steel, Rows 11 and 12, with the corresponding low-manganese one in Rows 3, 4, and 6.

Propositions 17 and 19.—That manganese has restrained the coarsening of the network, and hence presumably restrained the growth of the grains of austenite which are the parents of the cells of that network, is seen clearly in Table III and Fig. 6. This effect can also be traced in the micrographs, by comparing Row 6, Plate III, with Row 12, Plate IV, though in some cases not so confidently as in Table III, because so much depends on the exact spot photographed, and because a moderate difference in cell-size is not recognized easily by the eye, but has to be determined by actual measurement or counting.

70. *In General, Propositions 14 and 19.—Proposition 14. Preservation of the network* by the presence of manganese is familiar to us in the well-marked network common to rail steels, with their large manganese content, in spite of their cooling slowly enough to favor such a coalescence of ferrite as would in low-manganese steel mask if not break up the network.*

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* For illustration of this striking cellular structure in rails see Job, *The Metallographist*, Vol. 5, 1902, pp. 177–191; P. H. Dudley, *Idem*, Vol. 6, 1903, p. 111; Sauveur, *Transactions, American Institute of Mining Engineers*, Vol. 22, 1893, pp. 546–557.

Proposition 19 (*again*).—It is true that Roberts-Austen's assertion that manganese increases the grain size might at first be thought to refer to network size, and thus to contradict Proposition 19. But this contradiction disappears on careful examination. His cooling was so slow as to break up the network, that is, the walled cells, of his low-manganese steel by the precipitation of ferrite islets within them; whereas in his high-manganese steels this precipitation of ferrite islands is prevented, so that the network persists unbroken. His words should be interpreted to mean that the fragments of the walled cells in low-manganese steel are smaller than the unbroken walled cells in steel richer in manganese, which does not contradict the proposition that manganese lessens the true network size.

71. *Proposition 20* (*Provisional*). *Slow cooling coarsens the network only through lengthening the exposure to the high range of temperatures at which austenite grain-growth occurs, and primarily to the temperatures above the transformation range.*—In other words the network-coarsening effect of slow cooling is due not to the fact that it consists of a slow descent as such of temperature, but that it involves a collectively long exposure to a series of temperatures high enough to favor austenite grain growth. Thus, if a slow heating through Region 4, above the transformation range were so arranged as to occupy in rising through the several differentials of temperature exactly the same length of time that is occupied by a slow cooling, that heating and that cooling should have the same effect on the austenite grain size. Hence, after a long exposure to a high temperature above the transformation range, the walled-cell size or network size is affected relatively little by a change from a moderately slow to a rapid cooling, because here the lessening of the opportunity for coalescence is so small a fraction of the total opportunity.

This proposition follows from Principle 22, Paragraph 46, page 320. It may be difficult to verify it in the case of very slow coolings, because they obscure the network.

My data in Table III, page 344, agree fairly with this proposition. Thus, in the case of 10-minute heatings, the substitution of a furnace-cooling for an air-cooling increases the opportunity for coalescence in so large a proportion that it ought to coarsen the network materially. Accordingly in Table III we find that the walled-cell size of the furnace-cooled Specimen 84 heated 10 minutes at 1000 is 60 per cent. greater than that of the like but air-cooled Specimen 85. In conformity with the proposition, in the case of 6-hour heatings the substitution of a furnace- for an air-cooling does not increase the opportunity for coalescence in anything like so great a proportion; and accordingly this substitution in the case of B of Rows 11 and 12 (Specimens 92 and 104, Table III, heated at 900° for 6 hours), increases the walled-cell size by only 20 per cent.

Thus the experimental basis for the proposition is encouraging, though hardly sufficient.

72. *Proposition 21. (Provisional). Slow cooling through and below the transformation range coarsens the ferrite grains, and after a brief high heating slow cooling above that range also coarsens those grains.*—This is in accord with Principle 23, Paragraph 47, page 321.

Heyn's results support this proposition. On heating a series of like pieces of boiler-plate steel containing 0.03 to 0.04 per cent. of carbon and 0.27 to 0.28 per cent. of manganese at the same rate to 1100°, and cooling them thence at very different rates, he found that the ferrite grain size increased continuously and very greatly with the slowness of cooling.* Of course this goes to prove only that slow cooling increases the ferrite grain size, but it neither supports nor opposes the limitations of that coarsening which Proposition 21 postulates.

The familiar greater coarseness of ingots and other castings cooled slowly from a state of fusion than of those cooled rapidly is to be referred chiefly to slowness of solidification, and only secondarily to slowness of cooling from the freezing point down. For instance, of two like castings, cast in a baked sand mold, of which one is removed from the mold at the instant solidification is complete and quenched in water, while the other is allowed to cool very slowly in the mold, though the original columnar structure is likely to perist in both, in the slowly cooling one it is probable that coalescence of the austenite grains will further somewhat exaggerate the coarse grain.

Brinell asserted that steel of 0.52 per cent. carbon has a porcelainic fracture (F) when poured into water, but a coarse hackly one (A) when cooled slowly from the melting point.[*] Professor Sauveur too has shown that the network is much smaller in a fast-solidifying small ingot than in a slowly solidifying large one.[†]

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* *Journal, Iron and Steel Institute*, Vol. 2, 1902, p. 105.

73. *Proposition 22.* (*Provisional.*) *The rapidity of ferrite grain growth below A2 in Regions 7 and 8A, increases in low-carbon steel as the carbon content decreases.*—This is in accord with Principle 2, Paragraph 16, page 281. The greater the carbon content, the greater will be the quantity of pearlite, while the steel is in Regions 7 and 8A. This pearlite must needs act as a foreign body to prevent contact coalescence of the ferrite grains.

This more rapid grain growth of very-low than of higher-carbon steel in Regions 7 and 8A, is in marked contrast with the extremely slow grain growth of very-low-carbon steel in Region 4. (See Principle 5, Paragraph 21, page 289, and Proposition 5, Paragraph 53, page 337.)

In support of this proposition the following evidence may be cited:

Stead noticed, in his experiments on the grain growth of very-low-carbon steel at from 650° to 750°, which includes the temperature of rapid grain growth of alpha iron, "that the coarse granulation commenced to form at the surface of the bars, and in many of the results given it did not penetrate very far into the interior."[‡] This material had been decarburized by long heating in iron oxide; and it is a natural inference that the outside was more thoroughly decarburized than the interior: hence the greater grain growth at the outside than within.

Heyn also noticed, on cooling boiler-plate steel at varying rates from 1100°, that the surface zone was very much coarser grained than the interior. The carbon content of the surface zone was 0.03 per cent.; that of the core zone 0.04 per cent.[§] These numbers seem to refer to the initial composition of the steel. If so, and if as appears surface decarburization was not prevented in his experiments, then at the end of these experiments there should have been a still greater difference in carbon content between outside and inside.

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[*] *Stahl und Eisen*, Vol. 5, 1885, p. 619.

[†] *Transactions, American Institute of Mining Engineers*, Vol. 22, 1893, p. 548.

[‡] *Journal, Iron and Steel Institute*, Vol. 1, 1898, p. 161.

[§] *Idem*, Vol. 2, 1902, pp. 80, 105.

The greater coarseness of the outside than of the inside in Stead's and Heyn's results is not strong evidence, because it may be due to some other cause. Indeed, Metcalf pointed out in 1887 that large bars when quenched from a very high temperature were much coarser outside than inside,* and Ewing and Rosenhain noticed that in many cases "the largest and most rapidly growing crystals were formed at or near the edges of the specimen" of rolled sheet lead.†

Heyn further noticed a continuous increase in the ferrite grain size as the carbon content fell from 0.51 to 0.11 per cent. in a series of six steels examined in their rolled state. This increase was extremely rapid as the carbon content fell from 0.14 to 0.11 per cent.‡ In this case also the ferrite grain size was on an average somewhat greater in the outside than in the middle of the bars, in spite of the fact that the effect of superficial decarburization is here strongly opposed by the more rapid cooling of the outside than of the inside, which by itself would tend to restrict the grain growth of the outside. Ewing and Rosenhain noticed that the ferrite grains were smaller in higher than in lower carbon steel.§

This proposition is in general accord with the familiar observation that the fracture of steel in its usual marketable state is the coarser the less carbon it contains, at least from 0.90 per cent. down.

No one of these observations is conclusive in itself. But in view of the antecedent probability of the proposition, their agreement is such as to justify adopting it provisionally.

74. *Proposition 23. The rapid coarsening of very-low-carbon steel occurs in Region 7 and the upper part of Region 8 (say 650° to 750°); that of higher-carbon steel in the upper part of Region 4.*—But for all steels grain refining occurs at or very slightly above, the Ac3 corresponding to their carbon

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* The Author, *The Metallurgy of Steel*, p. 176.

† "The Metallographist," Vol. 5, 1902, p. 93.

‡ Mikroskopische Untersuchung von Eisenlegierungen. *Verhandlungen des Vereins zur Beförderung des Gewerbfleisses*, 1904, p. 369.

§ The Metallographist, Vol. 3, 1900, p. 127.

content, whatever that content may be. (Principle 20, Paragraph 43, page 317.)

This proposition is a brief recapitulation of parts of Propositions 1, 3, 4, and 5, Paragraphs 49, 51, 52, and 53, pages 324, 328, 333, and 337.

Acknowledgment.—My hearty thanks are due first and foremost to my Assistant, Mr. Arthur G. Levy, B.Sc., for his untiring and efficient aid. Moreover, I thank most gratefully Wheaton B. Kunhardt, Esq., of the Carpenter Steel Co., Dr. J. A. Mathews, of the Halcomb Steel Co., and Professor Charles F. Burgess, for the various steels experimented on; my colleague, Professor William Campbell, for photographs, suggestions, and advice; Messrs. Booth, Garrett, and Blair, and the Pittsburgh Testing Laboratory for the chemical analyses given in this paper; the Bureau of Standards for the determinations of the transformation points; and the Carnegie Institution of Washington for the grant under which this work has been done in part.

SUMMARY.

After an introduction for the inexpert (page 262) I propose, in Part II, 24 principles underlying certain structural changes. I distinguish between the grains of ferrite, cementite, austenite, or pearlite on one hand and the walled cells, each with a ferrite or cementite wall and a kernel of austenite, martensite, or pearlite on the other hand (page 273). An assemblage of such cells forms the network structure. Each kernel represents an austenite grain, and the walls represent the ferrite or cementite ejected by that kernel in cooling through the transformation range. I do not consider the like structure which forms during solidification (page 275).

Grain growth occurs by the union of neighboring grains, rather than by partitioning. The coalescence may be by contact (perhaps necessarily electrolytic), or migratory, of which the former should be the more rapid. The latter does not imply the direct union of grains initially wholly insulated from each other (page 277).

The rate of grain growth should in general increase with

the temperature, but in the transformation range itself such acceleration of the grain growth of ferrite should be checked as the temperature rises by the increasing quantity of impeding austenite, and that of austenite should be further accelerated by the progressive decrease in the quantity of impeding ferrite or cementite (page 287).

I consider seven effects of high and long heating in addition to grain growth, and suggest as a working hypothesis that they are due to the strengthening of the crystalline organization of the austenite (page 297).

The development of a network (cell-walls) is not instantaneous. The network structure represents an intermediate stage in structural development, when the ferrite or cementite ejected into the grain boundaries of the austenite in cooling through the transformation range has had time to coalesce into visible masses, but before the walls thus formed have had time to break up by spheroidizing or granulating.

The Widmanstättian figuring, or massing of the ferrite in the cleavages of the austenite, arises in like manner in cooling through the transformation range, and like the network structure it probably represents an intermediate stage in structural development (page 310).

Grain refining of steel containing any important proportion of carbon implies a nearly complete effacement of the network structure by diffusion, which cannot be instantaneous. The restrained annealing which follows grain refining may be called fine-grain annealing (pages 317, 319).

The reason why slow cooling coarsens the network is that it increases the total opportunity for austenite grain growth above, and in the upper part of the transformation range; hence if there has been a long high holding this coarsening effect of slow cooling should be slight, because the slow cooling increases the total opportunity for austenite coalescence in but small proportion (page 320).

The reasons why slow cooling may coarsen the ferrite grains are (*a*) that after a very short holding at high temperature it increases in large proportion the opportunity for perfecting the austenite crystalline structure, and through this the massing of the ferrite in cooling through the transformation range;

and (*b*) that it is only in the slow cooling through the lower part of that range and below it that ferrite exists and can coalesce (page 321).

In the case of very-low-carbon steel, the ferrite grains of which seem to inherit directly the grain size of the austenite, slow cooling above the transformation range after a brief high heating lengthens considerably the opportunity for austenite grain growth as explained above; and slow cooling through and below that range further increases the direct opportunity for the ferrite grain growth (page 321).

The effect of manganese I refer primarily to its sluggardizing action (page 322).

In Part III, I enunciate 23 propositions touching the structural changes.

(1) Low-carbon steel coarsens progressively and without limit between 600° and 770° C. (Stead.)

(2) (Provisional). Between A2 and A3, about 770° and 895° C., very-low-carbon steel if initially fine grained coarsens slowly; if initially coarse grained it refines slowly, to coarsen again.

(3) The grain refining of low-carbon steel occurs promptly at or shortly above Ac3; but

(4) That of higher-carbon steel also occurs at or shortly above Ac3, but is much slower, unless the carbon-content is close to that of the eutectoid.

(5) Prolonged holding at any temperature above the transformation range coarsens both ferrite grains and network structure, the coarsening increasing with the temperature, and at least in hyper-eutectoid steel, with time. The network coarsening of medium-carbon steel is less regular and, after long heating as distinguished from high heating, it seems to be masked by the cleavage massings.

(6) (Provisional). The rate of network coarsening increases with the carbon content.

(7) The proportion of coagulated ferrite increases with the slowness of cooling. (Boynton.)

(8) Long high heating retards the coagulation of sorbite into pearlite. (Boynton.)

(9) The network is most marked after cooling at an intermediate rate.

(10) That intermediate rate is more rapid for hypo- than for hyper-eutectoid steel.

(11) (Provisional). The network structure arises only in solidification, or within the transformation range.

(12) The parallel banding of ferrite is favored by long and high heating.

(13) The network structure is made more persistent by long and high heating.

(13) to (19) The presence of manganese retards the formation and the break-up of the network; increases the areas free from coagulated ferrite; and decreases the size (*a*) of the ferrite grains, (*b*) of the lamellæ of pearlite, and (*c*) of the austenite grains formed in Region 4; and the coarseness of the network.

(20) (Provisional). Slow cooling coarsens the network only in the ratio in which it increases the exposure to temperatures above and in the upper part of the transformation range.

(21) (Provisional). Slow cooling increases the ferrite grain size directly, and may increase it indirectly.

(22) (Provisional). The rapidity of ferrite grain growth is inversely as the carbon content.

(23) The rapid coarsening of low-carbon steel occurs between 650° and 750° C.; that of higher-carbon steel, in the upper part of Region 4 above the transformation range.

POSTSCRIPT. Nov. 23, 1911.—Since writing the above I have read a thoughtful, interesting, and very suggestive article on this general subject by M. Ziegler.* Agreeing with him in a general way, I question some of his contentions.

In his view the life history of the structure of steel is dominated chiefly by manganese sulphide (page 667), though slag, iron oxide, and like foreign bodies cooperate. He groups all these as "slag." Between the melting point and a temperature B slightly above Ar3, the austenite is amorphous, but in cooling it crystallizes at B into grains, which eject to their boundaries most of the slag. The ferrite or cementite born in cooling through the transformation range precipitates on these slag envelopes;

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* *Revue de Metallurgie, Memoires*, Vol. 8, 1911, pp. 655–672.

hence the ferrite or cementite network. A given mass of steel, taken as a whole, consists of metallic grains enclosed in slag envelopes, more or less discontinuous (page 663). High heating, in that it coarsens the austenite grains, thickens the slag envelopes and thus weakens the steel (page 663). Overstrain acts through breaking up these envelopes, and annealing effaces the effects of overstrain by re-dissolving these broken envelopes, and welding up the fissures (page 664). The inequiaxing of rolled steel is due to rolling below the temperature at which the slag envelopes form (page 666), and re-equiaxing is brought about by reheating above this temperature, which I take it must be at least as high as Ar1 because below that austenite does not exist and cannot generate slag envelopes. As far as this last comma I give strictly what I understand to be M. Ziegler's own opinions and reasons.

He seems to me to overwork an undoubted contributory cause, to which I have referred in Paragraph 42. I will now give my reasons for holding that this cause is not competent to explain unaided the phenomena which he refers to it.

1. I cannot question that slag, using this word generically as M. Ziegler does, is rejected by the austenite grains; but I see no reason for his contention that this austenite is amorphous from the freezing range down nearly to Ar3. Indeed, this contention raises a needless difficulty in the way of his theory. To fix our ideas let us take his point B as 1000°; any other temperature compatible with his putting it but moderately above Ar3 would do equally well for my present purpose. If the austenite is amorphous above 1000°, then we have no explanation of the progressive further coarsening, both of the fracture and of the ferrite and cementite network, caused by heating to still higher temperatures, and continuing with the temperature reached certainly to 1300° and extremely probably up to the solidus.

2. That slag is necessary to the network coarsening and other effects of high heating seems a needless assumption; for if the austenite grains are able to expel the slag to their borders, they should for the very same reason expel to those borders the ferrite which they generate in cooling through the transformation range. The median position of layers of slag within the middle of the ferrite network is no evidence that the slag network is necessary to this formation of the ferrite network; its median

position is perfectly accounted for by its being ejected into the austenite grain boundaries before the ferrite, that is during the sojourn above the transformation range. The presence of a slag envelope outside a given grain of austenite may assist the particles of ferrite born within that grain to move to its outside, as indicated in Paragraph 42, but that it should be necessary to that movement seems not only in the highest degree improbable but further wholly incompatible with the fact that the network formation goes on in steels very nearly free from slag, and for all we now know quite as rapidly and fully as in those which have much slag. The high-grade steels on which my present experiments were made probably had no large quantity of slag in view of their very low content of silicon and sulphur. Moreover, on the theory that a slag network is necessary to the formation of a ferrite network, unless that slag network were nearly continuous the ferrite network which it is alleged to cause also would be discontinuous, whereas even when that network is extremely thin it is habitually continuous or very nearly so. But the slag network must be very far from continuous, because if it were continuous then the strength of the mass as a whole would be simply the strength of slag, which would evidently form in that case the weak link in the chain. The strength of steel is so incomparably greater than that of any conceivable form of slag, that whatever slag envelopes exist must be very far from continuous. Because of this extreme discontinuity they are incompetent to be the sole cause of the continuous ferrite envelopes. Moreover, if, as he seems to hold, in steel with a not unusually high quantity of slag the slag envelopes have some approach to continuity, then any decrease in the quantity of slag ought to strengthen the mass as a whole enormously; but no such enormous strengthening is caused by decreasing the quantity of slag present, in current manufacture.

Nevertheless, though I question the formative effect of slag, I do not question its impeding effect. Not only do I believe that the network of ferrite or cementite can form without any aid whatsoever from slag, but I question whether slag has an important effect in aiding the birth of that network. But I recognize its effect in impeding the break-up of that network, as indicated in Principle 19, Paragraph 42, page 313, and M.

Ziegler's welcome paper suggests to me that in this impeding action we have the strongest ostensible cause of most of the seven effects of high heating, apart from grain coarsening, which, in Principle 10, Paragraphs 29 to 32, pages 298–304, I referred to strengthening of the crystalline organization. Even the prominence of the Widmansttätian figuring may be looked at as its persistence because of the sluggishness of its particles in breaking ranks and balling up. This sluggishness I referred to perfecting of the crystalline organization; but we may go farther and select one feature of that perfecting, its localizing the slag inclusions in the network and cleavages, as the dominant one. For the very rigidity and inertness of that slag, the feebleness of its tendency to break ranks and ball up, as compared with the corresponding tendency in the mobile ferrite, explain this sluggishness which underlies these seven effects. For instance the ferrite in the network and cleavage massings, if left to itself. would quickly ball up through its surface tension. The persistence of network and cleavage massings in long and highly heated steel explains itself as the result of the adhesion of the ferrite to the small and usually most discontinuous slag particles concentrated in the network and cleavages during the long high heating. In a word, I look at slag not as a necessary or even as a strong provocative of local concentrations, but as an impediment to their break up, not as a cause of their birth but as a prolonger of their life.

3. That the mechanism of overstrain is something much more than the rupture of slag envelopes Ewing and Rosenhain have shown beyond a peradventure.

4. That the recovery from overstrain is not primarily due to the mending of broken slag envelopes, which according to M. Ziegler occurs only above A1, is shown positively by the great recovery below A1.

To explain the recovery as he does by referring it to the re-dissolving of the broken envelopes by the reheating, is unsafe, because such re-dissolving should occur only at a temperature much higher than the relatively low one needed to remove overstrain. Indeed, were we to adopt his idea (page 667) that most of the manganese sulphide separates bodily from the molten mass before solidification, that would certainly suggest that the metal in solidifying was already saturated with manganese sulphide, so that envelopes of that sulphide broken by overstrain could not be fully re-dissolved except by heating close to the melting point. As for true slag, that is, silicates, phosphates, etc., that of course cannot re-dissolve at all.

That inequiaxing and re-equiaxing do not represent rolling below Ar1 and reheating above Ac1, is shown conclusively by the fact that re-equiaxing may begin as low as 400° and complete itself at a temperature as low as 600°.*

M. Ziegler properly points out that the increase of grain size by high heating should lead to increasing the continuity and thickness of the slag envelopes, because, the quantity of slag being fixed, it would go farther towards continuity if concentrated in a relatively few envelopes around a few large kernels than if spread out in a greater number of envelopes about smaller kernels, and to this he refers the injurious effects of high heating (page 663). But it is doubtful if the facts support this; because in a large proportion of cases increase of cell size by high heating, say to 1000° or 1100°, raises the tensile strength, instead of lowering it as is implied by the theory that slag envelopes are the dominating element.

NOTE TO PAGE 278. The support offered to the theory of Ewing and Rosenhain by their observation, that growing grains will not bridge a weld unless it contains eutectic, seems to lose its force on further consideration. On their theory, grain growth occurs through the progressive migration of the eutectic-filled eutectiferous boundary across the grain which is undergoing absorption by its neighbor, through which migration this absorption occurs. With this mental picture before us, we see that certain eutectiferous boundaries in the neighborhood of the weld would, in the natural course of their migration, come to the weld, and would thereby spontaneously set in that weld the eutectic needed for bridging it, whereon grain growth across it ought to occur. Thus one first questions whether their theory really explains the failure of grain growth to cross welds, and then asks whether this failure does not rather throw doub on their theory.

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* Heyn, "Über die Nutzanwendung der Metallographie, etc."; *Stahl und Eisen*, Vol. 26, 1906, pp. 580–596.

APPENDIX I. GENERAL DESCRIPTION OF EXPERIMENTS.

The treatment of the specimens shown in Figs. 18 to 28, Plate I, is described in a note on that plate. The treatment of the specimens described under Proposition 4, Paragraph 52, page 336, by means of which I showed that the refining of the fracture is completed not at Ac1 but at Ac3, is described under Appendix II. The treatment of the specimens by means of which the influence of length and height of heating, etc., on network size was determined, including the specimens represented on Plates II, III, and IV, is given in Table V.

The composition of the steels experimented on is given in Table I, page 332.

Pyrometry.—All temperatures were determined with the Le Chatelier pyrometer, using platinum - platinum-iridium couples. For all measurements for which accuracy was important a couple and Siemens and Halske galvanometer, both calibrated by the United States Bureau of Standards, Washington, D. C., were used. These couples were not in general exposed to any temperature above 1100°, though in a few cases they were exposed to 1200° for 10 minutes at a time. Parts thus exposed were always removed before further use for measurements needing accuracy.

For most measurements at temperatures in excess of 1100° another couple was used, together with a Keiser and Schmidt galvanometer. All couples were re-calibrated at short intervals.

Measurement of Walled-cell Size.—In the eye-piece of the microscope was a micrometer which showed a line $A'B'$, Fig. 8, the image of which, as seen against the specimen, covered a known length. A count of the number of cells cut by this line, and its length, permitted calculating the average sectional area of those cells, on the assumption that they were squares. Then the micrometer line $A'B'$ was traversed to the position $A''B''$, a recount made, and so on, across the field. In this way twelve measurements were made for each of the numbers which give the cell size in Table III. The assumption that the several cells are squares, or whatever like assumption is made, introduces a certain error; but this seems to be of a far lower order of magnitude than the inevitable error due to uncertainty as to what the true cell boundaries are in most sections. Duplicate measurements are generally in good accord, whether made on different sections of the same specimen, or on different specimens which had received like treatment.

In several test measurements the results thus reached agreed well with those reached by Professor Sauveur's method, which consists in measuring with a planimeter the area of a given part of the field, and counting the cells included in that area. I have no preference for the method here used. Its simplicity may make it applicable under certain conditions where Professor Sauveur's is not.

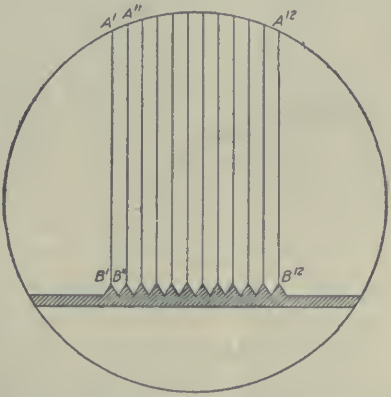

FIG. 8.—Walled-cell-size Measurement.

In the case of the electrolytic iron Professor Sauveur's method was used.

APPENDIX II. METHOD OF FINDING THE RELATION OF THE FRACTURE-REFINING TEMPERATURE TO Ac1 AND Ac3.

The purpose of these experiments was to learn whether the temperature at which the refining of an initially coarse structure, as indicated by the fracture, becomes complete, is Ac1 as asserted by Mr. Brinell, or slightly above Ac3 as our present theory requires. My general procedure was first to coarsen the whole of a bar of steel by overheating it; then to reheat it differentially so that one of its ends passed well above Ac3 while the other remained well below Ac1; then to quench it in water so as to harden those parts which were above Ac1, and thus to learn in what part of the length of the bar Ac1 lay; to examine it microscopically so as to learn where Ac3 lay; and to break it open lengthwise so as to learn whether the refining of the fracture completed itself just at Ac1 or above Ac3 as thus fixed. Under these conditions a marked change in the look of the fracture as distinguished from its coarseness might well occur at Ac1, because the parts cooler than Ac1 would not harden, whereas those hotter would, and of course a marked fracture-change might occur where the unhardened joined the hardened part. This change had to be distinguished from refining proper of the fracture, that is, the change from a coarse to a fine fracture. The carbon content of the steel tested had to be well below the eutectoid ratio in order that there might be a broad margin between Ac1 and Ac3. An even lower carbon content than that selected might have been better.*

A bar 1 in. square and 8 ins. long, of steel of 0.46 per cent. carbon, was first coarsened by holding for one hour at about 1350°, and cooling in air so as to develop the network structure. It was then planed in two along the cut *aaa* shown in Fig. 9, and the slots *cc* were made with a hack saw. One of its two pieces at a time was next heated differentially in a thick-walled copper cast box, Fig. 10, of which the left-hand end lay within a highly heated forge, while the other end projected into the outer air. Because of the high thermal conductivity of the copper the thermal gradient along this bar was extremely flat. When the hotter end of the bar reached about 900°, as indicated by the thermo-junction beside it, the bar was drawn and quenched in water.

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* Though the steel actually used contained 0.46 per cent. of carbon, it was not Steel II of this paper.

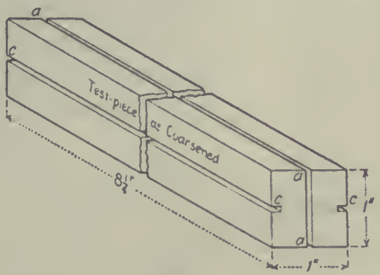

FIG. 9.

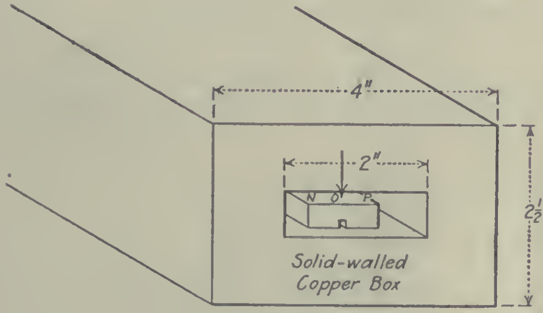

FIG. 10.

FIGS. 9 and 10.—Arrangement for determining the relation between the fracture-refining range and the transformation range, Ac1 to Ac3.

NOTE.—The test piece, as a square bar, is coarsened by overheating, cooled slowly, grooved along *cc*, and cut along *aaa* into two pieces, of which one at a time is refined differentially in the solid-walled copper box of Fig. 10.

Thus the successive points along the length of the bar represent the same steel after treatment which is identical, except that the quenching temperature rises extremely slowly as we pass from right to left of Fig. 10. The bar was next cut into smaller pieces in the unhardened part, and broken into such pieces in the hardened part. Then each of these little sections was set on a swage block in the same position as that shown in Fig. 10, and broken by blows in the direction of the arrow. The position of Ac1 was then determined by means of the Brinell ball test and by etching with picric acid on the face *NOP*, Fig. 10; that of Ac3 microscopically by the vanishing of the coarse ferrite network; and that of the refining of the fracture by ocular inspection. With this procedure the parts examined are shielded from decarburization during the thermal treatment.

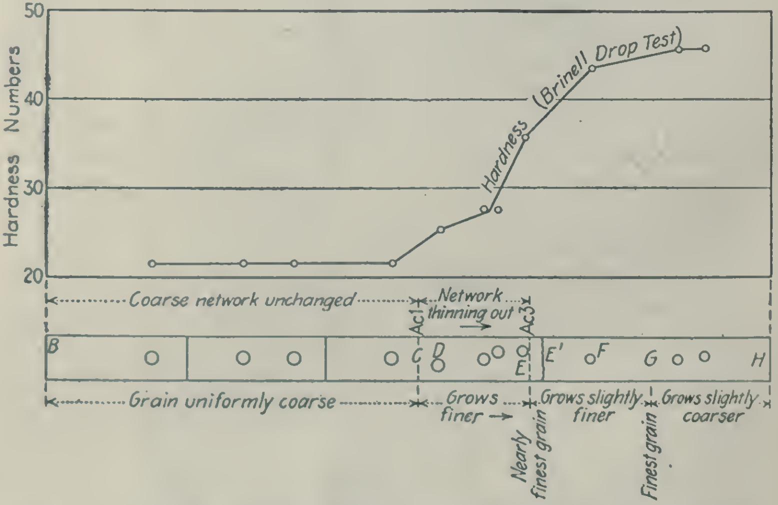

FIG. 11.—Fracture Refining extends through the Transformation Range.

NOTE.—The bar *BH*, coarsened, differentially reheated, and hardened as shown in Figs. 9 and 10, acquires the hardening power and loses its coarse fracture progressively in rising through the transformation range, as identified by the removal of the initial coarse ferrite network.

By heating in this way the region covered by the transformation range was spread out over a distance of 1½ ins. along the length of the bar, as shown in Fig. 11.

The hardness (Brinell), which was constant between *B* and *C*, increased progressively from *C* past *E* to *F*. The grain size in the fracture was practically constant from *B* to *C;* then it grew slightly finer from *C* to *D*, with a more marked change from *D* to *E*, and a further very slight refining up to *G*. Thence to *H* it re-coarsened slightly. The coloration by deep etching with either picric or nitric acid changed sharply at *C*. The cell-walls of the coarse ferrite network faded away gradually from *C* to *E* where they lost all continuity; but with careful etching their fragments could be detected with fair confidence as far as *E′*. Here even these slight remains of the network structure ceased to be ferrite, that is, to appear white after nitric acid etching. But in some cases the network could be traced as far as *F* as a little darker than the kernels which it enclosed.

I interpret this as meaning that as far as *E′* the central layer of the network remained so poor in carbon that, in quenching, it slipped back through the states of troostite and sorbite into that of ferrite plus a trifling quantity of troostite; hence its whiter look. From *E* or *E′* to *F* the central layer of austenite had received so much carbon by diffusion from the enclosed kernel that, in quenching, it was caught in the state of troostite; hence its darker look than the martensite kernels.

This experiment was repeated three times, with substantially identical results. This method is convenient for preliminary reconnaisances.

Another steel with 0.40 per cent. carbon treated in like way gave like results, except that the completion of the refining of the fracture seemed to coincide even more accurately with the vanishing of the coarse ferrite network.

The final disappearance of the coarse network at or slightly above *E* identifies *E* as being very near Ac3; and it was above *E*, and hence at or above Ac3, that the grain-refining of the fracture became complete.

The change in the picric-acid etch color at *C*, and the fact that the hardness began increasing at *C*, show that it was here that the change in carbon condition occurred, in short that *C* represents Ac1. Thus the proof seems complete that in this case fracture refining becomes complete at above Ac3, and far above the temperature at which the carbon condition changes, or Ac1.

That the hardness as measured by the Brinell test should continue increasing from *C* to beyond *E*, as the quenching temperature rose through and beyond the transformation range, is what we should naturally expect from the fact that the higher the quenching temperature the more rapid is the cooling through that range and hence the more thorough the hardening. But if the hardness is determined by cutting with a hack saw, it appears to reach its maximum immediately on passing to the right of *C*. This, too, is but natural, in view of the fact that these saws are not very hard, as is shown by the readiness with which they can be filed. The simple explanation is that at *C* the bar became as hard as the hack saw, which refused to bite at any point harder than *C*, and thus to detect the increase of hardness beyond *C*. It is thus that we should explain the older crude determinations which reported the hardness as leaping to a maximum at ACI.

THE MANUFACTURE OF PURE IRONS IN OPEN-HEARTH FURNACES.

By A. S. Cushman.

The industrial history of the country has been characterized during the last ten years by so many important changes and developments that, taken as a whole, they may well be considered as marking a new industrial epoch. These changes may be defined in part as social and political, and in part as purely technical evolution. Thus we find extraordinary change and activity following in the wake of the great movements for conservation of natural resources and government control of the quality of foods, drugs, and other staples which enter into interstate commerce. The day when a complacent public would buy any material offered for sale, with little or no regard for specification and with implicit faith in every word printed on a label, has probably passed, never to return. Quantity rather than quality has heretofore marked the industrial supremacy of the United States, but the signs of the times point to the fact that hereafter the maximum quantity consistent with a standard of quality will be the slogan of the progressive producer.

Europe has long been looked upon as the home of research and conservation, and it is only comparatively recently that America has been brought to a thorough realization of the necessity of re-ordering her ways if she is to take her place in the up-building of the industrial future of mankind. It is satisfactory to note that the wave of conservation is sweeping along all the lines of industry, and the ruthless waste formerly so common in manufacturing methods is rapidly disappearing. Almost all new countries, especially if bountifully supplied with natural resources, have had this experience. It is the natural result of rapid growth which leads to a disinclination to consider apparently minor details until forced to do so by stern necessity. The movement for the conservation of our forest products, which is comparatively new in this country, is old and well known in Germany, and it is now very generally conceded that this country has taken up the study of the conservation problem none too soon. It is but natural that in this great wave of economy the conservation of iron should have become such an all-absorbing subject. There can be no question but that this important commodity is directly and indirectly the greatest force for civilization that exists.

The production of iron as carried on prior to the invention of the Bessemer and open-hearth processes showed a steady growth, but after the appearance of these important inventions, by which large tonnages could be so rapidly and cheaply produced, the industry grew by leaps and bounds and eventually resulted in the greatest period of railroad development that the world has ever known. During such periods as this the question of durability and lasting quality had of necessity to take second place, and, in fact, the study of the literature would indicate that these matters were not given very serious consideration prior to the present decade.

It is the object of this paper to present to your attention the important step in metallurgical development which has made possible a process for making irons of extraordinary purity on the large scale usual in steel-making operations in the open-hearth furnace. Purity in a commercial product is difficult, if not impossible, to define, and it is necessary in the beginning to emphatically state that although the words "pure irons" in the title have been selected advisedly, there is no intention to go beyond the actual facts. All things are relative, and from this point of view the products which I am about to discuss may be defined as pure only when they are considered in relation to ordinary metallurgical products in which purity from the standpoint of chemical constitution has not been the principal object sought. Iron of one hundred per cent. purity will undoubtedly never be achieved, but if the efforts which have already produced the present commercial irons of such an unusual degree of purity are not relaxed, it is safe to assume that at least commercial purity will be attained.

In order to introduce the discussion, it will be of interest to give very briefly a summary of the various processes by which iron as differentiated from steel has been produced in the past, together with some historical facts and dates, all of which have a direct bearing on the subject.

Iron may be produced in one operation direct from the ore, a practice which was followed in nearly all the earlier methods which we read of in the history of iron manufacture. These direct processes as carried out in early times would be considered far too costly, and the yields too low, to make even an approximate return to them possible. It is not probable that the ancient workers of iron, who used the Catalan process, aimed at extraordinary purity or even knew very much about it. In this respect they were, however, safe-guarded by the comparative impossibility of obtaining high temperatures, so that impurities in the ore were probably never thoroughly reduced and hence passed into the slag instead of melting into the metal product. The following is an analysis of the crude iron and the finished bar as produced at Ragdohar, Salem District, India. The analysis was made by Harris and is quoted by Turner. It shows the possibility of purity when the forging process is conducted with great care.*

	Crude Iron.	Finished Bar.
Carbon	0.660 (Chiefly charcoal)	0.030
Silicon	1.113 (Chiefly slag)	0.010
Sulphur	0.005	Trace
Phosphorus	0.028	0.013
Manganese	0.013	Nil
Iron (by difference)	98.181	99.947
	100.000	100.000

The Catalan process, while of great interest in a historical way, is of course no longer of any commercial importance in the metallurgy of iron. Up to the present era nearly all of the purer irons of commerce have been represented by the various grades of so-called wrought irons. These wrought irons, with scarcely an exception, have been produced by indirect methods, that is, by the purification of pig iron which has been previously produced from the ore in a blast furnace. The methods for producing wrought irons vary considerably in character, but they are all based on the elimination of the impurities in the pig iron by means of oxidation. The oxidation can be carried on in two ways, either on a hearth, with charcoal as fuel, or in a reverberatory furnace. In one case the metal is in actual contact with the fuel, and the oxygen is supplied by the blast, the impurities passing off into the slag. In the second case, that of the reverberatory furnace, the metal does not come in actual contact with the fuel, and the oxygen is supplied by means of oxide of iron in the form of iron ore or mill scale. The furnaces used in carrying on the operations of the first case are known as the Walloon, the Lancashire hearth, the Franche Compti, and the puddling furnace commonly known in Great Britain and the United States.

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* Turner's "Metallurgy of Iron."

The Lancashire hearth and the Franche Compti processes are practically the same, the differences being chiefly in the details of the furnaces employed and a few of the minor operations, none of which, however, change the character of the processes but are principally dependent upon the local custom of the localities where they are employed. In Sweden the Walloon and Lancashire hearth processes are chiefly used, whereas in England and the United States the puddling process is by far the most common. The charcoal process, in the strictest meaning of the term, is being used only to a moderate extent. The great purity of the Swedish ores, and, until comparatively recently, the plentiful supply of charcoal, has stimulated the use of the charcoal process, whereas in England the comparative impurity of the ores and the difficulty in getting charcoal has brought about the adoption of the puddling process.

In 1784 Henry Cort introduced the puddling process into England, where it immediately replaced the charcoal processes to a very considerable extent. As first used a sand bottom was employed and this process eventually became known as "dry puddling." At a later date the sand bottom was replaced by a bottom made of iron oxide, this new development being known as "wet puddling" or the "pig boiling" process. The "pig boiling" process had much greater possibilities, and mechanical puddling which was developed later was applied to the wet method almost to the entire exclusion of "dry puddling." Sweden never used the puddling process to anything like the same extent as England, owing to the abundance of charcoal and the bountiful supply of very pure high-grade ores. These basic facts are undoubtedly the cause of the high reputation that Sweden has always enjoyed in the iron trade of the world. For many years Sweden has shipped large tonnages of charcoal iron to England and the United States for conversion into crucible steel and for other purposes where very high grade irons are required. The term "Swedish charcoal iron" has come to be synonymous with charcoal irons of the very highest quality. It is unfortunately true that the high reputation which has been earned by the best charcoal iron has been made the basis of a masquerade for materials entering the trade, which differ very essentially from the original types of pure charcoal irons, and which in too many cases represent very ordinary materials knobbled up in any possible way so long as they can be made to pass muster as charcoal iron. Unhappily the same thing is true with regard to wrought iron, and material is made to pass as wrought iron, which may be merely ordinary trimmings and scrap which after busheling or fagoting is forged or rolled into the semblance of true wrought iron. Fortunately, the introduction of the microscope as an every-day working instrument in metallurgical laboratories has rendered this form of deception increasingly difficult, and it can safely be said that no consumer need any longer fear a practical difficulty in buying to specification provided he is willing to enlist the assistance of a properly equipped metallurgical laboratory.

Up to 1895 more iron than steel was produced in Sweden, but since that time, as a natural result of the extensive use of the various types of steel as differentiated from iron, their production has decidedly increased, while the output of iron has remained about stationary. The heart of the iron industry in Sweden is in the western central portion, which includes the mines of Dannemora, Norberg, Persberg and the district of Vermland, Orebro, Vestmanland, and Kopparberg. In 1900 about fifty per cent. of all the ore produced in Sweden came from this district. In addition to these deposits, however, it is well known that along the northern borders are vast deposits of high-grade iron ores which have as yet been held mainly in reserve.

The first experiments which eventually led to the development and perfection of the open-hearth process were carried on by Josiah Heath about 1845. A reverberatory furnace was employed, but successful results were not obtained, as it was impossible to get the requisite temperature for conducting the work to a successful product. Siemens began his experiments about 1861, while at the same time Martin was independently working on the same problem in France. In 1867 steel produced by the open-hearth process was exhibited by Siemens and Martin at the Paris exposition, where a gold medal was awarded in recognition of the efforts of these two pioneer investigators. The earliest patent covering the open-hearth process was obtained in France in 1865 and covered practically what is now known as the open-hearth method which effects the decarbonization of iron and its subsequent recarbonization into steel.

The first open-hearth furnace introduced into the United States for the production of steel was built by Frederick J. Slade for Cooper, Hewitt and Company, then proprietors of the New Jersey Steel and Iron Company, at Trenton, N. J. In 1872 the production of open-hearth steel in the United States amounted to about 3,000 tons. Eight years later, in 1880, it was 84,000 tons, about forty furnaces being in operation. In the meanwhile the Bessemer process which, of course, preceded the development of the open-hearth process, had made wonderful strides in Europe and the United States, but it was not until the basic open-hearth process was developed that the real future of the open-hearth was assured. This process, in which lime was employed as the purifying agent, in conjunction with basic linings, made possible the employment of raw materials otherwise comparatively unavailable, and as time went by the furnaces were increased in size, thereby reducing the cost of operation and bringing this process directly into competition with the Bessemer in regard to the cost of production, the quality of the product being early recognized as superior for many purposes to Bessemer steel.

Our late much-beloved president, Dr. Charles B. Dudley, stated some years ago in an address before this Society that there was an ever-present danger that the ease with which the great modern metallurgical methods lent themselves to the manufacture of steel, might very possibly have been leading us away from the consideration of the desirability of iron as opposed to steel for those special purposes for which iron is particularly suited. Dr. Dudley further prophesied, in his usual conservative way, however, that the time might not be so far distant when the pendulum would swing back again in a return to some modern form of practical iron-making. That this same thing was gradually taking shape in the minds of a number of progressive metallurgists is clearly shown by a study of the literature which bears upon this subject. As far back as 1892 Col. H. Dyer read a paper on the production of pure iron and steel before the British Iron and Steel Institute. Dyer's experiments were made in a small way and with generally unsatisfactory results, as the product, while being pure as regards silicon, sulphur, phosphorus, carbon and manganese, was so over-burned and contaminated with oxygen that it was practically unworkable. Many other investigators took up the subject, but the results never got further than the experimental stage. While all these investigations attracted considerable attention from time to time, and produced discussions before technical and learned societies, no practical results were obtained. The thorough elimination of oxygen appeared to be an insurmountable stumbling-block and by most metallurgists the final reduction of an over-burned bath was not considered possible.

About 1903 H. H. Campbell* attempted the production of pure iron in an open-hearth furnace, and, as a result, obtained a product for which the following analysis has been recorded:

Carbon	0.025 per cent.
Phosphorus	0.009 " "
Silicon	0.005 " "
Manganese	0.040 " "
Sulphur	0.024 " "
Copper	0.10 " "

The oxygen content of his product was apparently not known to Campbell, but his opinion in regard to it is very clearly summed up in the following quotation: "These heats were made in a basic open-hearth furnace and their regularity both in chemical and physical character shows that we are dealing with a normal and definite metal and not with an accidental product. They were purposely made with the lowest possible content of manganese, and it seems positively certain that the steel must be saturated with oxygen." Some of this material was examined and reported upon by Henry M. Howe, who made a number of experiments upon it.† If Campbell was close to a possibility of the production of a commercial metal with an iron analysis made in a steel furnace, he does not seem to have been aware of the fact, owing undoubtedly to the difficulty which was then anticipated in any attempt to deoxidize a highly super-oxidized bath. A material saturated with oxygen is necessarily unfit for commercial use, and unless the oxygen can be reduced to a very low content the product is practically worthless.

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* "Manufacture and Properties of Iron and Steel," 1904, p. 482.

† *Engineering and Mining Journal*, Vol. 62, p. 557.

Numerous other references might be given from the literature of the last ten years, to show that many other metallurgists were at least ready to consider the possibilities as well as the difficulties involved in deoxidation work on a large scale.

About 1905 the United States Department of Agriculture received a great number of complaints from farmers and other people in different parts of the United States, in regard to the alleged rapid corrosion and deterioration of fence wire and of steel culverts for road grading. The Department, through the Office of Public Roads, undertook an investigation of this subject to see if it could be determined whether the claims made in regard to the superior rust-resisting quality of the older irons were justified by the facts, and also to see whether influence could be brought to bear upon American manufacturers to attempt the improvement of their products from the special standpoint of rust-resistance and durability. The writer was put in charge of these investigations, and on November 13, 1905, published a preliminary report in the form of a Farmers' Bulletin on The Corrosion of Fence Wire. In May, 1905, the manufacture of wrought iron and the relative resistance to corrosion of wrought iron and steel, was made the subject of a special debate before the meeting of American Institute of Mining Engineers which was held that year in Washington, D. C. The discussion followed a paper read by Mr. J. P. Rowe, in which he described his new method of mechanical puddling. The consensus of opinion of a number of leading American metallurgists who took part in the discussion was that chemical constitution, purity, homogeneity, and lack of segregation were the important points bearing on relative corrosion resistance. In Bulletin 239, on The Corrosion of Fence Wire, the writer expressed the opinion that whereas the tendency of iron and steel to rust or oxidize is a characteristic of the metal itself, independent of the presence of any impurities that it might contain, nevertheless not only the rate but the type or kind of corrosion which took place might easily be dependent upon the chemical and physical characteristics of the metal. It was also pointed out in the same bulletin that a careful search of the literature of the subject, as well as the results of a number of experiments which had been carried out by the writer and others, appeared to point to manganese as having something to do with the matter. Although manganese by itself as an impurity in iron and steel is known not to segregate to the same degree as some other impurities, nevertheless when it is present together with sulphur there is abundant evidence which points to the fact that manganese sulphide segregates and leads to undesirable results from several different points of view. It was interesting to note in these early days of the investigation, as shown by the results of questions which were propounded to the general managers of a number of companies which represented the largest consumers of wire, that practical men had already reached the conclusion that for some reason manganese was involved in the question of rust resistance.

In answer to questions sent out by the Department of Agriculture, the following opinions were expressed: (1) That Bessemer or mild steel wire will rust or deteriorate much more rapidly than iron wire; (2) that within certain limits, the more manganese there is present, the shorter will be the life of the metal. It is possible, of course, that even at the present time there are many metallurgists well qualified by knowledge and experience to discuss this subject, who will not agree with these conclusions. That they represent, however, the opinion of men well qualified to form an opinion, there can be no question; and, in fact, a careful review of the literature of the subject from the very beginning discloses the fact that a large number of authorities, including those of the British Admiralty and many noted investigators, have united in saying that within reasonable limits, other things being equal, the higher the percentage of impurities subject to segregation as the metal cools from a liquid to a solid condition, the greater the tendency to corrosion, especially when subjected to severe conditions. Greenwood* states that manganiferous steel is more corroded by sea-water than the less manganiferous metal. At the time that Bulletin 239 was written, the author was impressed by the fact that in spite of the great abundance of opinion to be found in the literature in regard to the influence of manganese and other impurities on the resistance of iron and steel to corrosion, there was very little actual experimental evidence which could be cited in support of such a conclusion. It seemed quite certain that different samples of steels or of iron respectively might differ quite as much among themselves in resistance to corrosion under service conditions as they did from each other, and, in fact, it is impossible to read the literature of the subject, covering the time in question, without finding eminent authorities ranged against each other in their opinions and discussions on this all-important topic.

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* Greenwood, *Steel and Iron*, 1896, p. 388.

A number of investigators independently entered the field, and shortly after this time the electrolytic theory of corrosion was advanced and developed. That the electrolytic theory is now accepted, by the large majority of authorities both in this country and in Europe, as having left the realm of pure theory for that of fact, is shown by numerous very recent contributions to this subject. In the early days of these investigations experimenters found it extremely difficult to obtain samples or specimens of steel which were to any extent free from manganese, although, of course, many examples of charcoal and puddled wrought iron of a high degree of purity in this respect could be obtained. About this time, however, a prominent American manufacturer, struck by the voluminous literature which had been appearing on both sides of the controversy, determined to make the effort to produce open-hearth metal of the highest possible commercial purity, which would be not only as free as possible from manganese but also from the other four chief impurities which have in the past been taken special note of in the manufacture of steel. This decision on the part of the manufacturer took the experimental work for the first time from a purely laboratory onto a large mill scale of operation. As this experimental work proceeded, the material at first produced was simply a low-manganese mild steel; and while the movement was decidedly in the right direction it was only a step forward and the product obtained was not very essentially different from mild steels that had been produced elsewhere from time to time. When, however, in following up this work the attempt was made to eliminate the manganese and carbon completely from the metal, the resulting ingots were of such an oxidized character as to be entirely unsatisfactory and to lead to most serious troubles in the attempt to roll and manufacture it into a final product. In fact, material made at this stage of the progress of the investigations was not pure iron and was of little utility. It soon became apparent that a very large amount of expensive investigation would be necessary and that considerable time would elapse before the experiments should be expected, even if successful in the end, to produce a material of a good market value which could be truly called iron. The writer is pleased to have the opportunity to emphatically record his appreciation and admiration for the determined and successful efforts to bring about this metallurgical improvement in the face of possible financial failure as well as in the face of much existing scientific and expert opinion that the problem was an insoluble one or at best, one not worth solving.

It has been the usual practice in the past to apply the term iron when used in its specific sense only to the products of either the charcoal knobbling fire or the puddling furnace, although, as has been stated, the product of the busheling or fagoting method of production has also yielded material which has managed to masquerade as iron. In conjunction with the results of chemical analysis, the microscope has recently been developed as a means of enabling the metallurgist to easily distinguish iron from steel. These methods of investigation have gradually led to considering iron to be a name applicable only to products which show slag inclusions under the microscope. Steel, from the very nature of its manufacture, does not exhibit this peculiarity, and it is not surprising, therefore, that the mere presence or absence of slag should gradually have come to be held as the distinguishing characteristic between steel and iron. The practical production of a new form of the metal, by a process in which the product was finished in the molten condition and cast into molds, necessarily yielding a slagless body, has reopened the question in some quarters as to whether or not the claim is justified that these pure forms of metal should be called iron and not steel. From the purely scientific standpoint, when we use the word iron, we must inevitably refer to a product which in all of its characteristics essentially corresponds to the element itself. It was, of course, impossible to decide just where the border line between an iron and a steel should lie, until the results of metallographic investigations which have been made within the last few years had taught us the essential difference between the pure ferrite structure and the carbon-iron steel. From the modern standpoint, neither the presence nor absence of slag could be held to settle the matter of nomenclature, except that the presence of slag inevitably points to the process by which the material was prepared. In the opinion of the writer, it is difficult to understand how a product which is in every way essentially metallic iron and exhibiting a true ferrite structure, could be defined as steel after the carbon had been eliminated to practical traces irrespective of whether the metal is found to contain inclusions of slag. In fact, a metal which contains 3 per cent. or more of a foreign body such as slag falls just that far short of being pure iron, and from a perfectly reasonable standpoint it would appear that after the carbon-iron alloy field has been eliminated, the nearer the metal approaches to the theoretical constitution of the element itself, the more it justifies the name of iron.

. This pure iron made in an open-hearth furnace has become generally known in this country and to a certain extent in Europe as "Ingot Iron." This term, as we are well aware, originated many years ago and was applied chiefly to the lower carbon product of the Bessemer converter and the open-hearth furnace. Of late years, however, it has fallen into disuse and from the standpoint of the steel maker it was undoubtedly a misnomer, so that gradually the term mild or soft steel took its place. Low-carbon Bessemer or open-hearth steel which usually carries from 0.3 to 0.5 per cent. of manganese or over is not iron, for the simple reason that it does not show the true ferrite structure and contains enough carbon to provide it with mild hardening qualities. Besides these facts, mild steels possess many qualities not possessed by iron and may be used for purposes for which iron would be unsuitable, although this applies more especially to the slag-bearing iron as made by the original processes. On the other hand, a pure slagless iron is probably superior for many other purposes to the general run of carbon-bearing metals. It would appear that the term "ingot iron" is especially suitable to a pure iron product made in an open-hearth furnace and cast while molten into ingot form.

As is the case with all new developments, mistakes have been made from time to time, and it is perhaps not too much to say that, especially in the earlier stages of the work, material has been produced and used, which was unsatisfactory and which was certainly not to the credit of the new industry. Many difficulties had to be overcome, but as the industry progressed the causes of trouble became more thoroughly understood and were gradually eliminated. Among the difficulties that have been encountered, I must refer to the fact that practically all the scrap available in this country carries copper varying from very small quantities to amounts in some cases exceeding one per cent. A great many conflicting statements in regard to the influence of copper upon iron have appeared in the literature, but in most cases it appears to have been considered an undesirable element, imparting the property of red shortness to iron or steel. When present in considerable quantities, this is unquestionably the case, and when accompanied at the same time with sulphur the effect is highly intensified. In very small percentages copper does not seem to appreciably affect the working or rolling of the product. Quite early in the investigation with which the writer has been associated, it was found that certain quantities of copper when added to steel would very appreciably retard the solution of the steel in dilute mineral acids. This was also found to be true, though to a much less extent, with the carbon- and manganese-free product of the open-hearth furnace, for the resistance to acid attack is to a large extent influenced by the purity of the metal. In fact, the presence of oxygen in the metal has been found to be more responsible than that of any other element as far as the much-discussed question of acid resistance is concerned. In the writer's experience and practice, the question of copper in iron has been given very careful attention; a wide range of heats have been made, which have purposely been given increasing quantities of copper up to as high as 1.5 per cent. These iron-copper alloys were of necessity made on the usual large scale of operation, but no particular difficulties were encountered in rolling or finishing them. It should not, however, be understood that the intention has been to have copper present in these purer forms of open-hearth irons, for, on the contrary, even though it should appear that the presence of copper within reasonable limits is no detriment to the product, the effort would still be strenuously made to reduce the percentage of copper to the lowest possible point.

In this respect there is one phase which is most interesting to the writer, and to which he would like to give expression. Practically all the steel produced in this country, and therefore most of the scrap available for melting purposes, carries varying percentages of copper. In the making of pure irons in open-hearth furnaces, since the process has no eliminating power on copper or metals of this class, no de-copperization is possible in the refining process. The percentage present must almost inevitably depend upon the amount which is usually found in the pig iron and steel of commerce. If the selling price of the product was commensurate with the expense attendant on securing metal entirely free from copper, there is no reason why such irons could not be readily produced, but it would immediately render useless for the purpose much of the pig iron and most of the scrap at present available. The conservation of our natural resources demands the use of scrap and even if this were not already the customary practice among manufacturers, progressive development should insist on its observance, thereby conserving to that extent our valuable ore deposits.

The manufacture of pure iron in open-hearth furnaces in its entirety is undoubtedly a distinctly new development, in spite of the fact that the process has a great many points in common with the open-hearth production of mild steel. In the case of the iron the oxidizing operation under highly basic conditions is carried forward to an abnormal extent. It is not too much to say that the effort is made to do what has generally been conceded to be bad practice in an open-hearth furnace, that is to say, the metal is deliberately over-burned. This treatment yields a bath of metal and slag heavily charged with gas and in a highly super-oxidized condition. The elimination of the carbon and manganese to such low percentages that they amount to no more than traces makes this super-oxidation a necessary step in the process. Having obtained the desired purity with respect to these elements the bath is given a heroic treatment,* whereby a thorough deoxidation takes place, the gas being at the same time satisfactorily eliminated. No difficulties of any importance are encountered in the pouring of the metal into the ingot molds. It would naturally be expected that such treatment as described might yield a wild metal difficult to handle in the pouring, but this is not the case if the work has been satisfactorily performed. As a matter of fact, the resulting ingots of this process not only possess great purity but are as free from oxygen and gas as any ingots produced from well-made mild steel. If the oxygen and gases are not properly eliminated the quality of the metal is very materially impaired, and it is not worthy of consideration as a commercial possibility, being decidedly inferior to first-class steel.

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* This deoxidation is mainly accomplished in the furnace itself by the proper addition of high-silicon pig iron, aluminum being used in the ladle merely as a "finisher."

In the earlier stages of the development of this process the elimination of oxygen was not understood, nor was it under proper control, and therefore very serious difficulties were encountered in the rolling mill and at other later stages of manufacture. As now practiced, the process yields a metal capable of giving most excellent results in the rolling mill and at all other stages of manufacture. The question of heat treatment was early found to present a most important problem, as the heat treatment required for ingot iron is decidedly different from that for mild steel. Very pure irons appear to possess a critical temperature much more marked and of a wider range than steel. The critical temperatures of very mild steels and pure irons, as well as the embrittling effect of improper heat treatment, has been studied from the theoretical standpoint in England especially by Mr. J. E. Stead.* The well-known tendency of the ferrite structure to produce a condition of orientation of the crystals within a certain critical temperature zone, leading to the probable formation of cleavage planes and fracture lines, has been discussed over and over again, and numerous papers and discussions of the phenomenon may be found in the Journals and Proceedings of The Iron and Steel Institute. The factor of safety in working this pure iron depends upon keeping either above or below the critical zone. A high temperature that would in many cases be ruinous to mild steel gives excellent results with ingot iron in the rolling mill, while a temperature much lower than that which is employed in the case of steels also yields satisfactory results. Between these two extremes, however, lies a critical temperature zone that must be carefully avoided. Investigations to properly define the boundary lines of this important zone are now being made. When these investigations are completed, it will be a comparatively easy matter to avoid the danger point. If, however, this danger zone has been encroached upon in the working, it has been found that a subsequent annealing at a higher temperature is beneficial to a very marked degree. This finding is in agreement with the work of Stead, who found on working with very mild steels that the embrittling effect could be overcome by an annealing process in which the temperature was carried as high as 960° C. It is impossible at this time to give the actual limits of the critical temperature for this metal, for not only is further investigation necessary but the critical temperature will be found to vary within certain limits according to the particular conditions which are encountered.

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* *The Metallographist*, Vols. 1 and 2.

The manufacture of pure iron in open-hearth furnaces when the process is properly carried out presents serious but not insurmountable difficulties in the furnace, the pouring, and the working. It is necessarily a more expensive process than that of soft-steel manufacture, owing to the necessary prolongation of the heat and the higher temperature required during the latter stage of the operation. For these reasons, labor, furnace repairs, ladle linings, fuel charges, and the item of metallurgical waste must all be necessarily in excess of those encountered in the making of soft steel. The process, however, holds forth the same tonnage possibilities as in the making of mild steel, and it permits of the making of iron on a basis comparable to the high-pressure methods employed in modern steel manufacture. It also, in contradistinction to the previous methods of iron manufacture, permits of the making of iron in very large masses, and the subsequent rolling of the same in a manner similar to that of mild steel. It is a well-known fact that the greater the amount of work that is given to iron or steel, the more superior will be the resulting product. This permits the making of commercial products out of iron that have been customarily made of steel owing to the steel's superior workability when compared with iron made by the earlier methods. Such a method for the production of iron, with practically the same equipment as is used in the production of steel, and with substantially the same tonnage possibilities, coupled with a cost not seriously greater than that of steel, certainly seems most desirable and it has, with good reason, been favorably received by the consumers of this country.

Up to the present time ingot iron has not been manufactured into all the products that are customarily made of steel, and of charcoal and puddled iron, but it has been successfully and easily rolled into billets, slabs, plates, sheets, merchant iron, wire, nails, rivets, pipe, and boiler tubes. A wide variety of forgings have also been produced. The work done so far would indicate that there is no reason why it should not have a wide and rapidly increasing use. It is not a "cure for all diseases," as is so commonly claimed for a new material, and unquestionably time and experience will relegate it to its proper sphere of usefulness. In some cases a high-grade product is unnecessary and of no special advantage, and in such cases its employment is a useless expense. In many cases ingot iron will be decidedly preferable to steel, in other cases not quite suitable, and in still other cases charcoal or puddled iron may be preferable to either steel or ingot iron. Time and investigation will throw more light on the subject, but in the meanwhile it presents a splendid and most interesting field for the student of iron.

The following are a few analyses of normal ingot iron as it is being produced to-day, all of which serve to show the possibilities of this process in the production of pure iron. The analyses are extremely accurate, the carbon and the oxygen being determined by combustion with the special care necessary in dealing with the determination of such minute quantities:

TABLE I.—ANALYSES OF NORMAL INGOT IRON.

	(1)	(2)	(3)	(4)	(5)	(6)
Silicon.......	0.003	0.002	0.005	0.004	0.006	0.003
Sulphur.....	0.014	0.015	0.019	0.017	0.018	0.014
Phosphorus..	0.002	0.001	0.005	0.004	0.003	0.003
Carbon......	0.009	0.011	0.015	0.02	0.016	0.008
Manganese...	0.012	0.015	Trace	0.02	0.015	0.025
Oxygen.....	0.024	0.020	0.021	0.016	0.022	0.019
Copper......	0.06	0.07	0.05	0.08	0.04	0.02
Aluminum...	0.005	0.011	0.012	0.010	0.013	Trace
Nitrogen....	0.006	0.004	0.005	0.003	0.005	0.007
Hydrogen...	?	?	?	?	?	?

In order to simplify the matter of following the normal analysis of ingot iron on successive heats, the writer suggested the adoption of a system which may or may not be new. A record of the fluctuations in analysis of the various elements encountered is plotted out so as to cover the entire number of heats for a given period. The curves as produced are known as normalizing curves, and any departure from the normal practice is at once clearly shown. A number of the typical normalizing curves are shown in Fig. 1.

Considerable attention has been paid to the physical properties of this new iron, and comparisons between it and steel and the purer forms of wrought iron have been made. In tensile strength it closely approximates high-grade well-rolled charcoal iron, and in elongation and reduction of area it not only equals but in many cases surpasses the finest soft steel. It is a well-known fact that iron is usually inferior to steel in elongation and reduction of area. Owing to its more steel-like qualities in this respect, therefore, pure open-hearth iron becomes available for deep drawing and stamping purposes where very often ordinary wrought iron is unavailable and steel must usually be employed. The pure ingot

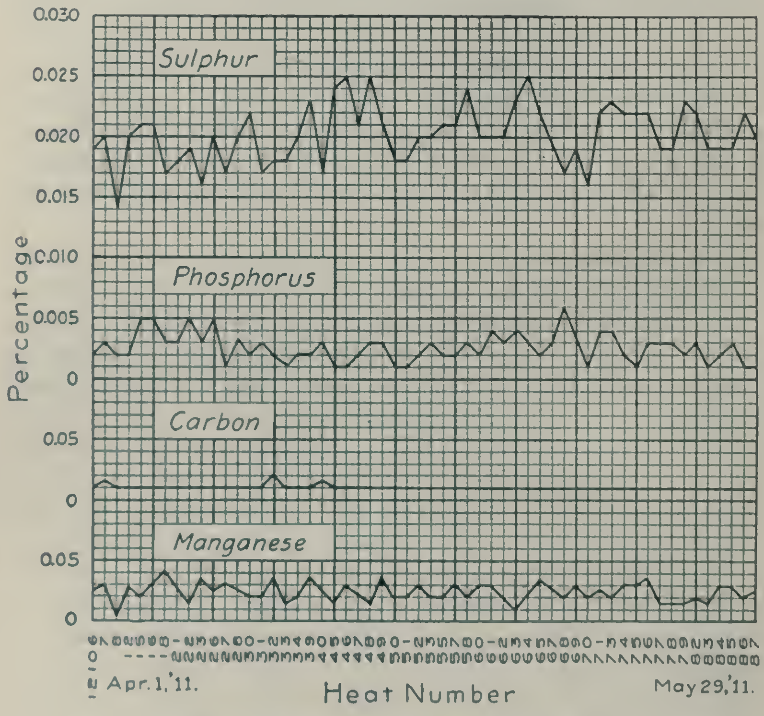

FIG. 1.—Typical Normalizing Curves.

irons, while perhaps not equal to mild steel under certain strenuous conditions, in most cases proved themselves to be adaptable. In fact, it is claimed for them—and reasonably so, it seems to the writer—that they appear to combine to a high degree the good qualities of both iron and steel, with a comparative absence of at least some of the undesirable qualities of each. The annealing, as well also as the temperature at which the metal is delivered from the rolls, the rapidity of cooling, will all, of course, just as in the case of steel, exert a marked influence on the tensile strength, reduction of area, and elongation. In brief, it is safe to state that the tensile strength lies between that of iron and mild steel, the reduction of area being usually higher than mild steel, and the elongation about the same as mild steel. Tables II and III give a number of typical results of physical tests on ingot iron.

A fairly wide range of variation of elongation and reduction of area is found to be dependent upon minor variations in the deoxidizing and purification treatment. Following the disappearance from the metal of the five ordinary impurities usually taken account of, the temperature at the completion of rolling, together with the rapidity of cooling, exerts a marked effect. A number of peculiarities have thus been noted which are not as yet thoroughly understood, but on which much investigation is being done that will later prove of decided value in modifying the material, the chemical analysis remaining the same or being made better.

The subject of the corrosion of iron and steel is receiving so much attention at the present time that the effort has been made not to discuss the problem particularly in this paper. The fact remains, however, that the elimination of the metallic, metalloidal, and gaseous impurities in iron can be made, as is shown by the production of these pure irons in open-hearth furnaces, contributory to a high degree of excellence and homogeneity. It would seem to follow as a matter of course that the elimination of the well-known impurities which are likely to segregate will minimize the tendency toward the production of material lacking in evenness of texture and homogeneity. The fact that very pure irons show a high resistance to the acid test has very naturally been used as an argument in their favor. The consensus of opinion among investigators of this subject seems to be that the acid test should not be used as a true measure of corrosion resistance under

TABLE II.—RESULTS OF MECHANICAL TESTS ON INGOT IRON IN FORM OF PLATES OF VARYING THICKNESSES, SHEET-BARS, AND SHEETS. MATERIAL MADE AT VARIOUS TIMES IN 1910 AND 1911.

Sheet No. 1. Mark.	Thickness, ins.	Area, sq. ins.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 8 ins., per cent.	Reduction of Area, per cent.
E-An. Bars.......	0.514	1.278	22,260	41,500	31.5	71.2
F-An. "	0.514	1.278	22,200	41,160	33.3	71.2
A-U.A. "	0.508	1.263	27,870	51,780	26.0	58.0
F-U.A. "	0.508	1.262	28,120	52,300	25.8	56.5
C-6 Heat......... 3,163 Bars....	0.347	0.782	32,290	55,550	22.4	54.0
Heat.						
3,498-L Bars.....	0.230	0.3461	34,380	46,580	17.25	73.6
3,498-P "	0.254	0.3823	26,420	43,790	32.25	69.9
3,499-L "	0.243	0.3657	31,720	44,160	26.75	67.7
3,499-T "	0.257	0.3863	24,340	42,460	28.75	49.8
3,499-L "	0.490	0.7374	25,290	41,910	34.75	75.2
3,499-T "	0.493	0.7444	24,380	41,740	36.50	67.8
4,556-B U.A. Bars	0.509	1.2650	28,056	50,040	31.20	57.9
4,556-D U.A. "	0.507	1.2570	28,737	50,450	30.40	59.0
4,556-A An. "	0.510	1.2650	24,735	41,800	33.90	72.4
4,556-D An. "	0.512	1.2700	25,277	41,640	37.80	72.55
....	....	0.7153	37,890	56,280	21.50	75.7
....	....	0.6754	38,380	55,720	24.13	79.67
3,331-1 Bars......	0.960	0.9187	21,980	41,700	34.50	77.7
3,331-2 "	0.953	0.9139	37,930	59,610	21.60	65.3
11,014 Bars......	0.785	1.178	34,950	51,510	24.8	66.7
11,037 "	0.440	0.657	25,150	44,410	33.6	78.2
11,027 "	0.731	1.970	31,800	55,250	17.7	59.0
12,005 "	0.770	1.150	38,710	63,400	20.0	50.1
13,018 "	0.467	0.700	35,500	62,350	18.6	58.5
11,079 "	0.587	0.879	24,650	44,650	29.6	59.4
13,047 "	0.694	1.04	22,110	54,900	29.2	52.2
4,616 "	0.692	1.03	22,200	44,410	38.8	73.8
12,055 "	0.593	0.884	23,900	44,100	29.7	72.3
12,054 "	0.769	1.15	26,750	48,400	21.0	55.8
11,088 "	0.568	0.851	36,530	65,670	20.7	61.0
13,112 "	0.868	1.30	24,750	42,450	35.8	77.8
11,101 "	0.456	0.679	34,500	50,600	25.7	58.1
11,094 "	0.698	1.05	22,550	46,200	31.2	65.1
11,100 "	0.530	0.795	23,150	62,020	21.0	74.5
12,066 "	0.750	1.13	22,700	41,050	37.0	70.06
12,067 "	0.989	1.48	34,430	42,400	37.2	72.7
13,103 "	0.516	0.775	21,900	43,900	32.0	76.9

service conditions. While much can be said in support of this point of view and while the writer does not believe that the acid test should be made the sole basis for specification, he has not the slightest hesitancy in recording the fact *that other things being equal* he would advise the purchase of material which showed well in the acid test, provided the prime consideration under the conditions of service was durability and longevity of the material.

As in all other new movements, the making of pure irons in open-hearth furnaces has developed unexpected difficulties. Un-

TABLE III.—RESULTS OF MECHANICAL TESTS ON INGOT-IRON WIRE, FEBRUARY 7–20, 1911, COMPARED WITH FIRST-CLASS SOFT STEEL.

Test No.	Character of Material.	Gauge No.	Thickness, ins.	Area of Cross Section, sq. ins.	Length of Test Piece, ins.	Ultimate Load, lbs.	Ultimate Strength, lbs. per sq. in.	Elongation	
								Ins.	Per cent.
	INGOT IRON.								
1	I. Unannealed Wire, Ingot Iron.	9	0.140	0.0154	13	1,442	93,600	0.39	3.0
2		9	0.141	0.0156	13.75	1,413	90,600	0.19	1.4
3		9	0.144	0.0163	12	1,372	84,200	0.24	2.0
		Average	0.142	0.0158	13	1,409	89,500	0.27	2.1
4		11	0.115	0.0104	13.75	1,008	96,900	0.28	2.0
5		11	0.113	0.0100	13.75	1,005	100,500	0.24	1.7
6		11	0.119	0.0111	10.5	868	78,200	0.13	1.2
7		11	0.118	0.0109	12	1,040	95,400	0.21	1.8
		Average	0.116	0.0106	12.5	980	92,800	0.22	1.7
8	II. Annealed and Galvanized Wire, Ingot Iron. (Wiped coating, same as regular practice.)	9	0.146	0.0167	12	960	57,500	1.43	11.9
9		9	0.146	0.0167	12	995	59,600	1.61	13.4
10		9	0.146	0.0167	12	982	58,800	1.64	13.7
11		9	0.143	0.0161	12	985	61,200	1.73	14.4
12		9	0.143	0.0161	12	975	60,600	1.24	10.3
13		9	0.143	0.0161	12	1,062	66,000	1.20	10.0
14		9	0.143	0.0161	12	1,050	65,200	1.33	11.1
		Average	0.144	0.0163	12	1,001	61,300	1.45	12.1
15	III. Annealed and Galvanized Wire, Ingot Iron. (Unwiped coating, similar to old-fashioned English practice.)	11	0.120	0.0113	12	715	63,300	1.43	11.9
16		11	0.120	0.0113	12	662	58,600	1.44	12.0
17		11	0.120	0.0113	12	740	65,500	0.97	8.1
18		11	0.118	0.0109	12	705	64,700	1.29	10.8
19		11	0.120	0.0113	12	620	54,900	1.80	15.0
20		11	0.119	0.0111	12	590	53,100	1.32	11.0
		Average	0.120	0.0112	12	672	60,000	1.38	11.5
	STEEL.								
1	I. Unannealed Wire, Steel.	9	0.144	0.0163	12.25	1,467	90,000	0.34	2.8
2		9	0.144	0.0163	12.75	1,495	91,700	0.30	2.4
3		9	0.144	0.0163	12	1,525	93,600	0.16	1.3
		Average	0.144	0.0163	12.33	1,496	91,800	0.27	2.2
4		11	0.116	0.0106	11.75	1,183	111,600	0.29	2.5
5		11	0.115	0.0104	12	1,150	110,600	0.22	1.8
6		11	0.122	0.0117	12	1,250	106,800	0.30	2.5
7		11	0.121	0.0115	12	1,220	106,100	0.23	1.9
		Average	0.119	0.0111	12	1,200	108,800	0.26	2.2
8	II. Annealed and Galvanized Wire, Steel. (Wiped coating, regular practice.)	9	0.146	0.0167	12	1,055	63,200	2.25	18.8
9		9	0.146	0.0167	12	1,060	53,500	2.10	17.5
10		9	0.141	0.0156	12	1,050	67,300	1.35	11.3
11		9	0.141	0.0156	12	1,130	72,400	1.57	13.1
12		9	0.141	0.0156	12	1,125	72,100	1.04	8.7
13		9	0.141	0.0156	12	1,035	66,300	2.03	16.9
		Average	0.143	0.0160	12	1,076	65,800	1.72	14.4

doubtedly many mistakes have been made as this work has progressed, but these have not been made with anything but the most honest intention to profit by them; and the belief that was strongly held from the very beginning was justified, that eventually order would come out of chaos and when reasonable perfection had been obtained a valuable commercial material would be the result.

After reviewing all the evidence available, both favorable and unfavorable, the writer is willing to affirm to-day that his belief in pure iron as a valuable article of commerce is stronger than it ever was before. He believes when all prejudices and interests have been left aside, and when the truth which always has and always must prevail is appreciated, that this new metallurgical movement will be recognized as thoroughly in keeping with and as a necessary part of the greater movement for the conservation of our natural resources.

DISCUSSION.

The President.

THE PRESIDENT.—We have had a very interesting paper on a very interesting product. It seems to me that Mr. Cushman is a little over-modest. I knew of Mr. Campbell's efforts to make this very-low-carbon steel, which were it seemed to me wholly successful, and also Mr. McMurtry's who made steel without manganese. The reason why they were not commercially successful was that Mr. Cushman had not yet appeared on the horizon. Mr. Cushman and Mr. Walker came along—I will not discriminate, I couple the two names together as having played a very important part in proving the great value of having as nearly pure iron as possible. I will not attempt to discriminate between them. If these two gentlemen had appeared upon the scene before our beloved friend Campbell was stricken down, I think that he would have gone on with that manufacture. He had it almost in his hand, and he would have gone on with it. I have believed that the only reason why Mr. Campbell did not go on and make that iron by the tens of thousands of tons was because he could not sell it; and he could not sell it because Mr. Cushman and Mr. Walker had not yet shown that it was a valuable product.

So I welcome this evidence of modesty on the part of a gentleman who can well afford to be modest.

Now as to this name "ingot iron" and steel, I think we want to go back quite a little. In the first place there were two kinds of iron, one of which could only be cast, and therefore was called cast iron. It could not be wrought, and so was not called wrought iron. There was another kind that could be wrought, but could not be cast, and it was therefore called wrought iron. Then there came a third kind which was neither the one nor the other, but a distinct thing which could be hardened, and was called "steel." These three classes of iron existed until Henry Bessemer and a few others came along, who for reasons which we need not go into, said that this was a fallacious classification, that it is perfect folly to say that steel is

The President. steel because it will harden; and they said that this very valuable kind of iron which is cast, which is malleable when cast, or is cast when malleable into a malleable steel ingot, was not steel because it had more carbon, or because it had less carbon, but because it was cast as a malleable ingot. There was vigorous resistance to that proposition; but more than a quarter of a century ago that resistance stopped, and it was then agreed universally and accepted by everybody, that that which is cast in a malleable mass, initially malleable, is steel, whether it has much carbon, or has little carbon, or no carbon. Now that is accepted. The only confusion that existed up to a year or so ago was this: Is blister steel, steel, or is it not steel? There was the anomaly that we had low-carbon steel which might contain 0.03 or 0.04 per cent. of carbon and could not harden to any appreciable degree. That was certainly steel. Then we had blister steel which was full of slag and could harden; and those two products had nothing in common except that they were made out of iron and were called steel. They had the name of steel in common, without any other thing in common. The question was, should we call them both steel? It seemed a great pity to call them both steel, and yet there did not seem to be any way out of it, until a few years ago a very important committee was appointed by our mother Society, which did take the matter up and has corresponded with authorities all over the world. There is good reason to hope that it will remove this anomaly.

Now I have a great veneration for Mr. Cushman's judgment; but I do hope that he will revise and change his attitude in this matter. We do not want to have our nomenclature all torn up again. It would be a source of great confusion, and I do not see what good it is going to do. It is true that this class of low-carbon steel which Mr. Cushman calls ingot iron came in, and Mr. Cushman says that the name "soft steel" gradually displaced it. That is not quite accurate. You cannot displace a thing which never existed. The name "ingot iron" never came into use. There was a concern in South Staffordshire that called itself the South Staffordshire Steel Ingot Iron Company; but with the exception of a few sporadic cases like that the term never came into use and therefore has not been displaced by the name "mild steel." The name "mild steel" came into use and has

The President.

stayed. Nobody has objected to it, and I hope that nobody will object, for it is a good name. We have a classification, and I do not see why we should disturb that classification. I earnestly hope that Mr. Cushman, than whom nobody can help us more, will take that point of view: that when we have a good classification, which after endless struggles, trials and disputes has finally been agreed upon by all hands, no one will come in and disturb it by saying that although steel from 2 per cent. carbon down to 0.03 per cent. carbon is all steel, yet when you take out one or two of those hundredths, or even thousandths, it is no longer steel, but ingot iron. If we do that you see what we are going to have. This so-called ingot iron is a very important product. I do not mean to say a word against the product, or against the people who have introduced it. I understand that it has been done with great enterprise, and I do not wish to say a word against it. It is a matter purely of terms. And here I want to say that quotations which Mr. Cushman made from Dr. Dudley about iron do not refer to lowness in carbon, but refer to the presence of slag, as in puddled iron, charcoal iron, and those things, which are a class by themselves because of their slag. When Dr. Dudley commended that iron he was not referring to the low-carbon iron, he was referring to the fact that it contained a lot of slag and possessed the particular qualities, the particular benefits which arise from the presence of that slag.

If I am not very much mistaken—and as to that Mr. Hibbard can tell us I think—the super-oxidation has run about as far in the electrical furnaces as in the American Rolling Mill practice. Super-oxidation is not a thing which should be the occasion for giving a new name to the product. By means of it there may be introduced a practically new and valuable chemical product which we are all delighted to have, but we do not want a new name. We have enough trouble in this world without having to change our classifications and nomenclature with which we have become familiar through long years of use. We are accustomed to calling wrought iron "iron," and we do not want to have to recast our ideas and say that "iron" sometimes refers to slag-bearing—i. e., wrought—iron and at other times it is the absolutely radically distinct product that Mr. Cushman

The President. has described. It seems to me a great pity to introduce this radically different, and to me very confusing, feature into our nomenclature, because I do not see that we are going to gain anything by it. We might just as well call this product dead-low-carbon steel or dead-soft steel, or what we please, but do not invent a practically new name which is going to cause so much confusion. Do not specify this product as ingot, but as a product containing less than 0.03 per cent. carbon. The South Staffordshire concern did not succeed in calling their product ingot iron; they said it was steel.

Gentlemen, I have spoken too long on this subject. I feel very strongly upon it, having struggled with this very troublesome matter of nomenclature more perhaps than any of you, or all of you collectively.

The paper is before you—a very interesting one. I hope it will be discussed as it deserves.

Mr. Moldenke. MR. RICHARD MOLDENKE.—It seems to me that the crux of the whole paper is in one point which was not described. The writer after telling how all the impurities were gotten out of the steel at the expense of adding oxygen to the steel, says that "having obtained the desired purity with respect to these elements the bath is given a heroic treatment, whereby a thorough deoxidation takes place." Now what is that "heroic" treatment?

Mr. Cushman. MR. A. S. CUSHMAN.—That is a very good and a very pertinent question. I do not think there is any great mystery about it; it is simply a question of deoxidation obtained by the addition of high-grade pig iron at the proper time in the process, taken in conjunction with careful control of the heat treatment throughout. My use of the word "heroic" may not have been a happy one, but at the time I prepared my paper I was not informed as to just how far I was at liberty to discuss narrow details of operation which have been worked out at great expense, and which might be with perfect propriety considered as not germane to a paper such as mine, which only attempts to give a general review of what I consider a new metallurgical development.

The President. THE PRESIDENT.—Do you use aluminum?

Mr. Cushman. MR. CUSHMAN.—Aluminum is used in the ladle but not in the furnace.

I can speak for the manufacture of this product and state

Mr. Cushman. that there is no great mystery about it. There is novelty in the fact that something has been done with deliberation which all other people have tried to avoid; but I do not think there is any tendency on the part of the manufacturer to make a mystery of the process. I am sure that any metallurgist would be welcome at the works and would be taken through and shown everything that there is to be seen. The reason that I used the term was simply that I did not know when I wrote this paper just to what extent I was privileged under the conditions that maintain to speak for the manufacturers.

Mr. Howe, we must all admit, is the dean of American metallurgy, and every word that he says must receive the greatest respect and consideration on the part of all of us who are attempting, in however slight a degree, to follow his footsteps. Nevertheless, I must venture with due humility to express an opinion which is evidently not in agreement with that of Mr. Howe. That it will be a great inconvenience to change the old nomenclature I will admit. It has been a great inconvenience, gentlemen, to change a great many old ideas. Whenever a proposition is made to change from old accepted standards, there are not wanting champions of conservatism to rally around the banner of resistance to change; but if this is a new metallurgical product, which I claim it is, then it requires a new nomenclature. We can show beyond question that we have practically eliminated carbon as a constituent. Are we to be asked to call a carbonless metal steel? If so, then I believe we will be indulging in a misnomer, for iron is iron whatever method may have been employed in its manufacture. Undoubtedly iron of high grade and great purity can be made in electric furnaces and now if we can make iron of equal purity in an open-hearth furnace, why not call it iron? It does not help matters to call it something which it is not. It seems to me that so long as a metal exhibits a pure ferrite structure without showing the slightest appearance of the carbon constituents, it should be called iron; and I for one shall continue, as far as I am able, to resist any system of nomenclature which attempts to call something something which it is not.

Mr. Walker. MR. W. H. WALKER.—I am much interested in the analyses of normal ingot iron which are given in Mr. Cushman's

Mr. Walker. paper, particularly in the very low content of copper. I would ask Mr. Cushman if he can tell us, even if ever so approximately, the proportion of the tonnage of ingot iron which is represented by such analyses as these? I appreciate that it must be only an approximation.

Mr. Cushman. MR. CUSHMAN.—With regard to the copper content, I wish to say that we have found it very difficult to control owing to the fact that it is impossible to always determine in advance the copper content of the scrap used. Many times the copper has run too high in spite of every effort to eliminate it. The subject of copper content has been made one of special study and many experiments have been made in which the alloy field has been invaded. Undoubtedly Mr. Walker is referring to the fact that samples taken in open market have shown higher percentages of copper than shown in the analyses of normal ingot iron as given in this paper. Much information has been gained and it is now possible to control the copper content in normal practice. The effort is now made to keep it as low as possible. This is done, not because we believe that copper does harm, but because we are endeavoring to work to the standard of a commercially pure iron. Copper probably does no harm within certain limits. On the contrary, there is a well-founded contention that when added to steel in small quantities it actually improves it in certain respects.

Mr. Hibbard. MR. H. D. HIBBARD.—The ground upon which Mr. Cushman defends the introduction of a new name it seems to me is not well founded. He says that the idea is to call something that is iron something else than iron. But it would be very easy for him to get up a name that would be distinctive and at the same time would not interfere with anything else in the existing nomenclature. This question of ingot iron was brought up over thirty years ago, and the fact was brought out that soft steel whose properties approximated those of iron, was called steel in order to add to its commercial value. Now the question arises whether this new product is not being called ingot iron with the idea that it adds to its commercial value.

Mr. Moldenke. MR. MOLDENKE.—I always oppose unnecessary changes in nomenclature, because I realize the trouble we have now with regard to cast iron and the so-called "semi-steel," which is an absolute misnomer and advanced solely for a commercial advantage. **Mr. Moldenke.**

MR. J. A. AUPPERLE.—It may be that this new product which we call "ingot iron" will displace the other product "wrought iron," so that eventually there will be no wrought iron made. Then we shall only have electrolytic iron as a chemical curiosity. Shall we then have no more commercial iron and shall the element "iron" be called steel simply because it has been melted? **Mr. Aupperle.**

According to the suggestion of Mr. Howe pure iron made in an open-hearth furnace should be classed as steel. Electrolytic iron if melted would be classed as steel, and wrought iron, if melted, becomes steel, even though it becomes purer by melting.

MR. E. F. KENNEY.—I should like to ask Mr. Cushman if it was not formerly their practice to use in this "heroic treatment" ferro-silicon rather than pig iron; and if so why they changed it? In a paper written by Mr. Carnahan of The American Rolling Mill Company, he spoke of the elimination of oxygen by ferro-silicon. That was some months ago. I should like to know if there was a change made, and if so, why? **Mr. Kenney.**

MR. CUSHMAN.—I can only reply to that by saying that we do not use ferro-silicon now. **Mr. Cushman.**

MR. KENNEY.—But you did? **Mr. Kenney.**

MR. CUSHMAN.—Yes. **Mr. Cushman.**

MR. ALBERT SAUVEUR.—How does the weldability of this ingot iron compare with the weldability of wrought iron? **Mr. Sauveur.**

MR. CUSHMAN.—It has given very excellent results in that respect. When the critical temperature is known and avoided there is no trouble encountered in welding. **Mr. Cushman.**

MR. G. L. FOWLER.—I can add a word to what Mr. Cushman has said in regard to the weldability of ingot iron. I have been using some recently for a flowing-in process of welding. We found in making attempts to weld large sections, such as locomotive frames, that it was almost impossible to make a good weld with any kind of metal, although we got better results with ingot iron than with any other; but on small sections, from ½ to ⅝ in. thick, there is no metal—mild steel, or anything else—that can equal the American ingot iron with either the acetylene blow-pipe or electric process of welding. **Mr. Fowler.**

Mr. MacFarland. MR. H. B. MACFARLAND.—I should like to ask a question relative to the physical characteristics of this ingot iron as shown by the tables in the paper. I note a considerable variation between the elastic limit and the elongation, in some cases as much as 100 per cent. Can Mr. Cushman give us any information as to the cause of this variation?

Mr. Cushman. MR. CUSHMAN.—Mr. Beck is present. He has had charge of all these physical tests for several years, and I should be very glad if he would reply to that question.

Mr. Beck. MR. W. J. BECK.—In regard to the physical tests as reported in the paper, I would say that the variations in the results came about from the different temperatures of heating the bar as it passed through the mill. Sometimes a bar is allowed to cool a little more than at other times, and therefore one bar may be annealed more than another. This list of tests is given to show the wide range that is possible with the material.

Mr. Marburg. MR. EDGAR MARBURG.—Is the term "elastic limit" used here in its commercial sense of "yield point?"

Mr. Beck. MR. BECK.—Yes.

Mr. Cushman. MR. CUSHMAN.—I should like to add one word which I did not include in my paper, gentlemen, and which will also explain some of these points. We are of necessity at the present time making the material in small bottom-poured ingots which necessarily leads to a somewhat underworked metal. This, however, will be improved when the product is made in larger ingots which will be subjected to the blooming mill operation.

THE HEAT TREATMENT OF AN ACID AND A BASIC OPEN-HEARTH STEEL OF SIMILAR COMPOSITION.

BY HENRY FAY.

It is generally conceded from the results of the work of Campbell, and of Webster, that acid open-hearth steel is superior in quality to a basic steel of similar composition, but so far as the writer knows no attempt has ever been made to see whether or not the superiority holds for variously heat-treated specimens. To test this, two steels were selected of the following composition:

	Acid Open-Hearth.	Basic Open-Hearth.
Carbon	0.430 per cent.	0.420 per cent.
Manganese	0.600 " "	0.600 " "
Phosphorus	0.027 " "	0.009 " "
Silicon	0.067 " "	0.033 " "
Sulphur	0.050 " "	0.055 " "

These two steels were made under the same conditions for similar purposes, one in a basic furnace and the other in an acid furnace. The similarity in composition is quite remarkable, the chief differences being the phosphorus and silicon as would be expected.

The steel was received in the form of bars, 2 by $\frac{3}{8}$ by 24 ins., and was used in this form for heat treatment. The heating was accomplished in an electrically heated muffle furnace, and temperatures were determined by means of a thermo-couple of platinum and platinum-rhodium.

Each steel was subjected to the following heat treatment: (1) heated to a definite temperature and immediately cooled in air; (2) heated to a definite temperature, held constant for two hours, and then cooled in air; (3) heated to a definite temperature and cooled in the furnace; (4) heated to a definite temperature, held constant for two hours, and then cooled in the furnace. The results of the physical tests on each series are shown in Table I, Series 1, 2, 3, and 4, respectively.

The bars after heating were machined into 18-in. test bars with a parallel section $1\frac{1}{2}$ ins. wide and 9 ins. long between the shoulders. The section in the jaws of the testing machine was 2 ins. wide.

TABLE I.

SERIES 1. HEATED TO A DEFINITE TEMPERATURE AND IMMEDIATELY COOLED IN AIR.

Specimen.	Temperature, deg. Cent.	Tensile Strength, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Elongation in 8 ins., per cent.	Reduction of Area, per cent.
A.........	700	78,300	35,300	21.1	....*
B.........	700	72,600	33,500	22.6	....
A.........	750	77,500	34,200	22.2	....
B.........	750	70,300	32,200	21.5	....
A.........	800	79,800	33,700	18.8	....
B.........	800	73,500	30,500	18.8	....
A.........	900	81,700	44,000	20.4	....
B.........	900	74,200	38,500	24.2	....
A.........	1000	81,700	40,200	18.8	....
B.........	1000	73,000	36,800	25.0	....

SERIES 2. HEATED TO A DEFINITE TEMPERATURE, HELD CONSTANT FOR TWO HOURS, AND THEN COOLED IN AIR.

A.........	700	75,100	37,200	21.1	....*
B.........	700	67,800	29,600	27.0	....
A.........	750	75,700	35,400	14.5	....
B.........	750	68,200	33,400	14.0	....
A.........	800	79,700	35,800	22.3	....
B.........	800	68,300	32,800	22.3	....
A.........	900	77,200	34,400	21.2	....
B.........	900	68,100	32,500	20.8	....
A.........	1000	79,700	34,200	20.8	....
B.........	1000	68,900	32,400	20.4	....

SERIES 3. HEATED TO A DEFINITE TEMPERATURE AND COOLED IN THE FURNACE.

A.........	700	67,000	33,500	14.8	44.2
B.........	700	70,800	26,100	18.7	44.8
A.........	750	80,100	38,700	21.0	45.6
B.........	750	66,900	30,000	23.4	48.7
A.........	800	80,200	37,500	21.8	42.7
B.........	800	72,200	38,200	22.7	44.5
A.........	900	73,500	39,800	21.8	41.1
B.........	900	65,700	33,800	28.9	41.3
A.........	1000	73,100	33,700	22.6	37.9
B.........	1000	68,100	26,100	25.7	38.0

SERIES 4. HEATED TO A DEFINITE TEMPERATURE, HELD CONSTANT FOR TWO HOURS, AND THEN COOLED IN THE FURNACE.

A.........	700	77,030	37,990	14.0	51.3
B.........	700	66,670	33,200	24.2	44.6
A.........	750	74,680	38,200	23.4	51.4
B.........	750	67,400	34,200	25.0	45.0
A.........	800	71,300	37,600	25.0	47.4
B.........	800	63,600	31,800	26.5	48.4
A.........	900	72,500	37,900	23.4	37.8
B.........	900	72,300	34,400	22.6	36.4
A.........	1000	71,200	37,000	22.2	36.8
B.........	1000	64,600	34,200	21.3	37.3

A....Acid steel. B....Basic steel. * Not determined.

Tests on the original bars were made, and gave the following results:

	Tensile Strength, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Elongation in 8 ins., per cent.	Reduction of Area, per cent.
Acid...................	85,400	43,700	20.30	43.2
Basic..................	73,500	37,700	25.00	39.6

The results obtained on the heat-treated specimens are given in Table I.

These results are very incomplete and are to be considered as qualitative rather than quantitative, but nevertheless they seem to show the superiority not only of the acid steel as originally received from the mill, but also when subjected to varied heat treatments. This difference cannot be accounted for by the carbon, as the results of the analyses were checked by two independent workers and these in turn checked against the mill analyses. The maximum variation in percentage of carbon can hardly be more than 0.03 per cent., and if it were so considered, it could not account for variations in tensile strength of from 6,000 to 10,000 lbs. per sq. in. Whether or not these differences can be accounted for by the phosphorus and silicon, both of which are higher in the acid than in the basic steel, only more extended investigations will show. To a person not connected with a mill it would be difficult to follow these results to the logical conclusion, and they are now offered with the hope that they may be of some value to others who may wish to carry the investigation further.

The microstructure of the various bars was studied with the hope that some differences might be found, but there was a marked similarity throughout. In the original specimens as received from the mill the basic steel seemed to show the effect of work slightly more than the acid steel.

The heat treatment, however, did not have the usual effect. In the specimens heated to the higher temperatures, beginning at 900° C., there was a very considerable tendency for the pearlite areas to become more dense in appearance and to segregate along lines parallel to the direction of forging. There was very little tendency toward the usual enlargement of the grain, except in limited areas.

DISCUSSION.

The President. THE PRESIDENT.—As I understand it, Mr. Fay practically confirms the work of Mr. Harbord and others showing that we had very good reason to suspect that the acid open-hearth steel was as a whole for some reason or other better than basic; but we wish that Mr. Fay would go a little further and show us why that is, for we have no agreement as yet as to the reason for the superiority of one process of manufacture over another. If the reason for such superiority can be made clear, then possibly by some perfection of the inferior process it might be improved to a quality equal to that of the superior.

Mr. Aupperle. MR. J. A. AUPPERLE.—Mr. Chairman, I would suggest in case of acid open-hearth steel that a study of the hydrogen, oxygen and nitrogen contents might be of use in accounting for the higher physical values of the acid steel.

Mr. Boynton. MR. H. C. BOYNTON.—I should like to ask Mr. Fay if he has made any other chemical tests than those noted, particularly for oxygen, hydrogen and copper, and as to their influence on the properties of the steel in question? The company with which I am associated makes both grades of open-hearth steel corresponding very closely to the analysis given. We have found that oxygen has undoubtedly some influence on the properties of steel.

Mr. Fay. MR. HENRY FAY.—No other tests than those indicated have been made. The work was done some time ago, and I have withheld the results for several years, hoping that some time I might be able to take it up more in detail. I have presented it at the present time more for the purpose of promoting discussion than of offering anything definite. I think the determination of oxygen, nitrogen and hydrogen would be extremely important. Certainly in future work those determinations should be made.

The President. THE PRESIDENT.—The oxygen question connects itself very readily with the basic process, in which we have a slag much richer in oxide of iron than in the acid process; hence the prob-

The President. ability that the steel under that slag is also richer in iron oxide in the basic than in the acid process. I think that the readiness with which the basic open-hearth forms blowholes, and all of the phenomena that we are so familiar with there, point to that as probably one reason for the superiority of the acid steel: the slag is much freer from iron oxide.

STUDY OF THE HEAT TREATMENT OF SOME LOW-CARBON NICKEL STEELS.

By Henry Fay and J. M. Bierer.

The increasing importance of low-carbon nickel steels has made it desirable and necessary to study in detail the effect of varying heat treatment upon the physical properties, and further, to determine the relationship between physical properties and microstructure. For this purpose we selected two steels, one containing 3.15 per cent. and a second containing 3.50 per cent. of nickel. It was impossible to get commercial steels with the carbon percentages exactly the same, but from the analysis it will be seen that the difference is extremely small. It was not our purpose to establish exactly the relationships between composition, physical properties, and microstructure, but rather to place "land-marks" for the guidance of the users of such steels. Even to those who have occasion to use similar steels we wish to express a word of warning in applying these results to large-size specimens, as all of our results were obtained on small pieces. What is true of these small specimens may not necessarily hold absolutely for larger pieces.

The two steels used for the investigation were made by the open-hearth process and had respectively the following analyses:

	Steel 1.	Steel 2.
Nickel	3.15 per cent.	3.50 per cent.
Carbon	0.35 " "	0.34 " "
Manganese	0.63 " "	0.60 " "
Silicon	trace " "	0.16 " "
Phosphorus	0.027 " "	0.030 " "
Sulphur	0.016 " "	0.014 " "

The bars used in the heat treatment were round, $\frac{7}{8}$ in. in diameter, and 5 ins. long. After treatment they were cut into the standard $4\frac{1}{4}$-in. test specimen, one inch of the end being reserved for microscopical examination.

The heat treatment was carried on in an electrically heated muffle furnace of such size that three specimens could be heated

Table I.—Physical Properties of 3.15-per cent. Nickel Steel.

Heat Treatment.	Temperature, deg. Cent.	Elastic Limit, lbs. per sq. in.	Tensile Strength, lbs. per sq. in.	Reduction of Area, per cent.	Elongation in 2 ins., per cent.
Original specimen.	...	48,000	90,500	56.7	28.0
Air-cooled........	650	47,000	93,300	46.3	27.5
" "	700	48,000	92,000	49.0	27.5
" "	750	48,000	91,800	53.2	27.5
" "	800	56,000	94,000	57.7	27.5
" "	850	49,000	91,500	56.6	26.5
" "	900	48,000	93,900	56.0	26.5
" "	950	51,000	95,800	55.1	26.0
" "	1000	47,000	94,500	51.8	24.0
Furnace-cooled...	650	47,000	89,200	58.3	27.5
" " ...	700	46,000	91,500	58.3	27.5
" " ...	750	44,000	86,250	53.5	27.5
" " ...	800	51,000	87,800	54.2	28.0
" " ...	850	49,000	85,900	52.0	28.0
" " ...	900	48,000	88,200	51.5	28.0
" " ...	950	46,000	87,400	52.0	28.0
" " ...	1000	43,000	88,200	51.1	27.5
Oil-quenched......	650	51,000	93,000	57.9	28.0
" "	700	47,000	86,700	60.5	28.0
" "	750	50,000	90,600	64.7	27.5
" "	800	63,000	98,700	63.6	26.0
" "	850	66,000	100,300	64.1	26.0
" "	900	62,000	96,700	62.3	26.5
" "	950	62,000	98,900	61.8	26.5
" "	1000	56,000	96,500	61.6	26.5

Table II.—Physical Properties of 3.50-per cent. Nickel Steel.

Heat Treatment.	Temperature, deg. Cent.	Elastic Limit, lbs. per sq. in.	Tensile Strength, lbs. per sq. in.	Reduction of Area, per cent.	Elongation in 2 ins., per cent.
Original specimen.	...	57,000	96,500	51.2	27.5
Air-cooled.......	600	56,000	91,500	54.4	28.0
" "	650	52,000	90,200	56.5	29.0
" "	700	51,000	89,300	57.9	29.0
" "	750	47,000	87,200	56.3	28.0
" "	800	48,000	92,900	55.3	28.0
" "	850	61,000	95,400	53.8	27.5
" "	900	61,000	94,400	51.4	26.0
" "	950	64,000	96,500	50.3	26.5
" "	1000	52,000	102,700	50.8	22.5
Furnace-cooled...	600	55,000	92,800	55.7	28.5
" " ...	650	55,000	93,800	57.6	28.5
" " ...	700	54,000	91,100	58.0	28.5
" " ...	750	47,000	88,900	59.0	28.5
" " ...	800	49,000	90,100	54.0	29.0
" " ...	850	56,000	92,400	54.3	30.0
" " ...	900	59,000	91,800	53.9	28.6
" " ...	950	57,000	90,200	53.7	29.0
" " ...	1000	51,000	94,800	50.0	25.0
Oil-quenched.....	600	56,000	91,500	57.1	28.5
" "	650	53,000	91,500	58.1	28.5
" "	700	53,000	90,200	59.0	28.5
" "	750	46,000	87,800	59.3	28.5
" "	800	53,000	93,900	61.7	28.0
" "	850	69,000	100,900	63.4	27.5
" "	900	69,000	100,900	65.4	27.5
" "	950	72,000	101,300	64.5	27.0
" "	1000	71,000	104,900	64.3	25.5

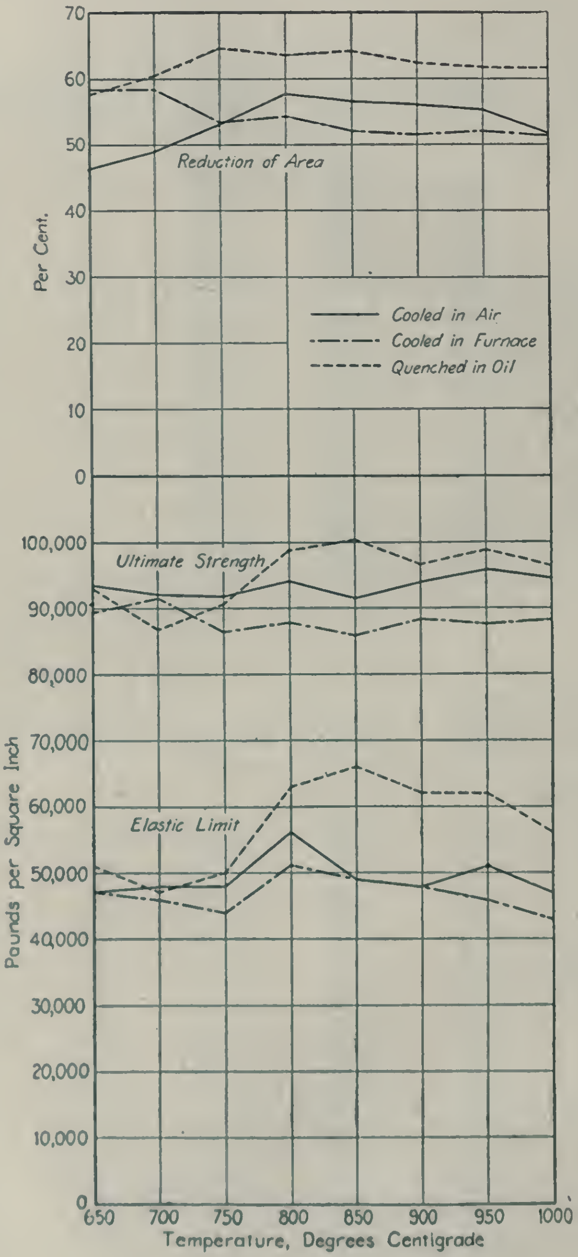

FIG. 1.—3.15-per cent. Nickel Steel.

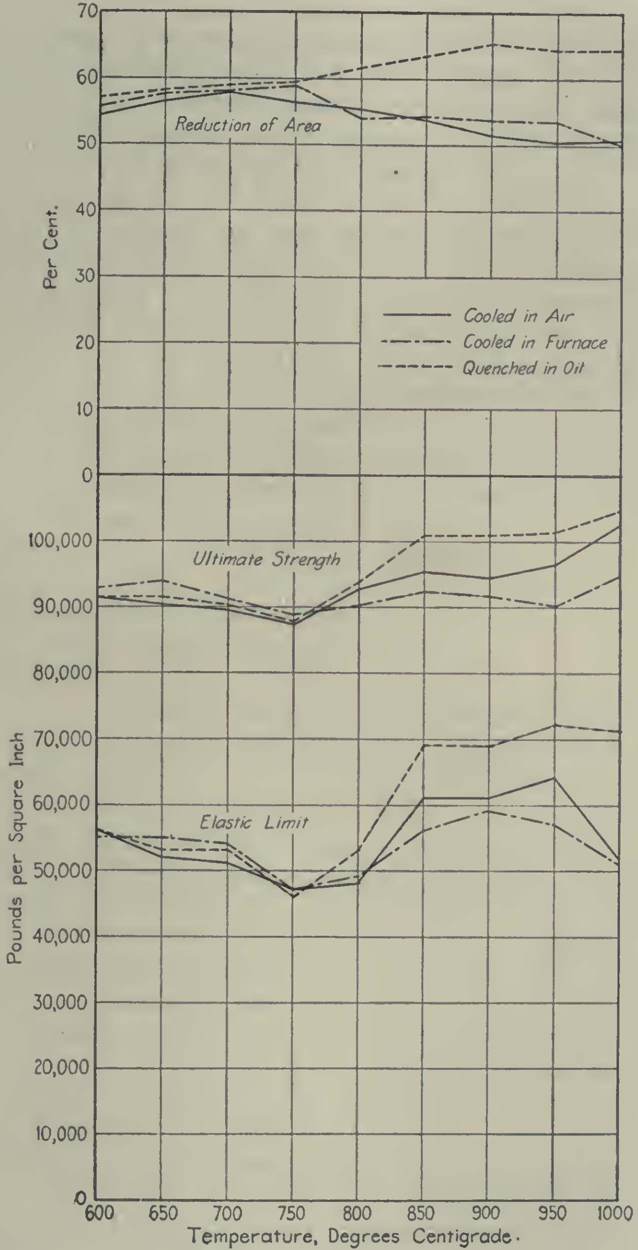

FIG. 2.—3.50-per cent. Nickel Steel.

simultaneously. The temperature was measured by means of a thermo-couple connected with a Siemens-Halske millivoltmeter. The couple was placed permanently in the furnace through a hole in the rear wall. All temperatures are accurate within 5° C.

Each set of three specimens was placed in the cool furnace, heated to the desired temperature, and held at this temperature for a period of two hours. At the end of this time one was quenched in oil, a second was allowed to cool rapidly in air, and the third was allowed to cool slowly in the closed furnace. All oil-quenched specimens were subsequently annealed two hours at 600° C.

The results of the physical tests are given in Tables I and II, and are shown diagrammatically in Figs. 1 and 2.

It will be noted from these results that in general the oil-quenched specimens give the highest values for tensile strength, elastic limit, and reduction of area; the next highest values are those of the air-cooled specimens, and the furnace-cooled specimens give the minimum values. This is true of both steels with a few exceptions. In the 3.15-per cent. nickel steel, the specimen heated to 700° C. shows a lower tensile strength and elastic limit than the corresponding air-cooled specimen. This is, however, explained by the fact that oil quenching from 700° had practically no hardening effect upon the metal.

It is also noticeable that the maximum values are reached at a temperature considerably above the critical temperature of either steel. This does not, however, necessarily mean that heating to high temperatures is desirable, but on the contrary it would seem advisable to heat higher than the critical temperatures, at which point some writers seem to think the best results are obtained. Our experience is not in accord with this latter statement, except for high-carbon steels. The most suitable temperatures for the heat treatment of these two steels lie between 800° and 850° C. At these temperatures air or furnace cooling or oil quenching produces very satisfactory results.

In the case of the 3.50-per cent. nickel steel, greater values for strength may be obtained at the higher temperatures but at the same time the structure of the steel has become very much coarser due to the higher heating.

The microstructure of these steels is similar to that of carbon steels. Whether the material is oil-quenched, air- or furnace-cooled, annealing for two hours at temperatures below 750° C. produces little or no change of the original structure and, it might be added, changes the physical values only slightly. At 800° there is a very decided change and all specimens show an extremely fine structure. The structure at 850° is likewise fine, but at higher temperatures the structure begins to grow coarse and is coarsest at the highest temperatures. In the specimens oil-quenched at 1000° there is no granulation, but there is evidence of crystallization in the metal. The time element is also an important factor, but it was not thought desirable to continue the heating longer than two hours. Prolonged heating would undoubtedly have produced a coarser grain in all except those specimens which were oil-quenched between 800° and 900° C.

The authors wish to express their gratitude to Mr. Jackson, of the Bethlehem Steel Company, for furnishing the material used in this investigation.

THE HEAT TREATMENT OF A STEEL CONTAINING 3.15 PER CENT. NICKEL AND 0.27 PER CENT. CARBON.

By William Campbell and H. B. Allen.

Introduction.—The subject of nickel steel has received considerable attention, notably by D. H. Browne,* and A. L. Colby.† Stoughton in his Metallurgy of Iron and Steel says that about 3.5 per cent. of nickel added to carbon steel will increase the elastic limit nearly 50 per cent. while reducing the ductility only about 15 to 20 per cent. About 3.5 per cent. of nickel will give steel approximately six times the life in resistance to fatigue. Under the heading of "Nickel Steel: Forging Quality," the following figures are given:

Chemical Composition, per cent.: C, 0.25–0.30; Mn, 0.5; Ni, 3.2.

Elastic limit, lbs. per sq. in.	49,270 57,300
Tensile strength, lbs. per sq. in.	87,360 77,800
Elongation in 2 ins., per cent.	34 31.5
Reduction of area, per cent	58 62

Guillet, in a paper on "Steels used for Motor Car Construction in France,"‡ gives the following figures:

Steel.			Condition.	Tensile Strength, tons.	Elastic Limit, tons.	Elongation, per cent.
Type	Nickel, per cent.	Carbon, per cent.				
1	1 to 2	0.3 to 0.4	As rolled.......	31.75 to 38	20 to 24	27 to 20
			Quenched at 800° C. Annealed at 500° C.	44.5 to 57	35 to 47	16 to 12
2	3.25 to 3.5	0.2 to 0.25	As rolled.....	35 to 41	21.5 to 25.5	25 to 20
			Quenched at 800° C. Annealed at 500° C.	54 to 66	42 to 54.5	11 to 14

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* *Transactions, American Institute of Mining Engineers*, Vol. 29, 1899, p. 569.

† "A Comparison of Certain Physical Properties of Nickel and Carbon Steel," Bethlehem Steel Company, 1903.

‡ *Journal, Iron and Steel Institute*, Vol. 2, 1908, p. 177.

Waterhouse[*] studied the heat treatment of a series of steels with 3.8 per cent. nickel and 0.41 to 1.52 per cent. carbon, and found that they behaved the same as ordinary carbon steels as regards temperature and microstructure. Overheating was quite perceptible between 1100° and 1200° C. As compared with ordinary carbon steels the critical points Ar1-3 are 20° lower for each per cent. of nickel.

Souther[†] recommends for a steel of 3.3 to 3.6 per cent. nickel, 0.2 to 0.3 per cent. carbon, 0.5 to 0.8 per cent. manganese, the following heat treatment: "After forging, heat to 1550° F. (844° C.) and quench in oil. Reheat to 800°–1000° F. (426°–537° C.) and cool slowly."

In an annealed condition this steel will have an elastic limit of about 60,000 lbs. per sq. in. and a reduction of area of over 50 per cent. After heat treatment an elastic limit of 90,000 lbs. per sq. in. and a reduction of area of over 50 per cent. is obtained. This strength can be increased by quenching in water or in brine and by varying the drawing temperature. It is possible to obtain an elastic limit of 120,000 lbs. per sq. in. with at least 40 per cent. reduction of area. The endurance of steel so treated under vibration or alternate stress is at least ten times that of a steel in an annealed condition.

Outline of the Research.—Material used: Open-hearth steel; carbon 0.27, nickel 3.15, manganese 0.65 per cent. Rounds, ⅝-in. diameter. Cold drawn.

1. Determination of critical points and of refining temperature (by microstructure).

2. Heat to various temperatures from 600° to 1350° C. and cool (*a*) in air, (*b*) in furnace; (*c*) quench in water, (*d*) in oil.

3. Heat to temperature giving maximum strength, when quenched (900° C.) and (*a*) quench in water, (*b*) quench in oil, and then draw at 200° to 660° C. and cool in air.

The physical properties, elastic limit, tensile strength, elongation in 2 ins., and reduction of area, were determined under (2) and (3) and the microstructure of the series compared.

Determination of Critical Points.—A piece of steel ⅝ in. in diameter and 1 in. long was used, with a small hole drilled to center.

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[*] *Proceedings*, Vol. VI, 1906, pp. 247-258.

[†] *Society of Automobile Engineers*, 1910.

The time and temperature were recorded by means of a stop-watch and Platinum - Platinum-Iridium couple and Siemens-Halske galvanometer. The heating was done in an electric furnace.

Ac1	begins 670° C.	max. 695° C.	
Ac2-3	" 725	" 750.	Ends 780(?).
Ar3	" 795(?)	" 760.	
Ar2	" 725	" (705) 660.	
Ar1	" 630	" 590.	

To check the point Ac2-3 or the refining temperature, a piece of steel was heated to about 1300° C. and cooled in the furnace. Pieces were reheated to 680°, 710°, 720°, 750°, 790° C., etc., and slowly cooled. The piece heated to 720° still showed traces of overheating, while that heated to 750° was refined. The piece heated to 790° showed a coarser grain.

Hence we may say that Ac2-3 is complete at 750° C., which is about 75° lower than for ordinary carbon steel.

Heat Treatment.—The test bars were heated in a gas forge, using a double iron muffle to insure uniform heating. The temperature was controlled by a thermo-couple enclosed in a quartz tube. The time of heating was from 35 to 50 minutes, and the bars were held at the maximum temperature for five minutes to insure uniform conditions. The furnace was then opened and three bars were withdrawn, one to cool between two bricks in air, another quenched in about 30 gallons of water at room temperature, and the last quenched in oil. The furnace was closed up and the fourth bar allowed to cool there.

In Table I are given the physical properties of the bars heated to various temperatures and cooled in air. At the bottom of the list is No. 16, the steel as received. Reheating to 700°, i. e., above Ac1 decreases the elastic limit as shown in No. 16_{11}, while the increase in elongation is marked as we should expect. The next heat, No. 16_{74}, to 750° C. (Ac2-3) gives the maximum ductility. At 1200° C. (No. 16_{29}) there is a marked increase in strength and a corresponding falling off in ductility. This becomes very marked at 1350° C. (No. $17_{48.2}$).

In Table II are given the physical properties of the bars cooled in the furnace. Heating to 610° C. (No. 16_{36}) lowers the elastic limit and increases the ductility, because heat treatment even below the critical points has marked effects on the properties of material that has been worked cold. As before, heating to 750° C. gives the greatest ductility, but even the 1200° C. heat shows little difference, showing that slow cooling may mask overheating.

The physical properties of the quenched-in-water bars are given in Table III. The sharp jump between No. 16_{38} and No. 16_{10} shows that at 700° C. Ac1 has been passed, thus confirming the other determinations. The maximum tensile strength of 275,800 lbs. per sq. in. occurred in the bar heated to 870° C. (No. 16_{40}). The tensile strength remains high to 1200° C. The results on quenched bars are liable to vary, as seen by comparing the three heats to 900° C. This is due in part to the slight set formed in the quenched bars at times. However, there are enough data to show that the strength does not fall off immediately above the maximum as has been claimed.

TABLE I.—BARS HEATED TO DIFFERENT TEMPERATURES AND COOLED IN AIR.

No.	Cooled in Air from —° C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
16_{11}	700	66,630	90,370	33.5	62.8
16_{74}	750	67,400	89,500	37.5	64.4
16_{17}	800	63,600	89,000	34.5	50.5
16_{20}	900	59,660	86,500	35.0	58.7
16_{23}	1000	58,240	87,840	33.0	59.7
16_{22}	1100	59,000	88,300	32.5	60.5
16_{29}	1200	71,740	93,340	25.0	42.3
$17_{49.2}$	1250	63,360	93,400	22.5	35.0
$17_{47.2}$	1300	69,700	95,500	....	36.2
$17_{48.2}$	1350	77,200	98,400	10.0	11.5
16	As received.	83,170	93,175	27.0	58.7

TABLE II.—BARS HEATED TO DIFFERENT TEMPERATURES AND COOLED IN FURNACE.

No.	Cooled in Furnace from —° C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
16_{36}	610	71,650	91,100	32.0	61.0
16_{12}	700	60,350	86,700	36.0	59.0
16_{15}	750	61,400	86,700	39.5	58.8
16_{18}	800	......	85,300	37.0	54.8
16_{21}	900	53,240	84,710	34.5	59.5
16_{24}	1000	52,490	85,760	33.0	55.1
16_{27}	1100	51,650	84,400	35.0	59.4
16_{30}	1200	51,390	83,800	35.0	54.9
16	As received.	83,170	93,175	27.0	58.7

Table IV gives the physical properties of the bars quenched in oil. The tensile strength is high but somewhat below that of the water-quenched bars. The elastic limit shows a falling off which may be accidental. (Bar 17_{24}, quenched at 900°, shows such a large reduction of area that it cannot be correct. It may have been drawn from the oil too soon. Bar $17_{43.2}$ is therefore taken as the standard in Table VI.)

TABLE III.—BARS HEATED TO DIFFERENT TEMPERATURES AND QUENCHED IN WATER.

No.	Quenched in Water from —° C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
16_{35}	610	81,300	101,000	27.0	54.5
16_{27}	665	72,370	94,480	25.5	64.0
16_{5}	680	67,240	103,950	....	....
16_{10}	700	136,900	153,200	....	14.0
16_{13}	750	197,000	211,200	2.5	4.4
16_{26}	800	223,000 (?)	223,000	1.0	2.4
16_{30}	820	146,450	230,650	2.0	4.5
16_{33}	850	139,100	162,800	....	....
16_{20}	870	168,360	275,800	3.0	7.0
$17_{13.1}$	895	......	252,300	2.5	4.1
16_{19}	900	112,200	233,800	....	....
17_{23}	900	159,380	268,150	2.0	7.9
16_{31}	900	118,200 (?)	253,780	2.5	5.3
$17_{14.1}$	925	......	245,200	4.0	6.8
16_{41}	945	206,400	265,000	2.5	5.5
$16_{15.1}$	980	......	228,500	2.0	3.7
16_{22}	1000	147,500	164,300	0.0	4.4
$17_{46.1}$	1000	......	252,000	6.5	....
16_{25}	1100	......	230,900	....	....
16_{8}	1200	......	234,000	....	....
$17_{49.1}$	1250	......	155,000	0.0	4.9
$17_{47.1}$	1300	......	96,400	....	1.0
16	As received.	83,170	93,175	27.0	58.7

The data given in Tables I to IV, inclusive, are shown graphically in Fig. 1.

TABLE IV.—BARS HEATED TO DIFFERENT TEMPERATURES AND QUENCHED IN OIL.

No.	Quenched in Oil from —°C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
$17_{43.2}$	895	197,300	236,000	10.0	29.0
17_{24}	900	161,400	202,730	12.5	51.2
$17_{44.2}$	925	162,800	229,300	10.0	26.0
$17_{45.2}$	980	119,700	231,400	8.5	23.6
$17_{42.2}$	1000	99,200	235,200	10.0	21.2

The next part of the work was the reheating of quenched bars; and the question arose as to the best temperatures for the initial quenching. Looking over the results obtained as shown in Fig. 1, we see that there is marked similarity between the air- and furnace-

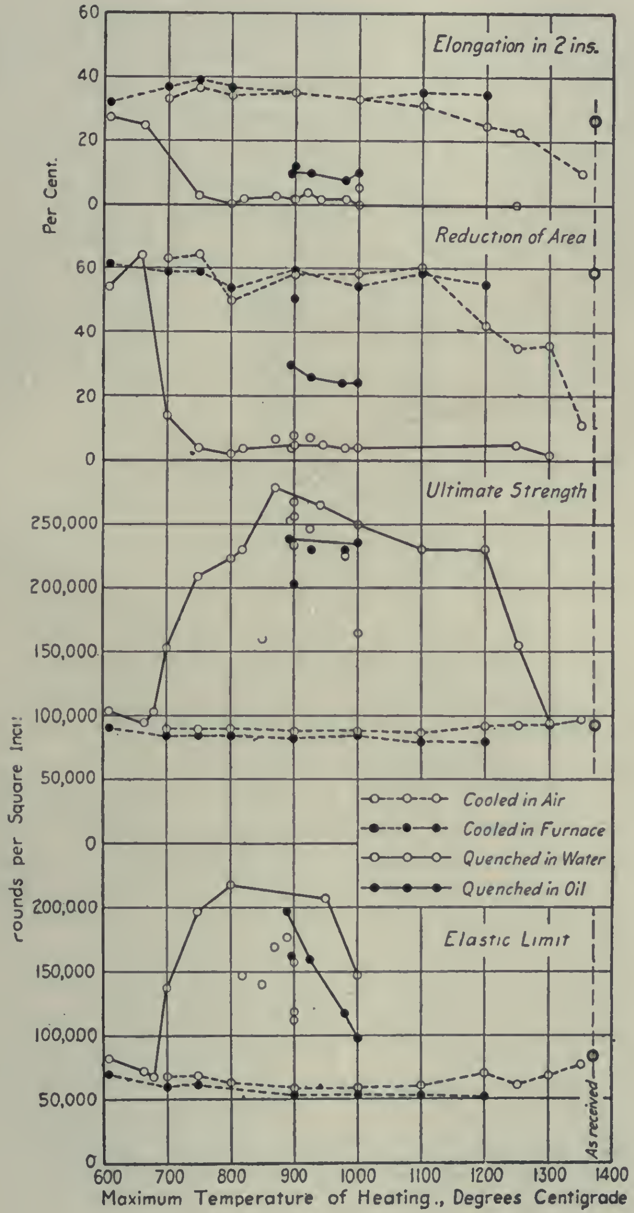
Elongation in 2 ins.
Reduction of Area
Ultimate Strength
Elastic Limit
Per Cent.
Pounds per Square Inch
60
40
20
0
250,000
200,000
150,000
100,000
50,000
Cooled in Air
Cooled in Furnace
Quenched in Water
Quenched in Oil
As received
600
700
800
900
1000
1100
1200
1300
1400
Maximum Temperature of Heating., Degrees Centigrade

FIG. 1.

cooled bars as regards tensile strength and elastic limit, the air-cooled in general being the stronger. This fits in with carbon steels generally. The ductility of the furnace-cooled bars is just a little greater, but the difference in all properties is not so marked as in carbon steels.[*] The falling off at 1200° C. shows overheating.

In regard to the quenched bars, Ac1 is well marked on the tensile-strength curve and by the drop in ductility at 700° C. The upper point Ac2-3 at 750° C. would be expected to show near the maximum strength and minimum ductility. The elastic limit of this heat is high but the maximum for tensile strength is at 870° C. This is probably due to the rate of cooling in the quenching process. It may be that we have to be well above the critical point to get the maximum effect.

In regard to the best temperature for initial quenching for the material to be tempered, the choice lay between just above Ac2-3, say 760° C., and 870° to 900° C. where the maximum strength was obtained. The latter temperature was chosen because, since the grain was much coarser than at 760°, the process of tempering could be followed more easily in studying the microstructure.

Six bars were heated to 900° C., quenched in water and then reheated to 200°, 300°, 400°, 500°, 600° and 650° C. in an electric furnace and cooled in air. The time of tempering was about 45 minutes at maximum temperature. Six bars which had been quenched in oil from 900° C. were similarly treated.

Table V shows the effect of quenching in water followed by tempering. The original quenching gives a low ductility, elongation 2 per cent., reduction of area about 8 per cent. Reheating to 200° C., while lowering the strength, increases the elongation to 12 per cent. and the reduction of area to 54 per cent. In order to find if tempering was effective at much lower temperatures, two more pairs of bars were heated to 900° C., quenched, and drawn to 150° and 175° C. These are bars $17_{23.8}$ and $17_{23.7}$ water quenched, and $17_{24.8}$ and $17_{24.7}$ oil quenched. Therefore, being quenched in another heat, these bars are not strictly comparable with the other six of the series. However, while maintaining a high tensile strength, the ductility is increased enormously by reheating to 150° C. These results are so remarkable that the work will be repeated with a series tempered between 100° and 200° C.

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[*] "On the Heat Treatment of Medium-Carbon Steel," William Campbell. *Proceedings*, Vol. VII, 1907, pp. 240-251.

TABLE V.—BARS HEATED TO 900° C., QUENCHED IN WATER AND REHEATED TO VARIOUS TEMPERATURES.

No.	Reheated to —° C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
17_{23}	.0	159,380	268,350	2.0	7.9
$17_{23\cdot 8}$	150	206,000	230,000	15.0	45.0
$17_{23\cdot 7}$	175	183,500	219,600	15.0	47.0
$17_{23\cdot 6}$	200	196,000	233,500	12.0	54.0
$17_{23\cdot 1}$	300	166,140	189,070	14.0	58.8
$17_{23\cdot 2}$	400	139,700	153,730	15.5	59.9
$17_{23\cdot 3}$	500	112,580	126,690	19.0	62.7
$17_{23\cdot 4}$	600	91,815	105,160	26.0	68.5
$17_{23\cdot 5}$	650	70,300	93,760	30.0	69.0
16	As received.	83,170	93,175	27.0	58.7

Table VI shows the effect of quenching in oil and reheating. The results are not so markedly different as in Table V, but from 200° C. the effect of reheating is uniform. At 650° C. we get back almost to the original steel with a little more ductility.

TABLE VI.—BARS HEATED TO 900° C., QUENCHED IN OIL AND REHEATED TO VARIOUS TEMPERATURES.

No.	Reheated to —° C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
$17_{43\cdot 2}$	.0	197,300	236,000	10.0	29.0
$17_{24\cdot 8}$	150	167,700	221,500	14.0	35.0
$17_{24\cdot 7}$	175	161,500	217,000	15.0	38.0
$17_{24\cdot 6}$	200	183,450	221,580	14.5	54.6
$17_{24\cdot 1}$	300	159,630	184,150	14.0	55.7
$17_{24\cdot 2}$	400	142,900	157,750	16.0	61.6
$17_{24\cdot 3}$	500	117,070	128,470	20.0	62.9
$17_{24\cdot 4}$	600	98,820	106,140	25.0	67.8
$17_{24\cdot 5}$	650	72,920	94,640	30.0	68.9
17	As received.	83,170	93,175	27.0	58.7

Fig. 2 shows these results graphically, and the uniformity above 200° C. shows that the tempering effects vary uniformly with the temperature.

From the above results we may select the following for comparison with other material:

No.	Heated to 900° C. and Quenched.	Reheated to —° C.	Elastic Limit, lbs. per sq. in.	Ultimate Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
$17_{23\cdot 6}$	In water . . .	200	196,000	233,500	12.0	54.0
$17_{24\cdot 6}$	In oil	200	183,450	221,580	14.5	54.6

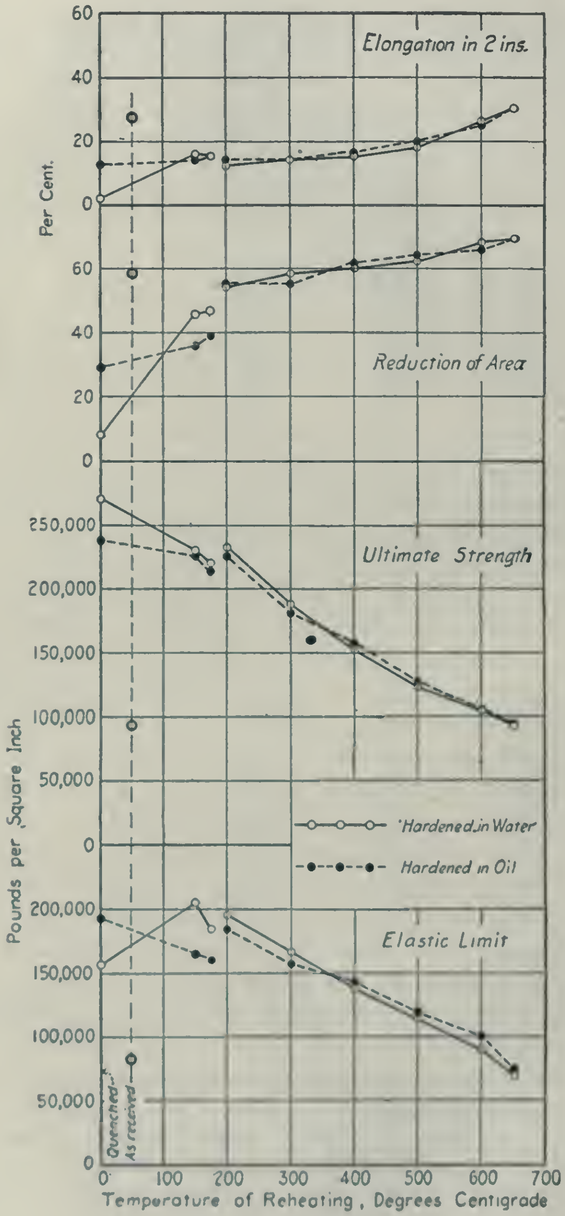
Elongation in 2 ins.
Reduction of Area
Ultimate Strength
Elastic Limit
Per Cent.
Pounds per Square Inch
60
40
20
0
60
40
20
0
250,000
200,000
150,000
100,000
50,000
0
200,000
150,000
100,000
50,000
0
Quenched
As received
Hardened in Water
Hardened in Oil
0
100
200
300
400
500
600
700
Temperature of Reheating, Degrees Centigrade

FIG. 2.

TABLE VII.

No.	Treatment.	Microstructure.
16	As received..........	Fine-grained mixture of ferrite and pearlite. Outside shows more ferrite and effect of cold drawing.
16_0	Heated to white, cooled in furnace.........	Coarse grain. Characteristic overheated structure.
	Heated; reheated to—	
16_8	680° C..............	Pearlite refined(?). Ferrite unrefined.
16_7	710° C..............	Almost refined. A little coarse ferrite remains.
16_9	720° C..............	A trace of coarse ferrite unabsorbed.
16_6	750° C..............	Refined completely.
16_5	790° C..............	Grain slightly coarser than last.
16_4	810° C..............	Grain slightly coarser than last.
16_2	820° C..............	Grain slightly coarser than last.
16_1	850° C..............	Grain coarser.
16_3	885° C..............	Grain coarser. A few large grains.
17_{22}	900° C..............	Grain coarser. A few large grains.
	Heated to—	
17_{10}	700° C. and quenched	Fine grains of ferrite in austenite.
17_{13}	750° C. " "	Very fine-grain martensite.
17_{16}	800° C. " "	Slightly coarser martensite.
17_{33}	850° C. " "	Slightly coarser martensite.
17_{19}	900° C. " "	Much coarser martensite.
17_{44}	1015° C. " "	Coarser martensite.

Microstructure.—The structure of this steel shows the same variation of ferrite and pearlite as normal low-carbon steel. The quenched material, however, is more difficult to etch. The best results on quenched and tempered material were obtained with 2 per cent. nitric acid in alcohol followed by a slight re-polish to get rid of the stain.

The overheated material is clearly seen to be refined at 750° C., which is at a much lower temperature than for a corresponding carbon steel.*

Quenching at 700° C. gives a fine-grain mixture of ferrite and austenite (martensite), while by quenching at 750° we have nothing but martensite. The higher the temperature above this the coarser the grain.

Tempering, of course, has no effect on the grain size, but affects the martensite itself, decomposing it more and more and therefore making it etch more and more deeply. In other words the change gives us pseudomorphs after austenite. The higher the temperature of reheating the greater the decomposition or change. Lastly, some work on the effect of time of tempering shows that at 300° C. there is a marked difference between tempering for a half hour and 18 hours. This work is to be continued.

Table VII gives the principal changes in microstructure.

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* "Further Notes on the Annealing of Steel," William Campbell. *Proceedings*, Vol. X, 1910, pp. 193-200.

Conclusions.—A steel with 3.15 per cent. nickel, 0.25 per cent. carbon, and 0.65 per cent. manganese, may be refined by heating to 750° C., which is about 75° lower than for an ordinary carbon steel.

Hardening is complete when quenched from this temperature, but higher tensile strength was obtained by quenching around 900° C.

Overheating becomes marked above 1200° C.

Tempering of bars quenched in water or oil gives very uniform increase in ductility and decrease in strength, the change varying very uniformly with the temperature of reheating, till at 650° C. the properties are very nearly those of the original steel, having a slightly lower elastic limit and greater reduction of area.

SOME CAUSES OF FAILURES IN METALS.

BY HENRY FAY.

In the history of the testing of metals there have been developed various methods for ascertaining whether or not the metal in question was suited for some particular purpose. The first of these methods to be developed to any extent was the purely mechanical test and this for a considerable period of time was the only method available. By this method it was possible to determine the tensile strength, the elastic limit, the reduction of area, the elongation, hardness, etc., all of which were important, but in some cases where the amount of material was limited this method was not applicable. Especially was this true in cases where only a fragment of the material was available for experimentation.

To supplement this method, chemical tests were applied and much valuable additional information was obtained. The application of analysis was hailed with delight, and it was predicted that chemical analysis would entirely supplant mechanical testing. This claim, however, was extravagant and it was found that both chemical and physical tests did not furnish all of the data necessary to explain some of the phenomena. This was particularly true in those cases where samples of steel of like composition had been subjected to different heat treatment in the process of manufacture. In such cases chemistry could give little or no additional information as to the causes of variation in the physical properties. In most cases, however, chemistry was able to throw much light upon the properties of materials, and by the studies of many investigators the effects of carbon, manganese, phosphorus, silicon, sulphur, and other elements were accurately determined.

A third method of testing, the metallographic method, was developed, and this not only supplemented the information obtained by the physical and chemical tests, but was able in many cases to fill in the gaps where the others yielded little or no information. Extravagant claims were also made for metallography as for chemistry, and these claims were modified as the experience of time demanded. At the present time metallography is a valuable asset to the testing engineer, and is frequently sufficient in itself, but is more often used in conjunction with the other methods of testing. Its particular uses are in experimental work where studies of new alloys are being conducted, in studies involving the heat treatment of steels, and in the pathological studies of material failing in service.

I do not wish to go into the history of metallography and will assume that its principles are known, but I would like to present several cases of its application to the pathological study of ordnance material. In this field it is highly important to properly diagnose the cause of failure so as to avoid a repetition of the disease. Whenever possible, metallographic tests have been made in conjunction with physical and chemical tests, but it will be seen from some of the results obtained that the diagnosis was really based on metallographical data.

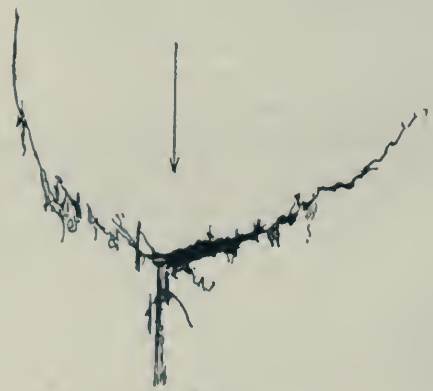

FIG. 1.

The first case which I shall report is that of a very expensive tube forging about 35 ft. in length, with walls 4 ins. thick, and internal diameter 10 ins. This tube, after having been in service for some time, developed a crack on the inner side about 2 ft. in from the end which would correspond to the bottom of the ingot. The crack was irregular, not completely continuous, and varied in width throughout its length as shown in Fig. 1. It also varied in depth, extending much further into the metal in the central portion, and at this point it branched to some extent.

Samples for chemical analysis were taken from (1) three holes drilled in the immediate neighborhood of the crack, and from (2) the other end of this forging. The results obtained are as follows:

	(1)	(2)
Carbon	0.47 per cent.	0.46 per cent.
Manganese	0.72 " "	0.70 " "
Phosphorus	0.024 " "	0.023 " "
Sulphur	0.024 " "	0.024 " "
Silicon	0.188 " "	0.195 " "

There is nothing to criticize in this analysis and hence it gives no clue to the cause of failure.

For physical tests, eleven specimens 1 by 1 by 6 ins. were cut from the neighborhood of the crack, some longitudinal or parallel to the direction of forging, others tangential, and one in a semiradial direction. In addition, two specimens were cut from the remote end of the forging, one tangential and the other longitudinal. The results of the physical tests are shown in Table I.

TABLE I.—RESULTS OF PHYSICAL TESTS.

FROM NEIGHBORHOOD OF CRACK.

Specimen	Tensile Strength, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
1	91,000	53,500	27.0	59.8
2	91,000	53,500	27.0	59.8
3	90,500	51,500	24.5	43.3
4	91,000	51,500	24.0	43.4
5	88,500	50,000	24.5	49.1
6	89,000	51,500	27.5	59.8
7	89,000	52,500	27.0	57.2
8	88,000	50,500	23.0	43.3
9	92,500	54,000	25.0	57.2
10	92,000	54,000	26.5	57.2
11	92,500	54,500	26.0	59.8

FROM REMOTE END OF FORGING.

Longitudinal	88,000	53,000	29.0	62.2
Tangential	83,000	38,500	25.0	34.0

From an inspection of these results it is evident that the metal is uniform and of good quality, and that the crack was not induced by the use of poor metal. The chemical and physical tests are confirmatory in every respect and yield no clue to the cause of the failure.

The specimens cut out for physical tests, before turning to size, were all rough-polished on two sides, and etching experiments were made to see if segregation could be detected. For etching purposes, a freshly prepared 6-per cent. alcoholic iodine solution and an 8.5-per cent. copper ammonium chloride solution were used. The latter solution is strongly recommended by Heyn,* who has used it to detect segregation of phosphorus, carbon, slag, cold-worked spots, etc. He recommends one gram of copper ammonium chloride to twelve of water and the time of etching as one minute. As a general rule this solution is more reliable than

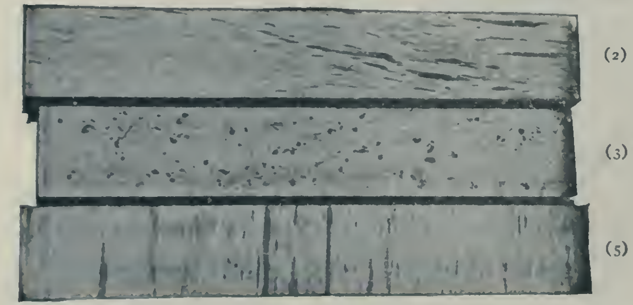

FIG. 2.

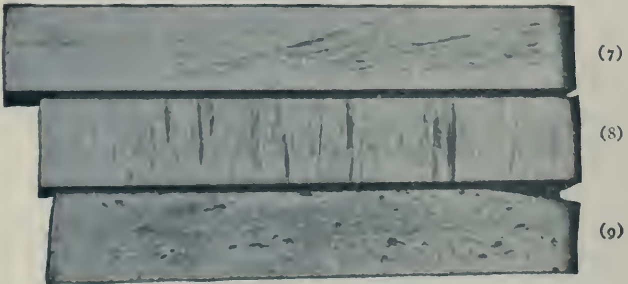

FIG. 3.

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* *Proceedings, International Association for Testing Materials*, 1906.

the iodine solution, especially if the latter is not freshly prepared, but the iodine solution gives results which are better suited for photographic reproduction.

These two reagents confirmed each other in every respect. The results of these etching tests are shown in Figs. 2, 3, 4 and 5.

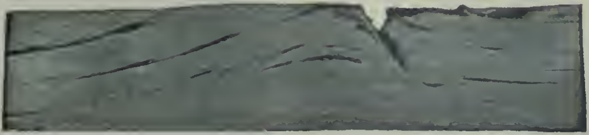
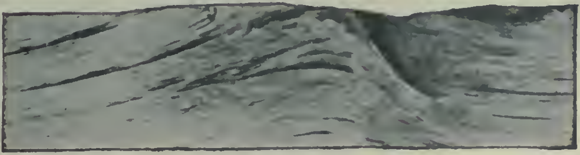


FIG. 4.



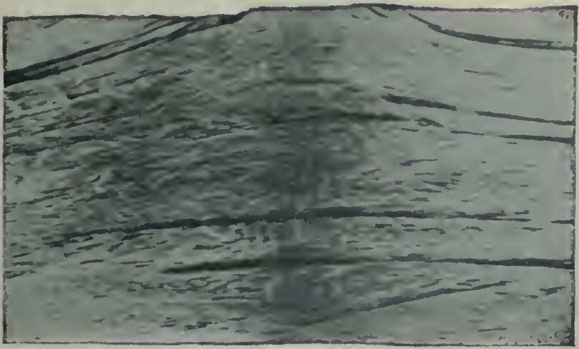


FIG. 5.

Characteristic markings are shown in each specimen and the relationship to the position in the original ingot is well defined. Heyn has shown that in material in which there is some segregated phosphorus the spots where the segregation occurs are marked by a more firm adherence of the deposited copper to these areas than

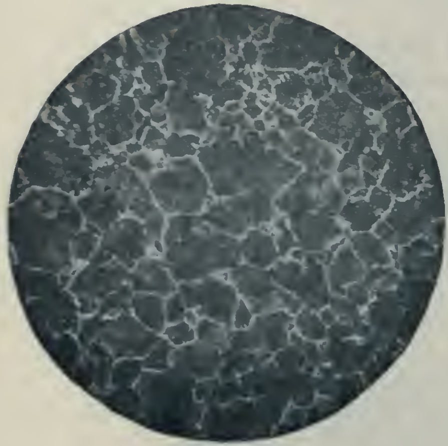


FIG. 6. × 40.

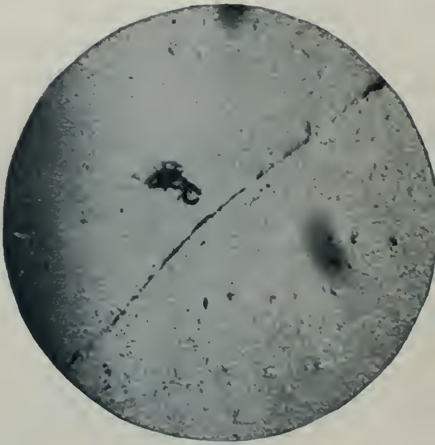


FIG. 7. × 40.

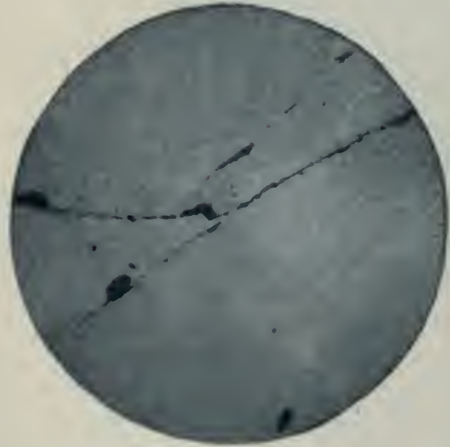


FIG. 8. × 40.

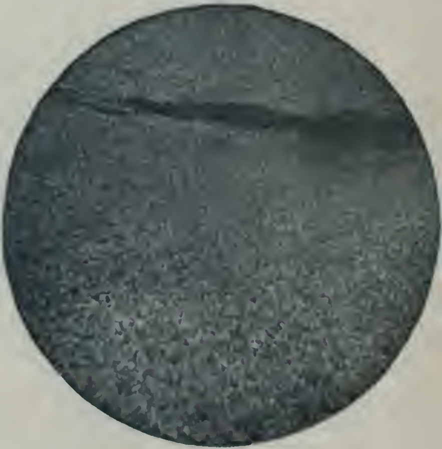


FIG. 9. × 40.

to the non-phosphoric, and by their taking on a bronze color. This statement was confirmed by trial of the copper solution on a specimen of cold-rolled shafting known to be segregated. On the same specimen the alcoholic iodine solution left the phosphide areas much brighter than the surrounding material. Parallel lines of segregated phosphide areas are shown in Specimens 5 and 8 in Figs. 2 and 3 respectively, and the cross-section of similar lines is shown in Specimens 3 and 9. These parallel lines are extended in the direction of forging and hence define the vertical axis of the ingot. In all sections cut parallel to the vertical axis, the phosphide lines are parallel or very nearly so except in the region of the crack, and this is well indicated in Figs. 4 and 5. The significance of this fact will be referred to again.

The cross-sections of these parallel lines therefore define the horizontal axis. On all sections etched on faces parallel to the horizontal axis, in addition to the phosphide marking, there is developed the "fir-tree" structure which is characteristic of the original ingot. This is proof positive, therefore, that the working and heat treatment of this forging had not been properly done, otherwise the ingot structure would have been eliminated entirely. Notwithstanding this defect, the physical tests are good, but circumstances might arise where the ingot structure would be decidedly detrimental.

That the metal did not receive the best of heat treatment is shown in the coarsely granular structure as indicated in the photomicrograph, Fig. 6.

Since the normally parallel lines of phosphide of iron are considerably distorted near the crack, it is reasonable to suppose that the metal in this region has been much distorted. It is highly probable that this distortion has been produced by a fold having been made in the metal during the time of forging. Evidence in favor of this view is obtained by microscopic exploration of this region. If a fold has taken place, there should be found evidence in the form of slag, particularly of oxide of iron, within the metal.

Figs. 7 and 8 show straight and branching lines of slag with some individual and finely divided particles. This is characteristic of this region. Both photographs show the unetched metal. In Fig. 9 is shown a photograph of the metal etched with picric acid. At the upper right-hand side is seen a crack and this leads into a globule of slag. Surrounding the crack and slag there is a decarbonized area, which is distinctly visible in etched specimens taken from the region of the crack.

The presence of slag and the decarbonized areas are significant. The decarbonized area indicates fairly clearly the nature of the slag, for it is easily believable that if a fold in the metal had taken place during the process of forging it would enclose a considerable amount of mill scale, or magnetic oxide of iron. Further, decarbonization by this mill scale would readily take place at the temperature of forging, as is so frequently found in forged bars which show less carbon on the outside than on the interior.

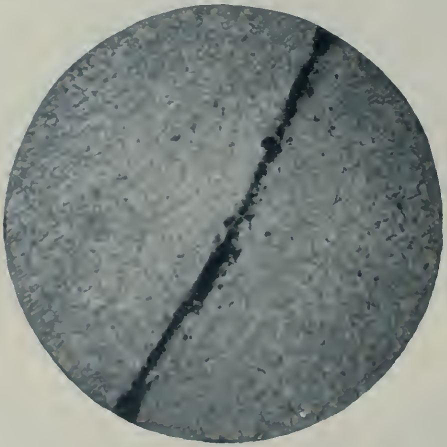


FIG. 10. × 40.

The presence of slag in a decarbonized area is further shown in Fig. 10 and is especially well shown in Fig. 11, where we have a small island of slag embedded in its carbonless area, and this in turn surrounded by the normal structure of the metal.

In previous papers* it has been shown that cracks are induced not only by slag included in the metal, but that when once formed the crack will follow along and through the slag into good metal. It seems then that the crack in this tube may be accounted for by the folding in of magnetic oxide of iron during the process of forging the metal. The fold was apparently welded together, and was not noticeable during the machining. Later, when the metal was subjected to severe strain, cracks opened up and soon extended deeper into the metal until it was found necessary to remove it from service. It has been further demonstrated that the forging and annealing did not remove the original ingot structure, and that the micro-structure was not entirely satisfactory. Physical and chemical tests do not offer any explanation as to the causes of failure, but metallographic methods seem to offer a completely satisfactory one.

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* "A Microscopic Investigation of Broken Steel Rails: Manganese Sulphide as a Source of Danger," Henry Fay. *Proceedings*, Vol. VIII, 1908, pp. 74–93.

"Further Investigations of Broken Steel Rails," Henry Fay and R. W. G. Wint. *Proceedings*, Vol. IX, 1909, pp. 77–88.

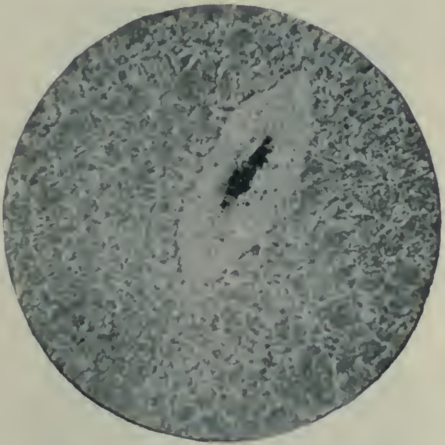


FIG. 11. × 40.

The second case which I wish to present is that of a broken gun lever for a 14-in. gun. A fracture was developed in the position shown in Fig. 12. Whether the crack existed before the lever was placed in position it is impossible to state, but if it did exist, it was not evident to ordinary inspection.

The metal was cast steel of the following composition:

Carbon	0.60	per cent.
Manganese	0.68	" "
Phosphorus	0.05	" "
Sulphur	0.05	" "
Silicon	0.29	" "

For physical tests a number of pieces were cut out near the fracture. The results of these tests showed that the metal did not meet the specifications, which demanded the following:

Tensile strength, lbs. per sq. in.	85,000
Elastic limit, lbs. per sq. in.	45,000
Elongation, per cent	12
Reduction of area, per cent	18

The coupons from which the original tests were made to determine the acceptance or rejection of the metal, probably gave these values on account of the smaller mass of metal concerned.

The stems of the test bars all showed cracks after the metal had been under tension. This is shown in Fig. 13. Subsequent examination of the microstructure showed these cracks to have taken place at the points where there was considerable segregated ferrite, as shown in Fig. 14.

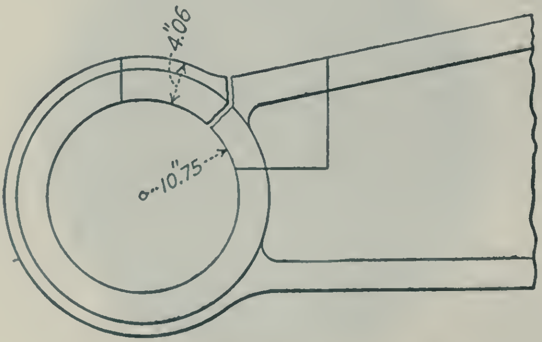
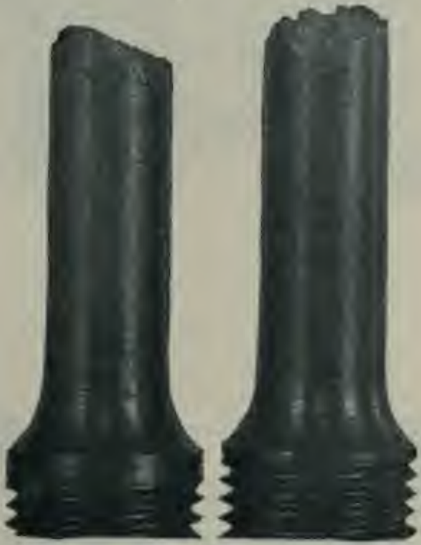


FIG. 12.

Microscopic examination was made of each one of the bars and there was revealed in each a very considerable amount of slag, which is evident in the ferrite areas of Figs. 14, 15, 16 and 17. This undoubtedly lowered the ductility of the metal to a very considerable degree. Further, there was found a very uneven distribution of the ferrite and pearlite areas, as shown in all of the photographs just mentioned. In Fig. 14, as already stated, the ferrite is segregated on the edge of the specimen and this part of the metal gave way before the test piece broke as a whole. Evidently the place where the greatest segregation of ferrite has taken place will determine the point of fracture, for the strength of the ferrite is considerably less than that of the carbonized area. The ferrite is also weakened by the inclusion of slag.

All steel castings should be annealed before being placed in service, not only to remove casting strains but also to get the metal into the best possible form. This gun lever has undoubtedly been most unsatisfactorily annealed, as shown in the photographs already mentioned and also in Fig. 18, which shows some of the original crystalline structure of the metal.



C 1 A C 2 B

FIG. 13.

That such conditions are unnecessary is shown by the fact that test bars cut out of a second gun lever, which failed in service, gave entirely satisfactory tests after having been heated to 875° C. and cooled in air. The microstructure was uniformly fine, as shown in Fig. 19. This second gun lever was bought on the same chemical and physical specifications, and showed exactly the same defects, namely, much slag, and segregation of ferrite.

The cause of failure of this gun lever may be attributed to imperfect heat treatment resulting in the segregation of the constituents of the metal.

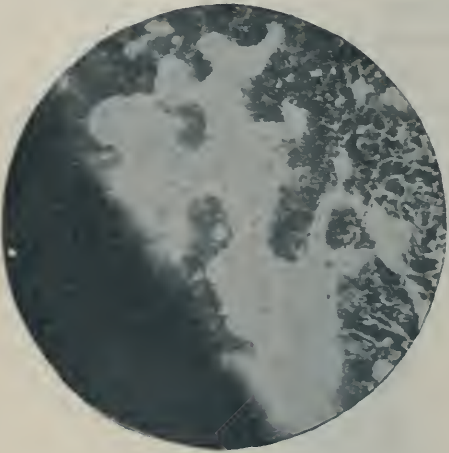


FIG. 14. × 40.

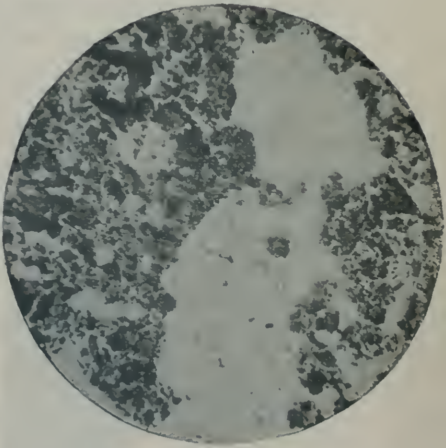


FIG. 15. × 40.

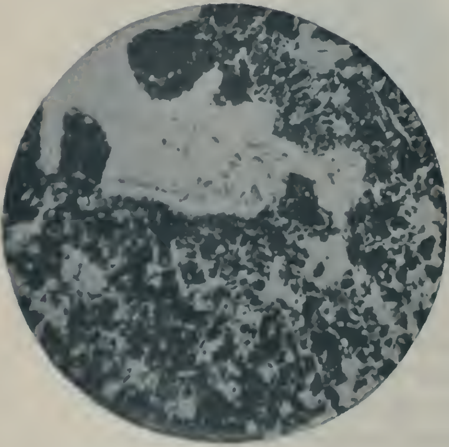


FIG. 16. × 40.

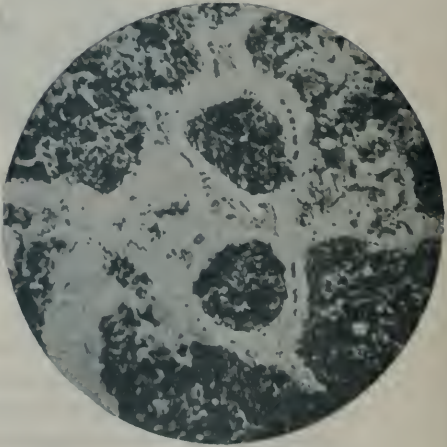


FIG. 17. × 40.

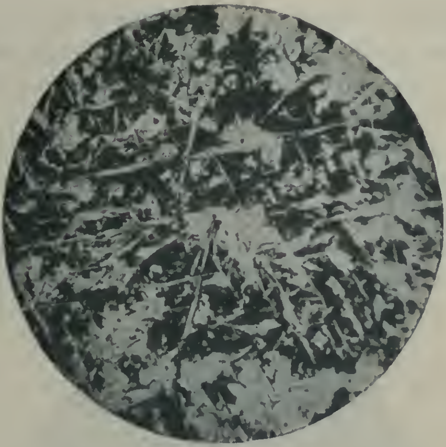


FIG. 18. × 40.

A third case I will present very briefly. It is that of a broken recoil spring, only a fragment of which was available for test. The fractured surface showed in the central portion a distinctly crystalline appearance. From the center core the fracture seemed to follow spirally toward the outer edge. The metal in the outer portion was fine-grained and not so bright as the central core. Several cracks showed, the largest beginning in the crystallized

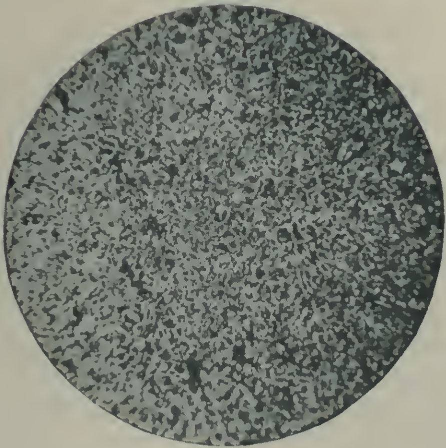


FIG. 19. × 40.

area and extending almost to the circumference. Only enough of the material for a determination of carbon and manganese could be taken from the specimen, and this showed 1.25 per cent. carbon, and 0.33 per cent. manganese.

For microscopical examination an almost complete cross-section was polished. The structure in the central portion was unusually coarse and showed a large excess of cementite, which was crystallized through and formed a network around the dark ground mass which resembled troostite. This is shown in Fig. 20.

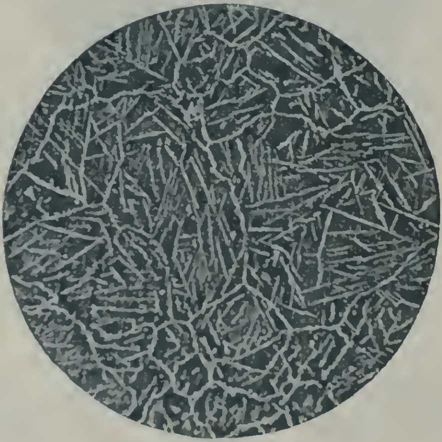


FIG. 20. × 40.

In the outer portion there was a uniform, slightly foliated, dark troostitic structure.

The appearance of two distinct areas is not unusual, as it is a most difficult task to harden any piece of metal 1½ ins. in diameter completely to the center. But it is quite unusual for a spring to contain as much as 1.25 per cent. of carbon; and further, it is also unusual for a spring to show evidences of having been heated to such a high temperature.

The photograph reproduced in Fig. 20 shows not only the high carbon percentage but is excellent evidence of a temperature sufficiently high to allow the cementite to crystallize out.

It is dangerous to allow the percentage of carbon to run so high on account of the possibility of formation of free cementite, which is an extremely brittle constituent. A crack might easily start in an area of cementite and when once started would follow through cementite to the outer surface. Lower carbon content would preclude the presence of free cementite. Finely divided cementite would also be much less dangerous, and this could be obtained by hardening at a lower temperature, as crystallized and granular cementite can only be obtained by heating for a prolonged time at a high temperature.

The causes of this failure are to be found in the use of a steel with too high a percentage of carbon, and in heating the metal to too high a temperature.

The preceding work was carried out in the testing laboratory of the Watertown Arsenal, and I wish to express my appreciation of the courtesy of the Commanding Officer, Colonel C. B. Wheeler, in granting permission to publish the results.

DUCTILITY IN RAIL STEEL.

By P. H. Dudley.

The phrase "Ductility in Rail Steel" does not, as often supposed, express equal physical properties in metal for rails of different chemical composition or different methods of fabrication, and for different conditions of service. Bessemer-steel rail sections of high phosphorus, even with low carbon content, are more fragile in the distribution of rapid large bending moments than the low-phosphorus medium-carbon steel of basic open-hearth rails. There is also a marked difference in the freedom from fractures under similar service between brands of steel made under the same specifications. Rails which are satisfactory in warm climates fracture more frequently in colder climates.

Iron forms the economic basic element of steel, its ductility being utilized with the metal manganese and the metalloids carbon and silicon to produce an alloy of double the elastic limit and ultimate strength of the best wrought-iron rails. The iron rails were adequate for the installation of the railroads, but were so rapidly impaired by the equipment of 1860 and 1870 that they were replaced by Bessemer-steel rails, which have since rendered possible the present extensive railway systems of the world.

Owing to the exhaustion of the available low-phosphorus ores, Bessemer rail steel is now of necessity a high-phosphorus and low-carbon alloy, the mean carbon being about 0.50, manganese about 1.00, and silicon 0.10 to 0.20 per cent.* The impurity of phosphorus is limited to 0.10, while that of sulphur was limited formerly to 0.075 or 0.08 per cent. The manufacturers this year charged for this limitation of sulphur five cents extra per hundred pounds and it has been omitted from most specifications, though it is generally required that its content be reported.

Plain basic open-hearth rail steel is usually a low-phosphorus and medium-unsaturated-carbon alloy, as most of the phosphorus has been reduced by this process from its content in the ores and iron to 0.04 per cent. or under. This permits, in this class of steel rails, carbon of 0.63 to 0.75 per cent. in combination with the same percentage of manganese and a silicon content of 0.10 to 0.20 per cent.

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*The chemical compositions refer in all cases to 100-lb. rails.

The metalloid carbon (under 1.0 per cent.) when alloyed with iron increases its tensile strength, but, as would be expected, lessens at the same time its ductility by increasing its viscosity to strains, yet does not increase its brittleness rapidly so long as the mineral aggregates elongate after passing the elastic limit and have a decided reduction of area at the necking or point of fracture. Steel which is rendered granular from any condition of chemical composition or fabrication partakes more of the nature of brittle solids than of ductile tough alloys.

Manganese also increases the tensile strength of the metal, although to a less extent than carbon when its amount is under 1.25 per cent. It is not an impurity, as frequently stated, but is a necessary element in oxidation processes to reduce the percentage of oxides in the completion of refining the steel.

Silicon is also a requisite element to increase the solidity of the metal, and in the small percentages which obtain in Bessemer or basic open-hearth steel, it adds little if any to the tensile strength of the alloy.

Phosphorus is an embrittler, especially with high carbon. It increases the viscosity of the steel and reduces its capacity to distribute rapid strains or those of large magnitude before fracture occurs, particularly in low temperatures. Therefore, when as much as 0.1 per cent. of this impurity is present the percentage of carbon must be limited for Bessemer rail steel practically to 0.5 per cent., with the necessary manganese. Phosphorus, it is said, may exist in rail steel in two or more compounds, one not being considered harmful to the steel. Several investigations have indicated the same result, but have not disclosed any direct method of fabrication to control the harmfulness of a part of or the entire phosphorus content. It is therefore considered best to reduce the total phosphorus in the ores in this country by the basic open-hearth process, and to replace the embrittling properties of a large content of phosphorus by a higher percentage of carbon. This enables the metal to withstand greater tensile and impact strains and distribute them in the rail section as a girder in a shorter time without fracture than is possible for the Bessemer metal with the higher phosphorus content.

Sulphur is an impurity and renders the metal hot short in rolling, producing checks and cracks, while the greater portion of the sulphur unites with the manganese forming manganese sulphide, which is occluded by the metal as a foreign substance, preventing its welding, and breaking up the continuity of its structure. Silicates which should have escaped from the bath of molten metal are also often occluded in the steel. These defects often become the incipient points of detailed fractures due to the gradual deformation of the section by the repetitions of passing wheel loads.

Test butts from Bessemer rail steel, carbon 0.50, manganese 0.90 to 1.10, phosphorus about, but under 0.10, silicon 0.13 to 0.15, show under the drop-testing machine a range of ductility from 6 to 18 per cent., although the majority of the tests give from 10 to 18 per cent.* There are some heats or groups of heats in which the ductility drops to 5 or 6 per cent. and in which the butt breaks under a second blow. The tests of the butts, unless otherwise indicated, refer to the crop from the top of the "A" rail.

Test butts from basic open-hearth rails, carbon 0.63 to 0.75, with about a similar content of manganese, phosphorus 0.04 or under, silicon 0.15 to 0.20, usually show under the drop-testing machine a range of ductility from 6 to 18 per cent., while an occasional melt gives 20 or 25 per cent. Twenty thousand tons of special basic open-hearth supersaturated steel rails, carbon 0.80 to 0.90, phosphorus under 0.03, with a small percentage of titanium added in the ladle, which have been rolled recently, show a ductility of 6 to 18 per cent. and are now in service. The rails seem tough as a girder, and abrasion tests indicate that they will show great resistance to flange abrasion on curves.

Melts of open-hearth rails for the New York Central Lines contain from 10 to 24 ingots and make from 90 to 180 rails. Three butts are tested from different ingots of each melt; one is from the second ingot teemed, one from the middle of the melt, and a third from the ingot before the last one poured. To pass the melt each butt must show a ductility of at least 5 per cent. in two consecutive

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*The method of determining the ductility in rail steel is described in my paper on "Elongation and Ductility Tests of Rail Sections under the Manufacturers' Standard Drop-Testing Machine." *Proceedings*, Vol. X, 1910, pp. 223–232.

inches upon the base or of 6 per cent. in one inch, while in one butt the ductility of the steel is totally exhausted. The butts for this record are selected in rotation from the different ingots and the range for the melts is thus ascertained. The tests as a rule indicate that the ductility is quite uniform for each melt. The exceptions can usually be traced to colder rolling, differences in heating, and sometimes to brittleness of the metal itself.

The size and weight of the ingots for open-hearth steel vary at different mills. The smallest ingot for a three-rail length of 100-lb. section weighs 3,934 lbs. The ingots for six-rail lengths of 100-lb. section at another mill weigh 8,200 lbs. The ingots at a third mill weigh 12,500 lbs. each and after more than a twenty per cent. discard are bloomed for eight 100-lb. rails, cut in two-rail lengths and reheated before rolling. These large ingots, with nearly sixty reductions in size to that of the section of the rail, receive so much mechanical work upon the metal that the effects of the carbon are intensified and the ductility is reduced two or three per cent. below that of rails of similar composition from smaller ingots. The slightly reduced ductility will give greater resistance for curve abrasion and still be sufficient for the section as a girder.

The ductility of ferro-titanium Bessemer rails, with 0.1 per cent. of metallic titanium added to the bath, carbon 0.60 to 0.70, manganese 0.40 to 0.70, though usually confined under 0.60, silicon 0.10 to 0.18, ranges from 6 to 15 per cent., showing a decided softening of the effect of the phosphorus content by the titanium and lower manganese. These rails are now to be submitted to extensive service tests to see whether or not the expected benefits will be secured in practice.

The value of 8 to 12 per cent. ductility in rail steel may be modified considerably in the fabrication of the rails by the checking of the columnar structure in blooming, producing large percentages of second quality rails. These checks do not roll out completely, and while many rails are rejected, some checks under the scale are invisible in the base of the rail and weaken it transversely, thus losing in effect the ample ductility intended by the chemical composition. It has been found that under high-speed trains of heavy wheel loads the half-moon or crescent breaks which occur in the base of the rail start from these incipient checks in the metal near the center of the base and in low temperatures generally develop as detailed fractures. When the "seconds" run from five to fifteen per cent. of the output, this type of break occurs in rails in which the checks were so minute as to be invisible, and therefore escape detection in the rails at the mills.

Some years since, when the output of the mills was large and the steel not well deoxidized before teeming the ingots, the half-moon fractures were confined mostly to the "A" rails; but they are now more common in the lower rails of the ingots, at least of some brands. The metal in the present blooming trains does not crack or check as much in the upper part of the better deoxidized open-hearth or Bessemer ingots as it does under the first heavier drafts for the lower and larger part of the ingots. The checks have not occurred to as marked an extent in the open-hearth ingots, making only one or two per cent. of seconds, as formerly in the Bessemer ingots, for the reason that the purer open-hearth metal is not as hot-short as the less deoxidized plain Bessemer metal. It is noticed, however, that in open-hearth rails the greatest number of seconds from some of the mills, even of the one or two per cent., are from the last rail in the ingot. While the mills are designed to produce rails, the differences in many of the details of furnace and mill practice have some influence upon the ductility which may be secured and utilized from a given composition of metal.

The New York Central and Hudson River, and the Boston and Albany Railroad Companies, have had a large experience concerning the requisite ductility of rails as girders to carry and distribute the wheel loads and also to resist flange abrasion upon curves under heavy traffic. Of the many thousand tons of Bessemer rails of 0.06 per cent. phosphorus and 0.60 to 0.65 per cent. carbon which I commenced to roll in 1891 and continued until the exhaustion of the low-phosphorus ores in 1902, ninety per cent. were required to exceed a minimum ductility of 5 per cent. per inch as shown under the drop test, which in fabrication by the manufacturers was raised to ninety-five per cent. The maximum ductility was about 18 per cent., and the average was over 12 per cent. The minimum ductility was 4 per cent. per inch for acceptance of ten per cent. of the rails, though confined by good mill practice to five per cent. of the output. The rails proved to be tough and but few fractures have occurred in service, while many are still in the tracks.

Six-inch, 100-lb. rails made in 1894, after sixteen years of service carrying 250,000,000 tons of traffic with a loss of less than 0.125 in. on the head, were tested early this year under the drop, base down on the supports. One butt gave 6 per cent. elongation before it sheared through the bolt holes and a butt from another heat gave 8 per cent. The metal in the head of either rail, like thousands of others in the track, did not flow to the sides but wore uniformly, owing to the efficient support of the metal of high elastic limit underneath the bearing surface. The resistance to flange abrasion was excellent, rails lasting six to ten years upon three- and four-degree curves under heavy traffic. Rails of the same sections containing 0.10 per cent. phosphorus and 0.50 per cent. carbon used to replace them, cut out upon the same curves in two or three years, although there has been some increase in the traffic.

The minimum ductility of the metal has been raised from 4 to over 5 per cent. per inch to insure malleability as well as ductility for the increased service under the present wheel loads and higher speeds. This is for the purpose of meeting the present decreased time factor in distributing the larger and more rapid alternating positive and negative bending moments in the rail as a girder under the moving wheel contacts of the heavier loads and higher speeds, particularly in freezing temperatures.

It has been instructive to note that new rail butts at 60° F. would withstand 2,000 lbs. falling 16 ft. Similar butts at 0° F. would fail when an 8-ft. drop was exceeded, yet under the 16-ft. drop they showed by their curvature that a permanent set and about one-half of the elongation had occurred in the metal as found for a 16-ft. drop before fracture. The cold had increased the viscosity of the metal; therefore its resistance to flow, combined with the reduced time factor for the distribution of the greater energy and speed of impact of the higher drop, started the rupture of the extreme fibers in the base of the rail before the stress could be distributed through the entire section, and fracture consequently took place. Similar conditions occur in the rails in the track which are deflected from the trackman's unloaded surface to the "general depression" under the total loads of the wheel bases of the locomotives or cars, with specific deflections in the rails under the passing wheel contacts.

The heavy wheel loads of slow freight trains with a large time factor to distribute the stresses through the sections as girders, do not check or fracture as many rails as similar wheel loads run at higher speeds with greater generated wheel effects and with less time for their distribution through the rails to the cross-ties, ballast, and roadbed. Oxidized flaws are often found in fractured rails, showing that a minute check had been caused by the maximum intensity of the wheel effect, but was so quickly relieved by the passing of the wheel that the rail was not completely fractured at the first occurrence. Broken rails have been found having in the base two distinct oxidized checks which had occurred several months apart and not until a third excessive wheel load at some subsequent date was the rail completely fractured. There are also many fractures which show that the impact of the passing wheel was so quick and great as to render ductile metal fragile without exercising its ductility in the least. This has led to a general investigation of the relations of the passing equipment to the rails and roadbed, as it is their combination which forms the means of transport.

The wheel loads have been more than doubled and the tonnages quadrupled since the invention of Bessemer steel for rails. Steel-tired 33-in. and 36-in. wheels have replaced those formerly of chilled cast iron for safety under heavy loads and high-speed trains. This change has replaced a non-plastic metal of great resistance to compression, though sensitive to shocks, by a metal with elastic and plastic properties of greater toughness to impact, although subject to all the laws of fatigue and deformation of such alloys. The tonnage upon the single point of contact of a wheel on the rail repeats itself upon all metal in the center of the tread for each rotation. A 36-in. wheel makes 560.2 revolutions per mile and for a static load of 5 tons is equivalent to 2,801 tons per mile. A run from New York to Chicago makes a total of over 2,700,000 tons applied upon every portion of the center of the tread exclusive of any generated wheel effects. A 36-in. tender wheel with an average static load of 9 tons applies 5,042 tons per mile to every portion of the center of the tread, or 756,000 tons for a three-hour run of 150 miles, exclusive of generated wheel effects.

It has not yet been possible for the tire manufacturers to supply the small tires with homogeneous high elastic limits for the entire circumference of the tread. The problem has not been solved to make all small tires sustain 100,000,000 to 150,000,000 tons without irregular deformation in one or more portions of the tread, which every revolution increases, one might say, in a geometric ratio. This produces an eccentric wheel capable of doing great damage at high speeds to the wheels and rails, particularly in low temperatures. It is often found that one wheel on an axle deforms while the metal in its mate maintains its rotundity through the uniform flow and wear of the steel in the wheel tread. The deformation of the metal is more rapid in wheels which run long distances than in those which make shorter runs and have more frequent intervals of rest, there being evidently some recovery from the fatigue of the metal.

More attention will be imperative in the future for transporting heavy loads at high speeds to maintain the drivers and wheels in the treads as true circles, in order to prevent the generated wheel effects becoming too excessive to distribute millions of repetitions of loads without exceeding the ductility in rail steel.

STUDIES ON STEEL TIRES.

By Robert Job and M. L. Hersey.

Conditions of railway practice having become much more rigorous in recent years, many materials, which under the less severe conditions gave good average service, do not now have the requisite strength or endurance. In some cases it has been necessary to change the composition of the material, while in others the methods of manufacture have been improved, or new processes devised, and in still others an entirely different material has been substituted. As is well known, almost every kind of material used in construction work has been affected.

The quality of steel tires for locomotive and tender service is of especial moment, and a great deal of study has been done to determine the exact condition of the steel in tires which have proved defective in service, as well as in those which have given normal mileage. We have been interested particularly in studying the characteristics of those which have failed, but which at the same time have been found to be fairly free from such defects as blowholes, slag and oxide, or other of the more manifest causes of failure.

In general our practice has been (after getting all the data obtainable regarding the conditions of service) to take a full transverse section of the tire, as near as possible to the point of fracture or failure, and to carefully scrutinize the polished surface for any visible defect. The section was then etched to develop seam lines and similar defects. Meantime, a standard test piece was cut from another portion of the tire and the usual tensile tests were made. A ⅜-in. section was next cut from the center of the transverse section and polished and etched for microscopic investigation. A similar section was taken from the transverse section ½ in. from the outside surface of the tire and prepared for microscopic examination, for reasons which will be explained later. Borings were also taken for analysis, the carbon being determined by direct combustion in a current of oxygen in an electric furnace. Photomicrographs were taken at a magnification of fifty diameters to determine the size of grain, that magnification having been found the most satisfactory for the purpose. Also, the hardness of the steel was determined with the Shore scleroscope.

A considerable number of tires have been tested, representing the output of many mills, both here and abroad, and the general results of the study have been exceedingly interesting—in many cases the cause of failure having been fully demonstrated. In the brief time at our disposal, however, we can merely touch upon the main findings.

A fair proportion of failures was due to radically defective composition; for instance, some tires contained over 1 per cent. of carbon. The proportions of manganese, phosphorus and sulphur were generally within the usually accepted limits for tires. The carbon varied from 0.53 to 1.03 per cent.

Under these conditions there was naturally a wide variation in the tensile properties of the steel. In one case the metal, with 0.86 per cent. carbon, and grain size of about $\frac{1}{4}$ in. at fifty diameters, had a tensile strength of 165,650 lbs. per sq. in. with an elongation in 2 ins. of 10.9 per cent. We also found a marked difference between the tensile properties of test pieces taken longitudinally and those taken transversely from the same tire, as shown by the following:

Carbon, 0.795 per cent.	Tensile Strength, lbs. per sq. in.	Elongation in 2 ins., per cent.
Longitudinal	157,000	9.3
Transverse	126,700	2.3
Carbon, 0.770 per cent.		
Longitudinal	150,000	11.0
Transverse	118,300	1.5

The hardness ranged between 37 (0.83 per cent. carbon) and 55 (0.82 per cent. carbon), and as would be expected, the degree of hardness did not necessarily bear any direct relation to the percentage of carbon or manganese, but varied evidently with the physical differences in the steel due to variations in heat treatment.

In the granular structure, the index of the rolling temperatures, we found remarkable differences. For instance, Fig. 1 represents a tire which fractured in service, its composition being:

Carbon	0.649 per cent.
Phosphorus	0.057 " "
Manganese	0.697 " "
Sulphur	0.039 " "
Silicon	0.100 " "

The large size of grain shows that the tire was finished at an excessively high temperature, and that it was ill adapted to meet even the ordinary conditions of service. Fig. 2 represents another very similar tire which also fractured in service. The size of grain is smaller, indicating a lower finishing temperature, but the size is excessive and unquestionably a source of weakness. Its composition was:

Carbon	0.539	per	cent.
Phosphorus	0.052	"	"
Manganese	0.781	"	"
Sulphur	0.032	"	"
Silicon	0.254	"	"

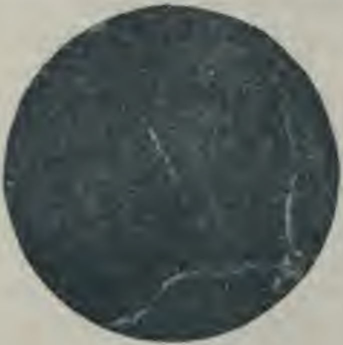

FIG. 1.

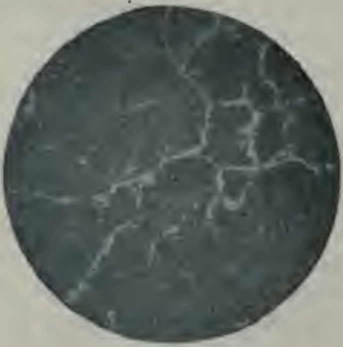

FIG. 2.

In contrast with these, Fig. 3 was taken from a tire which had been annealed. The granular form is fairly fine, although the structure is rather irregular. It represents, however, steel of far greater toughness and ability to resist shock than that shown in Figs. 1 and 2.

FIG. 3.

In examining tires which had not been defective in service we found the usual range in composition. The hardness tended toward the lower figures, for instance about 42 to 47 with about 0.70 to 0.80 per cent. carbon and about 0.70 per cent. manganese.

The granular structure in such tires was usually fairly fine, with granules of about $\frac{1}{4}$ to $\frac{3}{8}$ in. in diameter at fifty diameters though with a percentage of carbon as low as 0.50 the metal was in some cases coarsely granular.

From the brief account which has been given it is clear that the failure of a tire may be caused by any one of many variables, in both composition and physical properties, as well as by changes induced by conditions of service, as for instance skidding. It is also evident that with metal which is subjected to the severe service which must of necessity be borne under present conditions, every feasible precaution should be taken not only to guard against unsoundness and incorrect composition but also to avoid undue hardness and coarse structure. On the other hand, we must bear in mind that if a tough, fine-grain structure is secured by annealing the tires, the elastic limit of the steel is at the same time lowered and the tire is more liable to crush in service. The same condition was observed some years ago by one of the writers in connection with some service tests of annealed rails on the Philadelphia and Reading Railway. An account of this investigation appears in the Proceedings of the New York Railroad Club. December, 1906. Eleven 90-lb. rails were sawed into halves, and one half of each rail was annealed. The structure at the center of head after annealing, fifty diameters magnification, is shown in Fig. 4, and the natural condition in Fig. 5. The carbon content averaged 0.54 and the manganese 1.06 per cent.

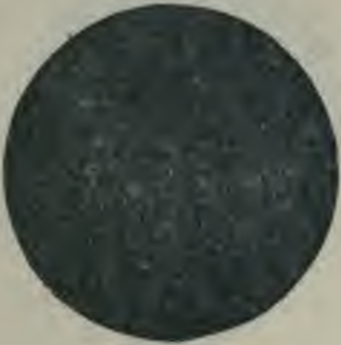

FIG. 4.

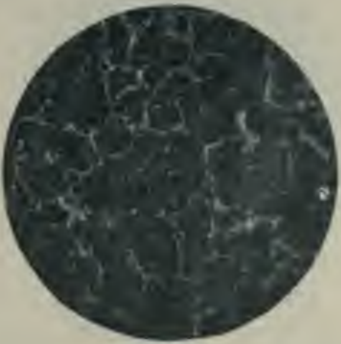

FIG. 5.

After 88 million tons' traffic, it was found that the annealed rails averaged 31.9 per cent. more wear and that they also showed a greater tendency to crush and splinter, but it was found on test that the elastic limit had been reduced from 80,600 to 70,225 lbs. per sq. in. As stated in the paper referred to above, "the clear teaching of the trial was that under heavy traffic high elastic limit is essential to the best service, and it was also evident that if one proposes to get greater toughness and safety by some treatment, like annealing, which lowers the elastic limit, one should also, to get the best results, increase the carbon or manganese contents or other constituents to the point at which the elastic limit will be high enough to resist crushing, even though the steel be somewhat unsound. In other words, the toughness and elasticity produced by the fine granular structure permit one to increase the hardening contents considerably beyond the point which otherwise would be safe, thus obtaining greater capacity for wear." If in a tire, for example, a carbon content of 0.55 to 0.65 per cent. is deemed sufficiently high with coarse-grain steel, it may safely be assumed that with fine-grain steel the proportion of carbon should be increased, say to 0.65 to 0.75 per cent. or perhaps higher, with far greater chances for safety than with the former brittle coarse-grain steel.

In the inspection of tires it has been customary to take a tire at random from a shipment and subject it to a drop test. This practice, of course, gives an indication of the quality of the one which is selected but not, unfortunately, of that of its mates which go into service, and on that account other supplementary tests are necessary. A section for tensile test cannot be taken from a tire without rendering it unfit for service, but we have found it entirely feasible to take a section for microscopic work in such a manner that the service value of the tire is not in the least impaired; and a study of this section gives a clear idea of the heat treatment received at the mill.

In our first experiments we took sections from the edge of the tire at a distance of ¼ in. from the surface, but we soon learned that such a section might not indicate the condition of the metal beyond the immediate surface of the tire. We found, however, that by taking the section at a depth of ½ in. from any surface, a very clear indication as to the character of the granular form at the center could be obtained. The relative difference in size of grain at the center and at $\frac{1}{2}$ in. from the surface of the same tire, is shown in Figs. 6 and 7, the former being at the center of the tire. The analysis was as follows:

Carbon	0.53	per cent.
Phosphorus	0.037	" "
Manganese	0.54	" "
Sulphur	0.030	" "

In Figs. 8 and 9 a similar example is given of a coarse-grain steel, Fig. 8 being from the center of the tire and Fig. 9 from a point $\frac{1}{2}$ in. below the surface. The composition was as follows:

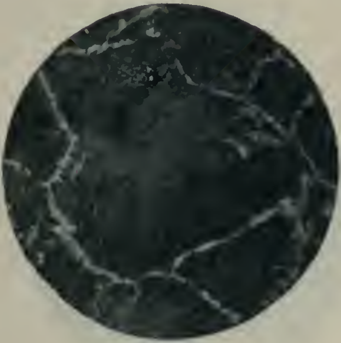
FIG. 6.

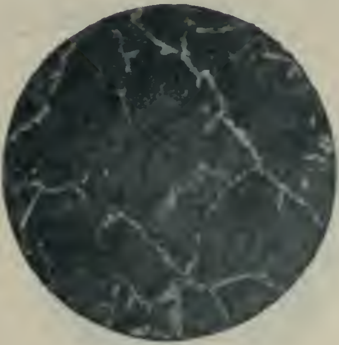
FIG. 7.

Carbon	0.83	per cent.
Phosphorus	0.077	" "
Manganese	0.47	" "
Sulphur	0.049	" "
Silicon	0.21	" "

We found also that a test section sufficiently large for the purpose could be obtained by boring into the tire with a hollow drill of special steel having walls not exceeding $\frac{1}{8}$ in. in thickness and a core $\frac{1}{4}$ in. in diameter; thus the drill had an outside diameter not exceeding $\frac{1}{2}$ in. A hole slightly more than $\frac{1}{2}$ in. deep was then drilled into the tire, preferably from the inside, using screw-cutting oil or compound. The core was broken off with a small wedge, and the end filed until the face was just $\frac{1}{2}$ in. from the surface of the tire. This small section ¼ in. in diameter was then polished, etched and photographed. The ½-in. hole bored into the tire in the position indicated does not injure the tire for service, and consequently tests of any number of tires in a given shipment may be quickly made, in order to prove definitely whether or not the heat treatment has been satisfactory, and in accordance with specifications. The small ½-in. test piece may also be used subsequently, if so desired, in making the hardness test with the scleroscope, provided a plane face is ground longitudinally on one side of the steel. Care must be used to have this face exactly parallel with the opposite side.

In drawing up specifications for the regulation of the heat treatment, it is simpler, we think, and far more satisfactory, to make an engraving from a photomicrograph at fifty diameters showing the maximum size of grain which will be accepted and to print it as part of the specifications.

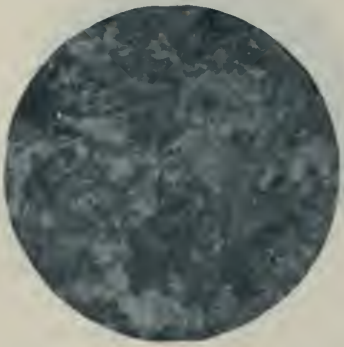

FIG. 8.

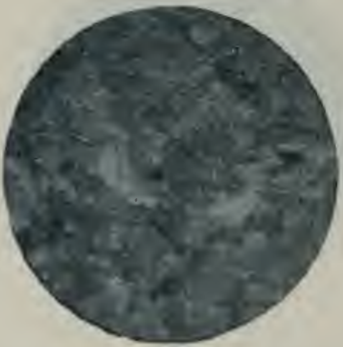

FIG. 9.

In the suggestions which have been made above, we realize fully that this method of investigation does not indicate whether pipes, blowholes or similar defects are present, and reliance must be placed in the present methods of testing and particularly of mill inspection to determine such quality. We are convinced, however, that if, in addition to soundness and freedom from the grosser defects of manufacture, the tire is of fine granular form, indicative of toughness, and of a composition proportioned to insure a suitably high elastic limit, there will result not only increased safety of service, but also a decrease in the number of failures which now occur.

In connection with our work we wish to acknowledge the assistance of various companies and individuals, particularly that of the Canadian Pacific Railway, the Lehigh Valley Railroad, and the Grand Trunk Railway.

DISCUSSION.

e President.

THE PRESIDENT.—The authors have referred to the question of porosity. I wonder whether it would not be practicable to devise a ready specific gravity test. Of course in the determination of the density we at once detect the presence of any considerable quantity of cavities of any kind. I do not know that that has ever been done on a commercial scale, but the matter seems so simple and the indications so valuable that it would certainly be worth while to try to devise some ready commercial method of determining density. Gentlemen, this paper is open for discussion.

r. Job.

MR. ROBERT JOB.—I may say that the determination of the grain size of steel saturated with carbon is beset with the greatest possible difficulties. In some cases it seems to be absolutely impossible to determine it exactly. Under some conditions of etching with special light it seems to be possible to get a fair indication of the grain size; in other cases also we have been able to get fair approximations by simply fracturing the steel and finding whether or not it is of a very coarsely granulated structure. In connection with our work we have tried many different etching media with the idea of developing lines between grains, but in most cases it is an exceedingly difficult matter with a carbon content of 0.80 or 0.85 per cent. In some such cases we have been able to get a fair indication of grain size by oblique illumination.

e President.

THE PRESIDENT.—Mr. Job regrets, as we all do, that in annealing coarse-grained steel we also soften it; but we must discriminate where before we have not always done so, that the refining which removes the coarse grain is the heating above the critical range. That which does the softening and weakens the steel is the slow cooling therefrom which need not at all follow. That is to say, we improve steel by heating it above the critical range, and then spoil it again by cooling it slowly. Now, avoiding rapid cooling, at least through the critical range, is, in my opinion, of the greatest importance.

The Presid

The thing which has been overlooked to a very great extent by steel makers, and on which great stress was laid in the report of the Committee on Heat Treatment presented this morning, is the importance of not spoiling the steel by cooling it extremely or unduly slowly after we have bettered it by heating above the critical range.

FLUE-SHEET CINDER FORMATION IN LOCOMOTIVES.

By Robert Job.

Flue-sheet cinder is the technical name for a growth which forms, as the name implies, upon the flue sheets of locomotives, gradually covering the ends of the flues, and unless removed by the fireman's laborious effort ultimately chokes the draught. Under some conditions this formation never occurs, and a locomotive may run for years without difficulty from this source, but with certain changes the same locomotive may find a normal rate of steaming impossible.

Some years ago we began an investigation of these cinders, or "sulphur lumps" as they are often termed, and the study has been continued under widely differing conditions of fuel and operation. The object, of course, has been to determine the cause of the formation and to develop an effective method of preventing it.

The composition of these cinders varies considerably, as is natural, and the following analyses of two of them, taken from engines using different types of fuel, will give a general idea of the range which may be found:

	Cinder 1. Anthracite Coal.	Cinder 2. Bituminous Coal.
Silica	52.15 per cent.	28.54 per cent.
Alumina	34.51 " "	12.30 " "
Total Iron (figured as Fe_2O_3)	10.29 " "	52.00 " "
Total Sulphur (figured as SO_3)	0.81 " "	4.30 " "
Lime (CaO)	2.68 " "	2.75 " "
Magnesia (MgO)	0.27 " "	0.40 " "
	100.71 " "	100.29 " "

In these analyses the total iron has been figured for convenience as the sesquioxide, although a part existed in the ferrous state. Throughout the paper, however, unless otherwise stated, the percentages of iron are given in terms of metallic iron (Fe), for convenience of comparison.

Cinder 1 was of a greenish-red color, while Cinder 2 was of a deep red due to the higher percentage of iron and to its more complete oxidation. It is interesting to note that in most cases relatively little sulphur was present in these "sulphur lumps," and the percentage of alkalies was usually low. The cinders were generally of a characteristic structure and examination under the microscope showed that they were built up of small dark-colored particles which had fused together into a dense though somewhat porous form.

Under service conditions it may be almost impossible to remove these cinders from the flue sheet owing to the more or less plastic state caused by the high temperature of the firebox. When cool they are, however, rather brittle, with a vitreous fracture.

From the nature of the case it was of course evident that the quality of the coal must be of great importance, so for comparison a large number of analyses were made of representative samples of the coal used and of some of the cinder formed on the run when this coal was burned. In a general way it was found that the composition of the coal ash corresponded with that of the cinder, but the percentage of iron in the cinder was always higher than in the ash from which it was formed; the reason will be explained later. Moreover, the fact was generally developed that when the percentage of iron in the coal ash was low, say below about 1 per cent., or when the color of the ash was white or gray, no formation of cinder, or only a slight one, appeared upon the flue sheets, regardless of the percentage of ash in the coal. In fact, no formation was observed with coal of this character even when the ash in the coal averaged over 25 per cent. With this coal the flues themselves became more or less choked with the light powdery white ash,—"sulphur dust," as the deposit is termed by the engine men,—but it did not adhere to the flue sheet and no clinkering occurred. Analysis showed that the percentage of iron in these flue ashes was lower than that originally present in the coal ash. In other words, the particles containing iron, being heavier, were not drawn into the flues.

It also developed that as the percentage of iron in the ash increased, the tendency toward and the extent of the flue-sheet cinder formation increased, often occurring when the ash contained 2 per cent. of iron. When the ash contained 2.5 per cent. of iron the cinder formation was more marked and it generally became excessive with 5 per cent.

In the course of investigation it was thought that the excess of iron found in the cinder might be derived in some manner from the firebox itself, perhaps by the action of fumes or of moisture upon the steel; but investigation showed that under the ordinary conditions of service, pitting or corrosion of the sheets of the firebox or combustion chamber did not occur, thus proving that the iron was present originally in the coal and was concentrated in the cinder.

From the results of the study, it appeared that the formation of the flue-sheet cinders was due principally to the presence of iron in the coal ash in percentages exceeding about 2.5. Also the actual building-up of the cinder was readily understood when it was considered that upon shaking the grates, or even by the jolting of the engine, fine particles of ash were loosened and easily drawn into the flues by even a gentle draught. The high temperature of the firebox and combustion chamber brought these fine particles to a partially fused or pasty condition when they contained a fair percentage of iron, and upon coming in contact with the flue sheet they adhered to it and were ready to hold the particles next drawn against them.

It was found in service that the cinder grew from the bottom of the flue sheet upward, this being due to the fact that the particles of iron oxide, which were relatively more fusible, were also heavier and were thus first drawn against the lower part of the flue sheet. The cinder accumulated there until the lower flues were partly closed; the draught was then, of course, diverted upward and the pasty particles of iron were carried higher until all the flues were more or less completely closed.

In the course of the investigation we have found that flue-sheet cinders may not in some cases be produced even when the percentage of iron in the ash is relatively high, on account of the greater percentage of ash in the coal. When less than 10 or 12 per cent. of ash is present with the percentage of iron indicated above, the flue-sheet cinders may be expected to form; but if with the same content of iron the proportion of ash in the coal averages about 15 per cent. or more, the complaint is apt to be that the coal "has no heat." The reason is that so much cinder forms upon the grate that the draught is interfered with, which in turn prevents the particles of iron from being drawn against the flue sheet to the extent that would occur with a better draught; while also, owing to the lower temperature in the firebox under these conditions, the particles are not apt to be in a partly fused condition, and consequently merely collect in the flues without adhering to them.

Choking of the draught through the grates is especially liable to occur when the percentage of ash in the coal is very low, say less than 5 per cent., with a high percentage of iron, say 7 per cent. or more. Cases are known in which the clinker upon the grates under these conditions was so fluid that it "ran like molasses," as the engine men said, sticking to the grate and effectively cutting off the draught.

It has also been noted that flue-sheet cinder formation is greater in simple than in compound engines, due to the fact that a larger number of ash particles are drawn against the flue sheet under the stronger draught of the simple engine. The formation is also much more rapid with wet coal than with dry, on account of the dissociation of the moisture, in contact with the incandescent coals, into oxygen and hydrogen gases which together with the draught produce intense heat and fuse into cinder particles of ash which contain a relatively smaller percentage of iron than that which causes clinkering with dry fuel. For this reason flue-sheet cinder formation is apt to be more prevalent in the winter months than in the dryer season.

From the general statements which have been made it is clear that the formation of flue-sheet cinders is an index of the fusibility of particles of the ash, and it might be thought that whatever would tend to decrease the melting point of the ash might at the same time be expected to increase this type of cinder growth. The evidence obtained seems to indicate however that this does not necessarily follow, and the decided increase in the content of iron in the flue-sheet cinders over that in the coal ash, shows that this element exerts a large influence in promoting the formation. It has been found in practice that flue-sheet cinder formation can be avoided under ordinary conditions of service by the use of white-ash coal, or coal in which the proportion of iron is less than 1.5 per cent.

DISCUSSION.

Wickhorst.

MR. M. H. WICKHORST.—I think Mr. Job has explained the matter of the formation of flue-sheet clinkers very nicely from the standpoint of the coal; but there is another factor that enters into the case, and that is the firebox itself, and the condition of the flue ends. Flue-sheet clinkers are evidently an accretion of small pasty particles, as can be seen by their structure; but the pasty particles must have some surface to adhere to, and that surface generally must be warm enough to catch and hold them. The flue ends are apt to become, as boiler makers say, "whiskered" or "burred." These fine "burrs" sticking out into the fire readily become heated, and I have noticed that the formation starts on these flue-sheet "whiskers."

Furthermore, engines which leak badly are inclined to become "honeycombed"—as the railroad expression is—rather quickly, and the impression seems to prevail pretty generally among railroad men that it is the honeycombing that causes leakage. I am rather of the opinion that the same primary condition produces both the leakage and the formation on the flue sheets; that is, if there is much scale formation on the tubes the flue ends readily become overheated, in that way being raised to a sufficiently high temperature to allow the pasty slag to stick.

STRENGTH OF STEEL FROM STRUCTURAL SHAPES.

By E. L. Hancock.

Recent studies to determine the strength of steel cut from different parts of the profile of structural shapes have shown some variation in the elastic limit (proportionality limit). In 1909 Professor Edgar Marburg presented a paper* giving the results of tests of steel from Bethlehem and Standard I-beams, and a paper† presented by the writer in 1910 on the same subject gave the results of further tests. During the past year the writer has made further investigations on this subject, and it now seems desirable to bring all results thus far obtained under examination in order to see just what variation of the elastic limit exists, to inquire into the causes of this variation, and then to discuss the seriousness of the variation and the possible remedies.

A review of the possible causes for variation in the proportionality limit of steel cut from different parts of the profile of a structural shape suggests the following:

1. Difference in chemical composition.
2. Difference in amount of work done on the material in rolling.
3. Difference in working and finishing temperatures of the metal in different parts of the profile.
4. Cold bending of the beams in straightening (gagging).

Difference in Chemical Composition.—The variation in the chemical composition of steel cut from the web, root and flange does not seem sufficient to explain the variations in the elastic limits that have been observed. This is shown by the results given by the writer in the Proceedings for 1910 and in a considerable number of other cases. For convenience of reference, the table of analysis reported last year‡ is repeated here as Table I.

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* "Tests of Standard I-Beams and Bethlehem Special I-Beams and Girder Beams *Proceedings*, Vol. IX, 1909, pp. 378-407.

† "Strength of Steel from I-Beams." *Proceedings*, Vol. X, 1910, pp. 248-257.

‡ Hancock on Tests of Steel from I-Beams, Table IV, *Proceedings*, Vol. X, 1910, p. 257.

TABLE I.—STEEL ANALYSIS.

Beam.			Test Piece from	Elastic Limit, lbs. per sq. in.	Carbon, per cent.	Manganese, per cent.	Phosphorus, per cent.	Sulphur, per cent.
Mark.	Depth, ins.	Weight, lbs. per ft.						
A	6	13.5	Web	32,000	0.201	0.558	0.059	0.110
			Flange	34,000	192	610	98	100
			Root	34,000	194	552	94	90
C	10	25	Web	24,750	0.281	0.311	0.112	0.080
			Flange	26,600	288	420	20	31
			Root	27,000	247	458	60	37
H	15	60	Web	19,500	0.238	0.440	0.007	0.028
			Flange	24,000	243	435	06	30
			Root	16,500	248	408	11	27
I	15	80	Web	27,000	0.212	0.435	0.020	0.042
			Flange	19,500	225	422	27	39
			Root	19,500	241	519	26	35
J	18	55	Web	31,500	0.251	0.518	0.021	0.032
			Flange	25,500	268	524	28	37
			Root	19,500	263	481	26	35
K	20	65	Web	34,500	0.228	0.452	0.053	0.024
			Flange	22,500	261	526	66	25
			Root	13,500	253	460	58	27
L	24	73	Web	33,000	0.285	0.389	0.010	0.028
			Flange	24,000	283	436	08	29
			Root	22,500	287	405	07	30
M	18	48.5	Web	19,500	0.232	0.392	0.007	0.049
			Flange	21,000	228	485	10	50
			Root	28,500	232	428	10	48

Beams L and M are Bethlehem Beams; all the others are Standard I-Beams.

In view of this evidence it was deemed unnecessary to consider the steel differing essentially in chemical composition in different parts of the profile. No further study has therefore been given this matter.

Difference in Amount of Work Done in Rolling and Difference in Finishing Temperatures.—It seems almost necessary to consider these two causes together, since the effect of the one or the other cannot be distinguished after the shape has once cooled. Professor Marburg obtained little variation in the yield point and practically no variation in the tensile strength, percentage of elongation, or percentage of reduction of area that could be attributed to the position of the test piece in the profile of the beam. The values obtained for the elastic limit of test pieces from the web, flange and root of five beams are given in Table II.*

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* Marburg on Tests of Steel Beams, Table II, *Proceedings*, Vol. IX, 1900, p. 385.

TABLE II.

Beam.		Test Piece from	Elastic Limit, lbs. per sq. in.
Type.	Size.		
Bethlehem I-Beam	24-in., 72-lb.	Web	43,000
		Flange	26,400
		Root	33,800
Bethlehem I-Beam	30-in., 120-lb.	Web	26,700
		Flange	25,800
		Root	26,400
Bethlehem Girder	24-in., 120-lb.	Web	35,150
		Flange	31,900
		Root	20,200
Bethlehem Girder	30-in., 175-lb.	Web	34,000
		Flange	25,500
		Root	17,100
Standard I-Beam	24-in., 80-lb.	Web	24,500
		Flange	25,400
		Root	15,200

In the Bethlehem I-beams the web gives the highest value, the root next, and the flange the lowest. In the first case there is a difference of about 17,000 lbs. per sq. in., while in the second there is no appreciable difference. In the Bethlehem girders the web gives the highest value, the flange next, and the root the lowest. In the standard I-beam the web and flange are practically the same and the root some 10,000 lbs. per sq. in. lower. In three out of five complete tests the root gave the lowest values and the flange or web the highest.

The values given in Table III were obtained in the tests on standard I-beams made by the writer last year.*

In the case of six out of eleven standard beams, the elastic limit was highest in the web, lower in the flange, and lowest in the root. The first three, A, B and C, are light beams in which the amount of work done in rolling could not vary much for material in different parts of the profile; F and H, however, are thick, heavy beams.

In the case of the tests of material from three Bethlehem beams reported last year,† and shown in Table IV, the relation of web, flange and root (highest first) holds for only one out of the three.

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* Hancock on Tests of Steel from I-Beams, Table I, *Proceedings*, Vol. X, 1910, p. 249

† *Ibid*, Table II, p. 250.

TABLE III.—TESTS OF STEEL FROM STANDARD I-BEAMS.

Beam.			Specimen cut from	Elongation in 8 ins., per cent.	Reduction of Area, per cent.	Elastic Limit, lbs. per sq. in.	Maximum Strength, lbs. per sq. in.	Location of Fracture.	Appearance of Fracture.
Mark.	Depth, ins.	Weight, lbs. per ft.							
A	6	13.5	Web	18.7	46	32,000	68,300	3rd in.	Silky.
			Flange ...	29	49	34,000	65,300	3rd in.	"
			Root	29	50	34,000	64,300	4th in.	"
B	10	25	Web	28.3	61.5	30,000	56,500	1st in.	Granular and Silky
			Flange ...	28.1	63	25,500	54,600	Center	
			Root	25	56.3	33,000	58,600	2nd in.	
C	10	25	Web	20.2	39.2	24,750	72,300	2nd in.	Granular and Silky.
			Flange ...	34.4	48.8	26,600	61 200	4th in.	
			Root.....	25	54·2	27,000	66,400	3rd in.	
D	12	31.5	Web	31.3	54	29,500	61,800	4th in.	Granular and Silky.
			Flange ...	26.6	50.6	22,500	59,500	3rd in.	
			Root.....	25	53	19,500	63,700	2nd in.	
E	12	40	Web	31.2	56.5	31,500	59,800	2nd in.	Granular and Silky.
			Flange ...	21.9	51.2	24,000	62,500	4th in.	
			Root.....	31.2	55.6	19,500	61,500	2nd in.	
F	12	40	Web	30	58.7	28,500	62,200	2nd in.	Granular and Silky.
			Flange ...	18.7	55	18,700	62,200	1st in.	
			Root.....	31	58	24,000	61,700	2nd in.	
G	15	42	Web	28.2	46.7	31,500	68,700	3rd in.	Granular and Silky.
			Flange ...	......	......	29,900	......	....	
			Root.....	26.6	48	22,500	56,000	4th in.	
H	15	60	Web	29	58.2	19,500	53,300	3rd in.	Granular
			Flange ...	29	60.8	24,000	55,200	3rd in.	"
			Root.....	29	59.2	16,500	53,800	2nd in.	"
I	15	80	Web	36	57.8	27,000	54.300	3rd in.	Granular
			Flange ...	31	61.2	19,500	55,300	2nd in.	"
			Root.....	33	56.9	19,500	55,000	4th in.	"
J	18	55	Web	28.5	53	31,500	55,800	2nd in.	Granular
			Flange ...	29	55 6	25,500	56,700	3rd in.	"
			Root.....	28.5	57.5	19,500	60,400	4th in.	"
K	20	65	Web	28.5	45.3	34,500	62,700	2nd in.	Granular and Silky.
			Flange ...	28	52.5	22,500	63,800	3rd in.	
			Root.....	30	54.3	13,500	64,000	2nd in.	

For Beam M the reverse is true, namely, root highest, flange next and web lowest; and in the case of Beam N the order is flange, web, and root. Summarizing all these tests, then, it is seen that out of the nineteen beam sections studied ten showed the relation of high web and low root values for the proportionality limit, with intermediate values for the flange. The remaining nine present considerable irregularity. In three beams the relation is web, root, flange; in three it is root, flange, web; in one, root, web, flange; and in two, flange, web, root.

It seems reasonable to suppose that the web and flange have more work done upon them in rolling than the root, and also that

TABLE IV.—TESTS OF STEEL FROM BETHLEHEM BEAMS.

Beam.			Specimen cut from	Elongation in 8 ins., per cent.	Reduction of Area, per cent.	Elastic Limit, lbs. per sq. in.	Maximum Strength, lbs. per sq. in.	Location of Fracture.	Appearance of Fracture.
Mark.	Depth, ins.	Weight, lbs. per ft.							
L	24	73	Web	25	52.8	33,000	72,600	Center	Silky.
			Flange ...	28	53.8	24,000	70,000	4th in.	"
			Root.....	28.1	57.3	22,500	67,300	3rd in.	"
M	18	48.5	Web	26.6	55.4	19,500	59,600	1st in.	Silky.
			Flange ...	31.3	55.7	21,000	57,300	2nd in.	"
			Root.....	31.3	58.5	28,500	56,200	4th in.	"
N	12	36	Web	21.9	44	31,500	72,600	1st in.	Silky.
			Flange ...	26.6	43.7	33,000	66,000	1st in.	"
			Root.....	25	51	24,000	68,400	2nd in.	"

they are finished at a lower temperature. It would be expected therefore that the root metal would have a somewhat lower elastic limit than either the flange or the web, and that the web would have the highest elastic strength. This would be especially true in the thick heavy beams and relatively much less true in the lighter thin beams where the work done upon the material is more nearly the same for different parts of the profile. That almost one-half of the beams reported should show results differing from what would naturally be expected challenges the attention, and one is led to investigate further the elastic strength of structural shapes.

During the present year tests have been made under the direction of the writer to see if further reasons could be found for the irregularity observed in so many cases hitherto reported, and to account if possible for the rather low elastic limit in some cases. Nine stub ends of structural shapes were selected as follows: three standard I-beams, 12, 15 and 18 ins.; one 24-in. Bethlehem beam; three standard channels, 4, 12 and 15 ins.; one 10-in. Bethlehem H-Section; and one 6-in. angle. The details regarding these materials and the results of the tests are given in Table V.

It is seen from this table that out of the nine structural shapes investigated, only three give the relation of web, flange, root. The heavy 15-in. 60-lb. beam shows no variation between web, root or flange. The channels all give a higher value for the root and about the same for the web and flange, except in the case of the small 4-in. channel where the web is considerably above the flange. In considering the results of tests of these channels, it should be noticed that they are all rather light sections and that in any case

TABLE V.—TESTS OF STEEL FROM BEAMS, CHANNELS AND ANGLES.

Beam			Specimen cut from	Elongation in 8 ins., per cent.	Reduction of Area, per cent.	Elastic Limit, lbs. per sq. in.	Yield Point, lbs. per sq. in.	Maximum Strength, lbs. per sq. in.	Remarks.
Type	Depth, ins.	Weight, lbs. per ft.							
I-Beam........	12	31.5	Web...	28	61.5	32,000	36,300	59,300	No strain lines.
			Flange.	24.5	68.5	26,000	31,300	52,200	Strain lines.
			Root ..	15	55	24,000	46,600	62,000	" "
I-Beam........	15	60	Web...	29	58	24,000	31,000	60,100	Pitted with rust.
			Flange.	26	59	24,000	32,000	62,500	" " "
			Root...	28	57.3	24,000	31,900	......	" " "
I-Beam........	18	55	Web...	24	48.7	34,000	36,500	65,300	No strain lines.
			Flange.	30.5	63	30,000	33,800	61,200	" " "
			Root...	28.5	55	26,000	32,200	60,700	" " "
Bethlehem I-Beam...	24	73	Web...	23.5	56.5	44,000	50,000	67,500	No strain lines.
			Flange.	27.5	62.3	38,000	42,000	62,600	" " "
			Root...	25.5	61.2	40,000	42,000	64,200	" " "
Channel......	4	5.25	Web...	24.5	61.5	42,000	44,000	63,200	Heavystrainlines
			Flange.	25	49.2	34,000	40,700	64,200	" " "
			Root ..	25	50.4	46,000	50,000	65,500	" " "
Channel......	12	20.5	Web...	27	58.5	34,000	41,600	63,200	No strain lines.
			Flange.	24	58	34,000	43,800	65,300	Strain lines.
			Root..	21	55	40,000	51,500	66,000	" "
Channel......	15	33	Web...	26	63.7	24,000	35,900	56,500	No strain lines.
			Flange.	28	59.3	28,000	32,200	60,300	" " "
			Root..	27	65.7	32,000	35,800	59,500	" " "
Angle........	6	21.9	Web-1.	27	59.6	34,000	39,000	58,300	No strain lines.
			Root..	24	53	34,000	38,600	62,900	" " "
			Web-2.	30	63.3	34,000	34,200	54,200	" " "
Bethlehem H-Section..	10	88.5	Web...	31	50	26,000	31,600	53,800	No strain lines.
			Flange.	36	53	20,000	28,500	52,200	" " "
			Root..	31	51	18,000	26,000	53,100	" " "

the root of a channel received much more work in rolling than the root of an I-beam, and that on account of the proximity of the rolls the finishing temperature is lower. The angle showed no variation. The Bethlehem beam section showed the relation of web, root, flange.

A determination of the carbon and manganese was obtained for three of these shapes, as follows:

		Carbon, per cent.	Manganese, per cent.
4-in. 5.25-lb. Channel	Root.....	0.258	0.473
	Web.....	0.160	0.428
15-in. 33-lb. Channel..............	Root.....	0.195	0.410
	Web.....	0.205	0.467
24-in. 73-lb. Bethlehem I-Beam.....	Root.....	0.151	0.628
	Web.....	0.191	0.708

The rather high percentage of manganese in the Bethlehem beam may possibly account for the high elastic limit shown for the test pieces cut from it. The high values obtained for the 4-in. channel cannot be attributed to its chemical composition, but may be accounted for in part by the thinness of the metal.

Cold Bending of Beams in Straightening (Gagging).—After the beams and channels come from the hot rolls and are allowed to cool, they become bent due to the unequal cooling of the various parts. It becomes necessary therefore to straighten them while cold. This process is known as gagging and often causes the mill scale to come off, causing what will be called "strain lines." The presence of these strain lines indicates that the metal has been overstrained in straightening. Overstrain in this connection is to be understood as meaning that the yield point of the metal has been reached in the portions of the beam where the strain lines appear. This falling off of the mill scale is seen in all tension tests of mild steel when the yield point is reached, providing the mill scale has not been previously removed.

The effect of overstraining in tension upon the elastic properties of iron and steel has been investigated by Muir, the writer, and others,* and it has been found to have a very marked effect. Overstraining a piece in tension or compression reduces its elastic limit to zero or nearly zero, depending upon how much of the yield at the yield point was developed before releasing the load. After a sufficient rest, varying from a few hours to eight or ten days, according to the kind of material, the elastic limit gradually returns and reaches a value somewhat higher than the original. This is illustrated in Fig. 1,† where a piece of steel cut from the web of an I-beam was overstrained, re-tested immediately, and allowed to rest, etc. In this case the piece was considered overstrained when the full yield, *BC*, Fig. 1, had been developed. These phenomena have been obtained by a number of investigators and may be considered well established. Fig. 1 is not introduced as a proof of this interesting behavior of the elastic limit but merely as an example. Since beams are straightened cold at the mills, as indicated by the strain lines, it occurred to the writer that this might be a cause for the low values obtained for the elastic limit in some cases and the high or irregular values in others.

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* *Philosophical Magazine*, June, 1907.

† Acknowledgment is made to the *Engineering Record* for the use of the cuts of Figs. 1, 3 and 4.—ED.

The occurrence of the strain lines indicates that the material has been overstrained. It should then be expected that it would follow, if possible, the same laws that govern overstrained tension test pieces; namely, that its elastic limit would be reduced to zero or nearly so, and that subsequent rest would bring this elastic limit

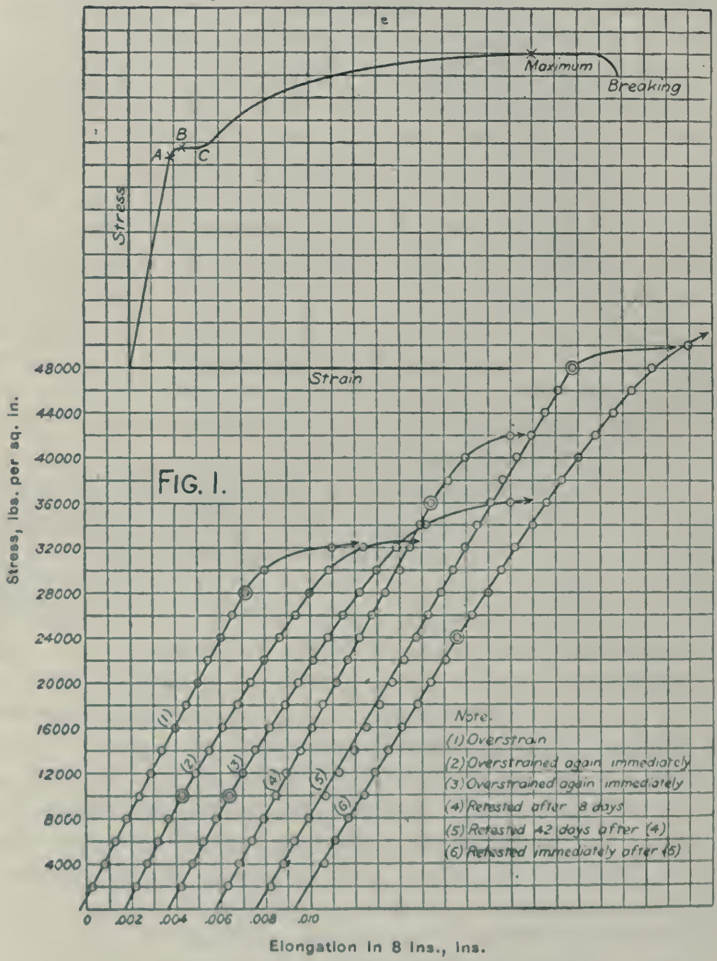

FIG. 1.—Overstrain of Steel from I-Beam.

back to and even above its original value. It seems reasonable to suppose, however, that the recovery will not be as rapid in a beam as in a single test piece; that it will be more rapid in small or thin beams than in large or thick ones; that it will be retarded by the presence of adjacent material that has not been overstrained. This latter supposition will be made clearer by referring to Fig. 2. This photograph shows three 10-in. 25-lb. I-beams as they were received from the mill. No. 1 is marked as "not gagged," but it shows strain lines all along both flanges indicating that it had been straightened. No. 2 was "lightly gagged" and No. 3 "heavily gagged." The depth of the strain lines on the web indicates the spots where the overstraining was most severe. These overstrained portions and the adjacent portions not overstrained indicate that there must be internal stresses in the beam and that recovery from overstrain might be considerably retarded by these stresses.

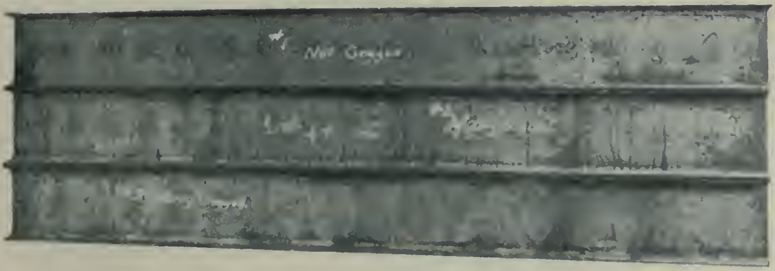

FIG. 2.

In selecting the material from which the tests recorded in Table V were made, particular attention was paid to the evidence of overstrain in the portion of the beam by observing the presence or absence of strain lines. The last column gives a record of these observations. Examining this table with this information in mind, we observe that the web of the 12-in. beam shows no evidence of overstrain while the flange and root do show such evidence. The results for the elastic limit are entirely consistent with the above observations as well as with the amount of work done in rolling. The 15-in. beam was so pitted with rust that all strain lines, had they ever existed, must long since have been obliterated. There is no variation in the elastic limit and since this is a thick heavy beam a very high elastic limit would not be expected. A difference due to work in rolling might be expected, however. No strain lines are shown on the 18-in. beam and the values obtained for the elastic limits are explainable by the work in rolling. The 24-in. Bethlehem beam showed no strain lines and the values for the elastic limit of root and flange are so nearly equal as to call for no particular discussion. The 4-in. channel was evidently heavily gagged and the high values obtained for the elastic limit may be attributed to the complete recovery from the overstrain. That the root is so much higher than the other portions may be due to a greater amount of work in rolling and to the finishing temperature. In the 12-in. channel there seems to have been complete recovery from the overstrain, while in the 15-in. channel and the angle there were no signs of overstrain.

The 10-in. Bethlehem H-section was selected as a type of heavy section, the flange being one inch thick. This particular section had been in storage two years so that all strain lines had disappeared, and the metal should have recovered its normal elastic properties. It is believed that the values of the elastic limit for these pieces are low because of the small amount of work done in rolling and the comparatively high finishing temperatures.

An attempt was made to test the three 10-in. 25-lb. standard I-beams as beams by supporting them at the ends and loading them at the middle. The results of the tests were not satisfactory, since the beams yielded on account of lack of side restraint. The results of the bending tests are for this reason not reported. The bending tests were carried far enough to cause scaling of the beam at the center and were then discontinued. Eighteen-inch lengths were cut from the middle and left-hand end of each beam. Test pieces were cut from the compression flange and root and from the center of the web. These were tested in tension. The results are shown in Table VI and in Figs. 3 and 4.

It is presumed that the end sections of the beam (E) were so slightly stressed that the elastic properties in tension were not appreciably changed. The test pieces cut from the ends should then show the strength of the metal from Beams 1, 2 and 3 irrespective of the bending tests.

TABLE VI.—RESULTS OF TESTS OF PIECES CUT FROM 10-IN. 25-LB. STANDARD I-BEAMS.

Test Piece.	Elongation in 8 ins., per cent.	Reduction of Area, per cent.	Elastic Limit, lbs. per sq. in.	Yield Point, lbs. per sq. in.	Maximum Strength, lbs. per sq. in.
1 E W....	30	62.2	32,000	36,600	57,700
2 E W....	32	60	32,000	36,200	58,100
3 E W....	29.5	58	32,000	36,000	58,400
1 E F....	30	64.2	28,000	33,100	57,800
2 E F....	25.5	62.3	20,000	30,900	58,000
3 E F....	26	60.2	24,000	33,400	58,600
1 E R....	30	62.5	28,000	33,500	58,000
2 E R....	25.5	58	20,000	33,300	59,100
3 E R....	23.5	63	28,000	32,000	59,200
1 C W....	29.5	66	30,000	35,600	53,300
2 C W....	30	63.3	30,000	35,200	57,700
3 C W....	29.5	62.8	28,000	35,100	59,500
1 C F.....	29	58.7	16,000	32,200	58,100
2 C F.....	25	60	28,000	31,800	58,300
3 C F.....	22	62	14,000	35,700	58,200
1 C R....	....	62.8	14,000	29,500	57,200
2 C R....	24.5	58.7	22,000	37,600	60,000
3 C R....	22	61.2	16,000	38,300	59,750

NOTE.—Nos. 1, 2, and 3 refer to the beams shown in Fig. 2. E refers to the end portion; C to the center portion; W, F, and R refer to web, flange, and root.

The time between the gagging of the beams and the cutting of the sections was about three months and the tests were made about a month later. The beams were thin and light and the material probably had sufficient time to recover from the overstrain due to gagging before the tension tests were made. This view is confirmed by the values of the elastic limit for the E pieces given in Table VI.

The center sections (C) were overstrained in the testing machine about one month before being tested in tension. Beam 2 was not overstrained quite as much as Beams 1 and 3. The effect of this overstraining is shown in the low values obtained for the elastic limit of the flange and root pieces. It is belived that this overstrain reproduces very closely the same effect as the gagging at the mill, and the tension tests show that even after the lapse of three or four weeks the material has not regained its original elastic strength. The great difference between the elastic limit and yield point should also be noted. The similarity of the low elastic limit occurring with the normal yield point in the root and flange pieces from the center (C) to those obtained by Professor Mar-

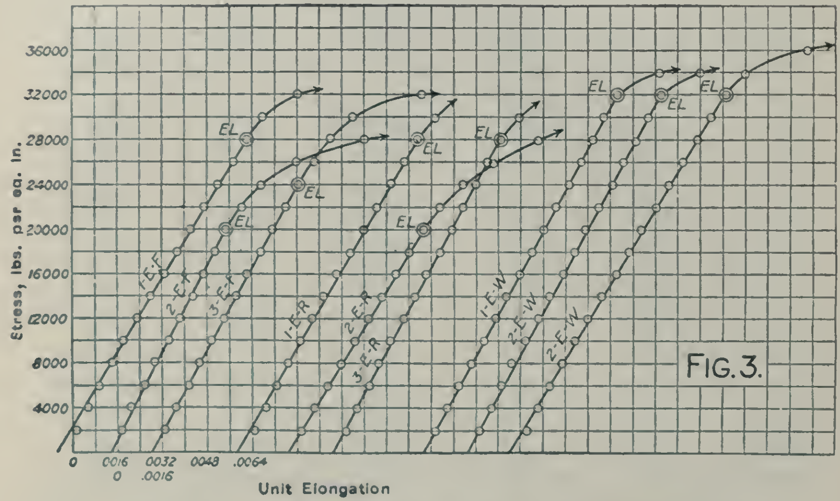

FIG. 3.—Tension Tests of Specimens from I-Beams.

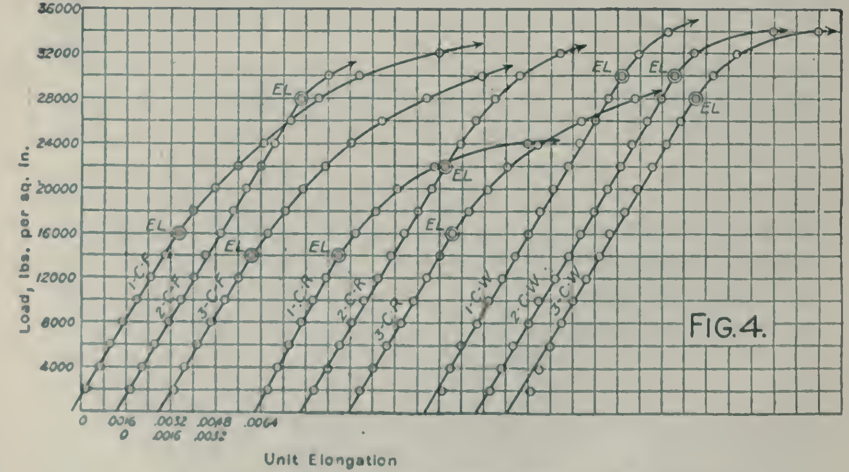

FIG. 4.—Tension Tests of Specimens from I-Beams.

burg* suggests a probable similarity of cause. That is, it seems reasonable to suppose that portions of the beams from which his test pieces were obtained had been recently gagged.

Test pieces were cut from the other side of the compression flange of the C sections. These were heated to about 1600° F. and allowed to cool slowly. They were then finished and tested. The results are as follows:

Test Piece.	Elastic Limit after Heating to 1600° F., lbs. per sq. in.	Elastic Limit, Other Side of Flange Untreated, lbs. per sq. in.
1 C F	24,000	16,000
2 C F	24,000	28,000
3 C F	26,000	14,000

The results show a considerably higher elastic limit after treatment for the pieces from Beams 1 and 3, as would be expected if the low values were caused by overstrain. Test piece 2 C F shows little variation.

To determine whether or not the results of tests obtained by the writer last year could be checked this year by testing pieces from the opposite root and flange (most of these had been saved), test pieces were prepared from the opposite root and flange of Beams H (15-in. 60-lb.) and K (20-in. 65-lb.). The results of these tests are as follows:

Beam.	Test Piece from	Elastic Limit (obtained 1910), lbs. per sq. in.	Elastic Limit (obtained 1911 from opposite flange and root), lbs. per sq. in.
H	Flange	22,500	28,000
	Root	13,500	22,000
K	Flange	24,000	26,000
	Root	16,500	24,000

This seems to indicate a recovery of the elastic limit perhaps from overstrain, although there was at this time (1911) no evidence of strain lines on either of these beam sections. The expense of the preparation of the test pieces has made impossible the testing of pieces from the remaining sections investigated in 1910, although most of these sections have been stored for safe keeping.

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* See *Proceedings*, Vol. IX, 1909, p. 385.

Further tests were made upon material from an 18-in. 55-lb. standard I-beam to determine the effect of gagging. One section of the beam was heavily gagged and another section not gagged at all. Test pieces were cut from the root and web and tested as soon as possible (within a month of the time of gagging). The results are given in Table VII. The average value of the elastic limit for the gagged pieces is 19,500, and that for the pieces not gagged 27,000 lbs. per sq. in.

TABLE VII.—RESULTS OF TESTS OF PIECES CUT FROM AN 18-IN. 55-LB. STANDARD I-BEAM.

Test Piece.	Treatment.	Elongation in 8 ins., per cent.	Reduction of Area, per cent.	Elastic Limit, lbs. per sq. in.	Yield Point, lbs. per sq. in.	Maximum Strength, lbs. per sq. in.
Web....	Gagged	23	57	20,000	32,800	60,100
Web....	"	28.5	59	18,000	30,600	59,000
Root....	"	20.5	56	22,000	36,600	61,100
Root....	"	26	50	18,000	36,100	62,000
Web....	Not gagged	30	55	28,000	33,300	58,700
Web....	" "	28	56	30,000	34,000	59,000
Root....	" "	28.5	56	26,000	33,100	59,700
Root....	" "	30	55	24,000	32,000	60,500

All the evidence seems to show that overstraining a beam lowers the elastic limit of the metal in the beam temporarily but that this elastic limit returns after a period of rest. The time at which the normal elastic limit has again been reached depends upon the amount of overstrain, the size of the beam (height and thickness), and probably somewhat upon the chemical composition of the material. From the few tests made it might be said that the time necessary for complete recovery of 10- to 20-in. beams varies from three months to a year.

It has been noticed that nearly all the finished test pieces were slightly bent, supposedly due to the internal stresses, and an attempt was made to find some relation between the condition of the test piece as to bending and its elastic limit. With the Ewing extensometer used in making the recent tests this bending due to internal stresses does not seem to affect the results. It was noticed that the pieces annealed before finishing (1 C F, 2 C F, and 3 C F) showed no signs of bending.

Conclusions.—The above review of the tests thus far made on steel cut from different parts of the profile of structural shapes and a study of the conditions of manufacture of such shapes leads to the following conclusions:

1. That the tensile strength, percentage of elongation, and percentage of reduction of area are practically not affected by chemical composition or treatment during or after rolling; that the yield point is affected but slightly; that the elastic limit is affected, but not seriously when the proper precautions are taken.

2. The chemical composition of open-hearth steel beams is so uniform throughout as to cause no practical difference in the strength of metal cut from different parts of the profile.

3. If all parts of the profile of a beam were finished at the same temperature, it would be expected that the root of the web would have a slightly lower elastic limit than the center of the web or the flange because of the smaller amount of work done upon it in rolling. But the flange and web, being thinner, are finished at a slightly lower temperature than the root and so have their elastic strength increased still more. The web probably receives more work than the flange in rolling.

4. Overstraining structural shapes lowers the elastic limit of the metal but not permanently, the elastic limit returning to and above its original value when the material is allowed to rest. Cold straightening of beams probably accounts for some low values of the elastic strength obtained in recent investigations. The evidence of recovery should give confidence to the users of these materials.

The writer wishes to thank Mr. H. G. Parker, Mr. E. P. Peterson, and Mr. H. R. Winter, members of the Senior Class of the Worcester Polytechnic Institute, for their faithful services in carrying out many of the tests recorded above.

A STUDY OF THE ELASTIC PROPERTIES OF A SERIES OF IRON-CARBON ALLOYS.

By C. R. Jones and C. W. Waggoner.

The original object of this study was to determine whether any relation existed between the variation of the tensile properties and certain known magnetic properties[*] of this series of steels. The steels were made expressly for experimental purposes and the tests were made under the direction of the writers by George G. Crewson and T. S. Patterson as the basis of a thesis in Mechanical Engineering at West Virginia University.

The steels were made by the crucible process and after being rolled were allowed to cool in air. The chemical analysis of each specimen is given in Table I. The steel marked (x) does not belong to this series, but is a remarkably pure piece of iron and was used because of its low carbon content. The small variation of all the elements in the series proper, except carbon, is worthy of note, and microscopic study has shown the steels to be quite homogeneous. The carbon varies from 0.60 to 1.37 per cent., while the phosphorus varies from 0.01 to 0.013 per cent., the silicon from 0.14 to 0.19 per cent., the manganese from 0.14 to 0.17 per cent., and the sulphur from 0.012 to 0.014 per cent.

It is a matter of regret that the range in the percentage of carbon was not greater and also that only a single specimen of each steel was available; but owing to the purity of the steel it is believed that the results are worthy of publication.

All the test pieces were made from rods 0.625 in. in diameter and were turned to a diameter of 0.46 in. for a length of 2 ins., and fitted with $\frac{5}{8}$-in. threads at the ends. After being turned to size within 0.001 in., the steels were annealed for two hours at 1000° C. in an electric resistance furnace and allowed to cool in the furnace. This temperature was chosen because the steels used in the magnetic determination had been annealed in a similar way. An hour was required to bring the furnace up to the specified temperature. The rate of cooling was not observed. After cooling, the coating of oxide was removed by light filing.

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[*] Waggoner, *Physical Review*, Vol. XXVIII, 1909, p. 303

Fig. 1 shows the stress-deformation diagrams for the entire series, and the calculated results are given in Table I. These results are illustrated graphically in Fig. 2, the percentage of carbon being plotted as the abscissas, with the ultimate strengths, elastic limits, percentage of total elongations, and percentage of reductions of area as ordinates respectively. For purposes of comparison Tables II to IV, inclusive, have been compiled from the results of other investigators along the same or similar lines. These results have also been plotted in Fig. 2.

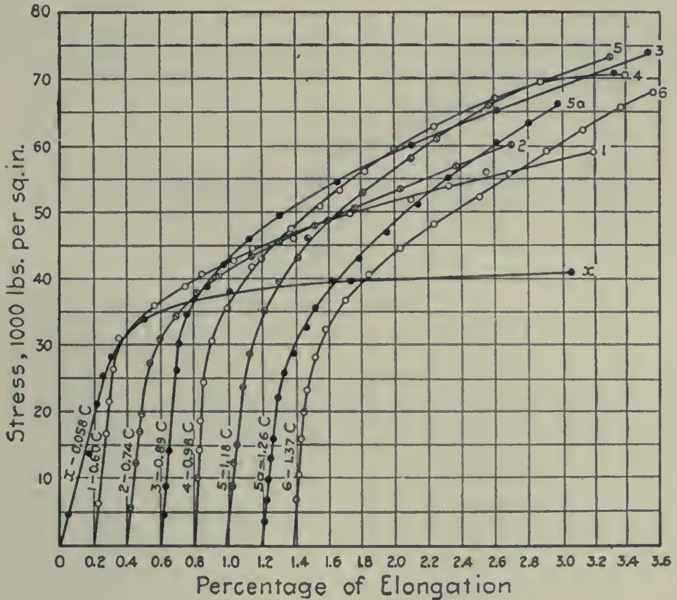

FIG. 1.—Stress-Deformation Diagram for Steels with Varying Percentages of Carbon.

Taking up the series of curves in the order in which they appear in Fig. 2, it will be noted that all the maximum strength-carbon curves are similar in form and similar to the

TABLE I.—RESULTS OF WRITERS' TESTS ON STEELS ANNEALED FOR TWO HOURS AT 1000° C. AND COOLED IN FURNACE.

Mark	Chemical Analysis of Alloy, per cent.					Maximum Strength, lbs. per sq. in.	Breaking Load, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
	C	Mn	Si	P	S					
I	0.058	0.071	0.008	Trace	.....	45,800	22,800	26,000	28.0	75.7
1	0.6	0.14	0.15	0.013	0.012	76,500	72,750	28,500	20.0	29.5
2	0.74	0.14	0.16	0.012	0.013	81,250	79,600	28,500	14.0	18.2
3	0.89	0.155	0.19	0.010	0.013	95,250	93,100	32,000	13.5	12.6
4	0.98	0.15	0.16	0.012	0.013	94,600	94,600	27,500	8.0	11.3
5	1.18	0.14	0.14	0.012	0.013	90,400	90,400	25,500	3.0	5.96
5	1.26	0.17	0.16	0.012	0.014	84,100	84,100	23,500	2.5	2.38
6	1.37	0.16	0.19	0.011	0.012	89,600	89,600	23,500	Broke in Threads.	

TABLE II.—RESULTS OF HARBORD'S TESTS ON STEELS ANNEALED FOR 30 MINUTES AT 900° C. AND COOLED IN FURNACE.

(Metallurgy of Steel, F. W. Harbord, p. 583.)

Mark.	Chemical Analysis, per cent.					Maximum Strength, lbs. per sq. in.	Breaking Load, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
	C	Mn	Si	P	S					
1	0.130	0.185	0.020	0.011	0.02	48,100	......	29,100	45.0	71.16
2	0.180	0.200	0.024	0.022	0.02	62,350	......	35,500	33.5	60.24
3	0.254	0.190	0.032	0.025	0.02	66,000	......	38,300	32.5	54.84
4	0.468	0.160	0.072	0.016	0.025	72,700	......	40,400	28.0	46.26
5	0.722	0.217	0.098	0.015	0.029	96,780	......	......	16.5	25.86
6	0.871	0.190	0.080	0.019	0.019	112,900	......	......	11.2	15.84
7	0.947	0.181	0.081	0.013	0.019	114,700	......	......	10.0	15.42
8	1.306	0.180	0.050	0.015	0.017	99,760	......	......	5.5	5.58

TABLE III.—RESULTS OF ARNOLD'S TESTS ON STEELS ANNEALED FOR 72 HOURS AT ABOUT 1000° C. AND COOLED IN FURNACE.

(Metallurgy of Iron and Steel, F. W. Harbord, p. 584.)

Mark.	Chemical Analysis of Alloy, per cent.					Maximum Strength, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Elongation in 2 ins., per cent.	Reduction of Area, per cent.
	C	Mn	Si	P	S				
1...................	0.08	0.02	0.03	0.02	0.03	41,690	19,750	52.7	76.7
1*a*...................	0.21	0.05	0.05	0.02	0.03	47,610	20,200	42.3	65.7
2...................	0.38	0.08	0.03	0.02	0.02	56,040	21,280	35.0	50.6
3...................	0.59	0.10	0.07	0.02	0.02	71,400	25,460	22.0	23.3
4...................	0.89	0.09	0.03	0.02	0.02	82,180	37,650	4.5	4.2
5...................	1.20	0.15	0.07	0.02	0.03	73,650	36,280	6.0	4.9
6...................	1.47	0.13	0.08	0.02	0.01	50,000	24,200	19.0	17.7

TABLE IV.—RESULTS COMPILED FROM CAMPBELL'S TESTS ON STEELS SIMILAR IN COMPOSITION TO THOSE RECORDED IN THE PRECEDING TABLES, HEATED TO 950° C. AND COOLED IN FURNACE.

(From "Heat Treatment of Steels," *Proceedings*, Vol. VI, 1906, p. 211; Vol. VII, 1907, p. 240.)

Mark.	Chemical Analysis, per cent.					Maximum Strength, lbs. per sq. in.	Elastic Limit, lbs. per sq. in.	Total Elongation, per cent.	Reduction of Area, per cent.
	C	Mn	Si	P	S				
3...................	1.72	0.075	0.013	0.29	0.018	95,000	45,200	7.5	6.3
4...................	1.61	0.19	0.013	0.19	0.013	96,350	48,500	7.5	6.8
5...................	1.04	0.12	0.012	....	0.017	111,500	46,800	10.5	10.9
6...................	0.70	0.068	0.012	0.141	0.019	90,500	39,700	16.5	20.5
*x**	0.42	0.20	.....	.....	.....	64,420	41,800	19.4	44.5

* From *Proceedings*, Vol. VIII, p. 248; heated to 1050° C. and cooled in furnace.

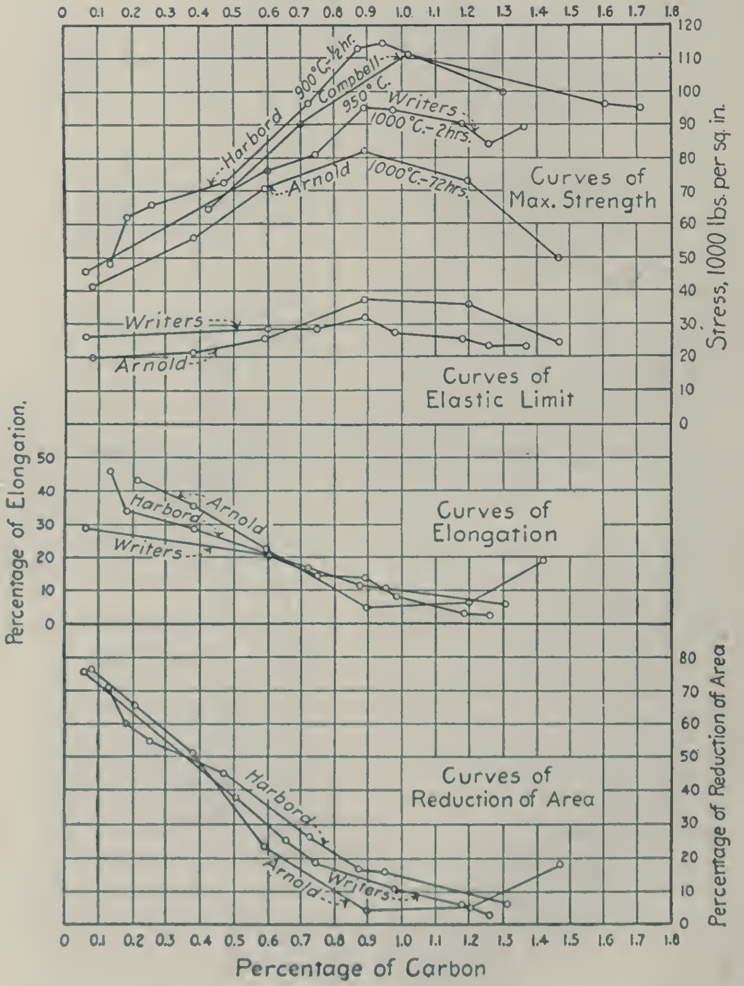

FIG. 2.—Variation of Physical Properties of Steel with Carbon Content.

tensile strength - carbon curve predicted by H. M. Howe* from purely theoretical considerations of the microscopic constituents. His curves show a maximum at or near the eutectoid ratio for the iron-carbon diagram. Stoughton† has suggested that the intimate mixture of the crystalline constituents when the steel is near the eutectoid ratio has the effect of increasing the molecular attraction between the crystals, thus raising the maximum strength of the material; for below the eutectoid the pearlite is surrounded by a network of soft ferrite while above it is surrounded by a network of cementite, both of these networks having a weakening effect upon the attraction between the molecules of the crystals. Although the experimental maximum seems to occur at a lower percentage of carbon than predicted by Howe, the curves show a strong confirmation of the theories of Howe and Stoughton. The results of the tests made by the writers would indicate that pure iron should have a tensile strength of about 40,000 lbs. per sq. in., and that the tensile strength of the series of annealed crucible steels tested increased about 630 lbs. for each 0.01 per cent. of carbon up to 0.89 per cent., after which the strength gradually decreased.

In addition to the test recorded in Table II, Professor Harbord made another test on the same series of steel which he soaked at at 750° C. for twelve hours. The maximum strength - carbon curve for this heat treatment follows the same general law and up to the point of maximum strength agrees closely with that of the writers. It is somewhat more irregular, however, crossing the writers' curve three times and falling away more rapidly after reaching a maximum. It is omitted here to avoid confusion.

The difference between the several maximum strength - carbon curves is not greater than could be reasonably accounted for by differences in heat treatment and chemical composition. The curves fall in the order in which they would be logically placed by the methods of annealing employed, and Harbord's, the writers', and Arnold's curves appear in the order of their respective percentages of manganese.

The elastic limit - carbon curves seem to follow the same general law as the maximum strength - carbon curve, the maximums for the curves plotted occurring at the same percentages of carbon. The variation of the stresses at the elastic limit, however, is much less marked than the variation of the maximum strengths, and in other tests that the writers investigated the results were either irregular or showed more than one critical point.

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*Howe's "Iron and Steel and other Alloys."

†Stoughton's " Metallurgy of Iron and Steel."

The curves for total elongation and reduction of area are similar, and indicate that there is a critical point in each corresponding to the percentage of carbon which gives maximum strength. This critical point may represent a minimum value or a marked change in the direction of the curve.

A comparison of the curves between the tensile strength and carbon, and those for the magnetic hysteresis loss,* show a striking similarity as to shape. The maximum occurs a little later in the magnetic hysteresis - carbon curve than in the tensile strength - carbon curve, both maxima occurring near the eutectoid ratio of the iron-steel diagram. The similarity in shape between these curves would indicate a close relationship between the magnetic hysteresis and the tensile strength of annealed steel. If we assume that the hysteresis loss is due to the reversal of polarity of the ferrite molecule in the steel when placed in an alternating magnetic field, it seems probable that the increase in magnetic hysteresis up to the eutectoid ratio may be due to the decrease in free ferrite and the increase in pearlite, the opposition to reversal of magnetism increasing with the decreasing percentage of free ferrite. The occurrence of the maximum of hysteresis at the eutectoid ratio may be due to the very intimate mixture of solid ferrite and cementite—so intimate, in fact, that the reversal of magnetism is obtained only by the application of a large magnetizing force.

The decrease in the hysteresis loss with increased cementite above the eutectoid ratio would seem to be due to the freeing of the ferrite molecules in the solid mass of pearlite by the net-work of excess cementite which surrounds the pearlite crystals and separates them from each other, making it easier for the ferrite molecules to follow the alternations of the impressed magnetic field. This breaking up of solid pearlite of course increases with the excess cementite or total carbon.

A further similarity between the magnetic hysteresis and tensile strength is shown in the fact that mechanical working of a steel produces an increase in both properties.[*] This fact follows naturally from the explanations given for each property above.

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* Waggoner, *Physical Review*, Vol. XXVIII, 1909, p. 307.

Hadfield[†] has shown that the effect of the temperature of liquid air on the mechanical properties is to increase the strength at the lower temperatures. Cooling steels to the temperature of liquid air has been shown to produce an increase in the hysteresis loss,[‡] when at high magnetizations, over the loss at ordinary temperatures. This increased density of the steels produced by the low temperatures seems to have the same effect on both the magnetic hysteresis and the maximum strength.

The magneto-striction of this series of steels has been studied by Dorsey,[§] who found that $\Delta l/l$ maximum decreases with the carbon content to about 0.9 per cent. carbon and then increases. Assuming that magneto-striction is nothing more than the attempt on the part of the ferrite molecules to orient themselves in a certain way under the influence of a very high magnetic field, the shape of the magneto-striction - carbon curve with its minimum near the eutectoid may be explained in a manner similar to that given above for the tensile strength and magnetic hysteresis. In the case of magneto-striction we have to deal not only with the difficulty of orientation, but also with the fact that the material actually elongates under the magnetic force, thus bringing into account its tensile properties.

From the above study it would seem that in general the factors which influence the tensile strength of an annealed steel also influence, in the same way, its magnetic hysteresis. In the series of steels tested those factors are: the carbon content, the mechanical working, and the increased density caused by extreme low temperature.

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[*] See Waggoner, *Physical Review*, Vol. XXVIII, 1909, p. 399, fo. effect upon the magnetic hysteresis of forging steel.

[†] Hadfield, *Journal, Iron and Steel Institute*, Vol. 67, 1905, p. 147.

[‡] Waggoner, *Physical Review*, Vol. XXVIII, 1909, p. 397.

[§] Dorsey, *Physical Review*, Vol. CLXX, 1910, p. 715.

SOME RECENT DEVELOPMENTS IN TESTING BOILER TUBES.

By F. N. Speller.

It is generally understood that much depends on the boiler tube, especially in locomotive service, this being relatively the weaker member, so that the usefulness of an engine is frequently determined by the strength of its weakest tube.

A variety of specifications have been drawn up, with that of the American Railway Master Mechanics Association as a basis, to regulate the quality of the material and the manufacture of locomotive boiler tubes. At the present time each road has a specification of its own, usually differing in some points from the others, but in general they require a flanging, flattening or crushing-down test on one out of each lot of 100 or 250 tubes. In the manufacture of lap-welded tubes, we have found it necessary, in order that the tubes will be, as far as possible, uniformly satisfactory, to make such tests on each end of every tube, and have designed a special machine to make these tests on the crop ends as they are cut off.

Fig. 1 is made from a photograph of this machine showing the crop end or short piece from a tube in the act of being flattened down in the horizontal grips, which hold the piece while a flange is turned by a die. At the same time the sample is crushed down in the direction of the axis of the tube as shown in Figs. 2 and 3. The machine is operated by hydraulic power, both the horizontal and vertical cylinders being controlled by the same lever. The flanging die is preferably magnetized by a coil so as to keep the sample in place until the grips take hold. By placing the machine in a convenient position, it may be quickly operated by the man who cuts off the crop end, thereby enabling him to separate tubes which are not welded or which show other defects. While experimenting with flanging dies for this machine, we received one from James H. Gibboney, Chief Chemist, Norfolk and Western Railway Company, which proved quite satisfactory for the purpose. This test, made on every tube, makes it practically certain that the weld is as strong as other parts of the tube, and that the physical properties are uniformly up to the standard required.

Among the various requirements in specifications written for locomotive boiler tubes, there are naturally in each many excellent provisions, some of doubtful value, and a few clauses which tend to produce the opposite result from that for which the specification was written.

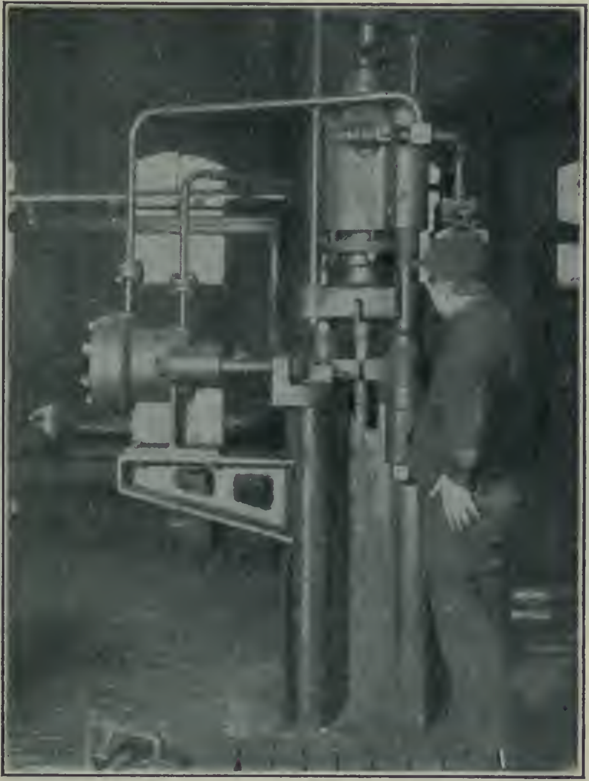


FIG. 1.—Flanging Machine, Sample of Tube at First Stage of Test.

Locomotive tubes, whether seamless or lap-welded, must sooner or later be safe-ended; hence the welding quality of the metal should be one of the first considerations in manufacture. Some specifications now written restrict the chemical composition in some particulars, so as to hamper the manufacturer in making a good welding steel. There is no difficulty in making steel with a maximum of 0.03 per cent. of phosphorus if necessary, but there is reason to believe that 0.05 per cent. of phosphorus is a more reasonable maximum limit which does no harm, and, with other conditions the same, will give a tube better adapted to service and much more easily welded.

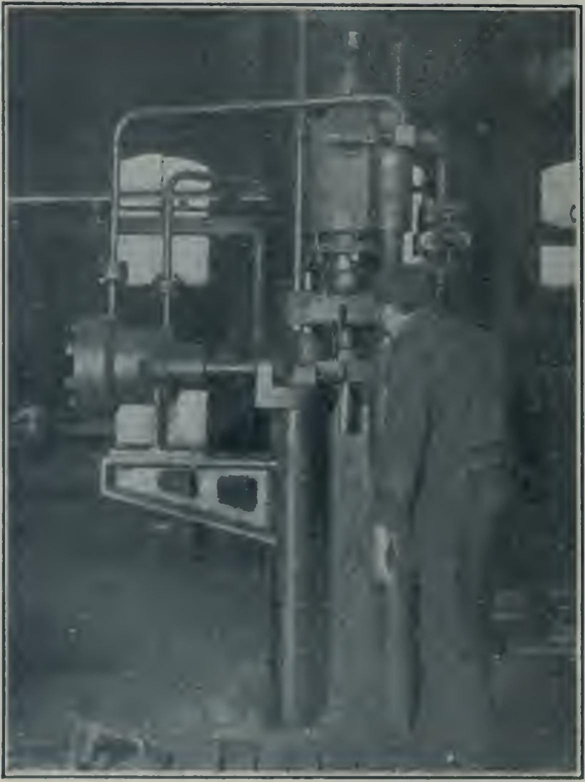
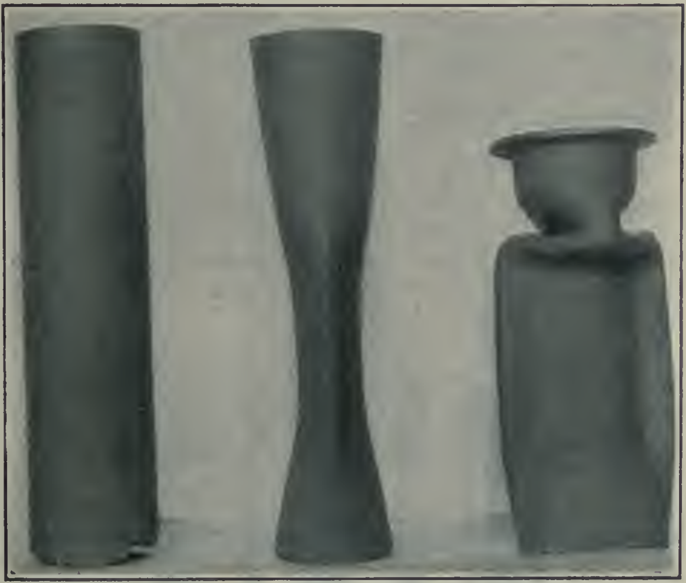


FIG. 2.—Flanging Machine, showing Finished Test.

Another restriction which experience teaches is operating against the best quality in locomotive boiler tubes is unreasonable sulphur requirements. The highest sulphur allowed in samples taken from individual tubes is, in some cases, 0.035 per cent., which means that in the ladle test it must not exceed 0.030 per cent. With producer gas this means that the heat must often be held and a heavier burden of lime carried, which tends to render the steel "dry" in welding and more liable to be crystallized or burned. If there is any advantage in a closer sulphur specification than say 0.05 per cent. on samples taken from individual tubes, we would be glad to learn what is gained thereby from those who have had more experience. Personally, I have not found a case of failure which could be attributed to the sulphur being as high as 0.05 per cent.; on the other hand, there are undoubtedly countless numbers of tubes giving good service which contain close to this amount of sulphur.


(*a*) (*b*) (*c*)

(*a*) Crop End before Testing. (*b*) Same after Flattening.
(*c*) Finished Test.

FIG. 3.

Analyses of the surface of beads taken from tubes after being in the boiler some time, show that sulphur is absorbed from the hot flue gases, so that if there is any advantage in using steel of 0.030 per cent. sulphur, it would appear to be only temporary. The results of this investigation are given in the diagram in Fig. 4. The engines from which these tubes were taken had been operating on different roads under widely different conditions, but in each case the tubes had all given equally good service and were being removed for safe-ending. It also appears from a comparison of the sulphur taken up by individual tubes under the same con-

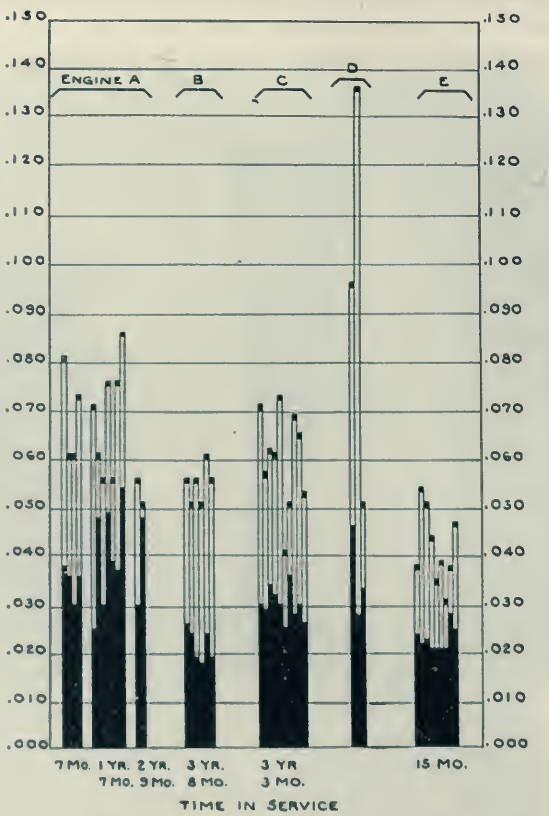


FIG. 4.—Diagram showing Amount of Sulphur absorbed by Tube Ends from Flue Gases in Locomotive Firebox. Lower Shaded Portion = per cent. original Sulphur; Upper Open Portion = increase of Sulphur in Bead.

ditions, that there is no consistent relation between the original sulphur content and the amount absorbed; so it does not follow, because the tube was originally low in sulphur, that it would therefore show comparatively low sulphur after being subjected to the action of the hot flue gases; the results rather suggest that the low-sulphur tubes are more susceptible to sulphurizing by the hot flue gases.

A study of records in lap welding may throw some light on the relative influence of variations in sulphur content. For example, two heats which had been re-phosphorized gave the following welding records, each piece being tested in the flanging machine after the first run through the welding furnace and rejected if there was any indication of opening at the seam:

Heat No.	Number tested.	Chemical Analyses, per cent. S	P	Mn	C	Per cent. not welded.
3432	1272	0.045	0.036	0.43	0.105	14.7
30522	1401	0.027	0.037	0.47	0.113	20.2

The average of nine heats of steel which ran 0.03 per cent. or less of sulphur showed 20 per cent. more rejections on account of bad welds than eight heats where the sulphur ran over 0.04 per cent., these heats being nearly the same in other respects.

As to the effect of foreign elements in these amounts on the corrosion of steel under ordinary conditions, we have no evidence that their presence has any decided effect one way or the other in well-made steel, and the grade of soft steel made exclusively for the manufacture of pipe certainly belongs to this class. The degree of uniformity obtained, even between individual Bessemer heats made for this purpose, is, it is safe to say, as good as that shown in the records of the most highly refined product of the open-hearth furnace, and uniformity, both physical and chemical, is undoubtedly a large factor both in welding and in corrosion.

The supposed beneficial effect of great purity has, I believe, been greatly over-estimated. Some time ago the writer found by using the sensitive ferroxyl test that cross-sections of steel, which had all the variations due to segregation, would not show a regular difference of polarity under repeated tests made on the same section, the explanation evidently being that external influences such as finish and accidental irregularities in oxidation predominated and overpowered the much smaller differences in potential due to irregularities in the metal itself.

We believe it would be to the advantage of all concerned if a standard specification was agreed upon for boiler tubes, in which there could be no objection to a test on the ends of each tube along the lines described above, provided the chemical requirements were not unnecessarily restrictive.

THE BEHAVIOR OF CAST ZINC UNDER COMPRESSION.

By J. C. Trautwine, Jr.

At a recent meeting of the Engineers' Club of Philadelphia, a member stated that *The Civil Engineer's Pocket-Book* gave 7,500 lbs. per sq. in. as the crushing strength of "spelter" or cast zinc, and quoted Stoney (*Theory of Stresses*, 1886, Art. 352, p. 382) as saying: "In my own experiments I found that cast zinc will spread out to any degree under severe pressure, but it will bear 5 or 6 tons (11,200 to 13,440 lbs.) per sq. in. without any very appreciable change of shape."

Per contra, Mr. H. H. Quimby, the City's Engineer of Bridges, describing tests by the Philadelphia Bureau of Surveys in connection with the proposed use of cast zinc for key-plates in his new steel-concrete arch bridge over the Pennsylvania Railroad at Forty-second Street, said: "I tested a 4-in. cube of cast zinc by first running the pressure up to 6,000 lbs. per sq. in. at the lowest speed of the machine. As the scale beam would not stay up long under this, I gradually lowered the load to 2,000 lbs. per sq. in., and left it so for the night. In the morning the beam was down, though the elasticity of the block was still carrying considerable pressure."

At the first opportunity I looked up the matter, and found no mention of anything like 7,500 lbs. per sq. in. as the ultimate compressive strength of zinc, either in *Trautwine* or elsewhere; but found, in *Trautwine* (19th edition, 1909, p. 921) the following statement of loads and corresponding deformations: "Spelter or Zinc (cast). By writer. A piece 1 in. square, 4 ins. high, at 2,000 lbs. was compressed 1/400 of its height; at 4,000, 1/200; at 6,000, 1/100; at 10,000, 1/38; at 20,000, 1/15; at 40,000 yielded rapidly and broke into pieces."

Tabulated, these data appear as in Table I:

TABLE I.

A	B	C
Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.	Modulus of Elasticity = $\frac{B}{A}$, lbs. per sq. in.
1/400 = 0.0025	2,000	800,000
1/200 = 0.0050	4,000	800,000
1/100 = 0.0100	6,000	600,000
1/38 = 0.0263	10,000	380,000
1/15 = 0.0667	20,000	300,000
Not recorded	40,000 (ultimate)	......

I have been unable to find any further data respecting this experiment.

This statement first appeared in our Fifth Edition, 1881, and it has since remained unchanged. Prior to that edition, we gave no figures for the compressive resistance of zinc.

In the *Report of the United States Board Appointed to Test Iron, Steel and other Metals*, 1881, Volume 2, page 100, is given a tabular record of an experiment upon a cast-zinc cylinder, 2 ins. long, 0.625 in. in diameter, of which the essentials are given in the first two columns of Table II, while the third column gives moduli of elasticity, obtained by dividing the unit stresses given in the first column by the unit deformations given in the second.

Professor Robert H. Thurston, who was a member of the Board, states (*The Materials of Engineering*, Vol. 3, p. 296): "All the zinc thus tested by the Author was very pure, and made from New Jersey calamine." Professor Thurston at once proceeds to say: "the effects of varying time and rapidity of strain are observable in zinc, as in tin;" but, in the Board's report, as in our own statement, I am unable to find any data as to speed of applying the loads, or as to the times during which the specimens were left under successive loads.

In the Board's experiments (see *Report*, 1881, Vol. 1, p. 313) the compressions were estimated by noting the number of revolutions of the handle necessary to produce a certain compression.

In Fig. 1, our data, up to 20,000 lbs. per sq. in., are shown

Table II.—Cast Zinc in Compression.

Tests of Metals, *Report of United States Board*, 1881, Vol. 2, p. 100.

Zinc, from New Jersey calamine (Thurston, *The Materials of Engineering*, Vol. 3, p. 296), cast in iron mold; Cylinder, 2 ins. long, 0.625 in. in diameter.

A	B	C
Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.	Modulus of Elasticity = $\frac{B}{A}$, lbs. per sq. in.
0.0015	3,259	2,170,000
0.0025	4,889	1,790,000
0.0055	6,519	1,190,000
0.0110	8,149	740,000
0.0185	9,778	528,000
0.0255	11,408	448,000
0.0340	13,038	384,000
0.0400	14,668	367,000
0.0510	16,297	319,000
0.0595	17,927	301,000
0.0720	19,557	272,000
0.0875	21,187	242,000
0.1065	22,816	214,000
0.1150	24,446	212,000
0.1215	26,076	214,000
0.1545	27,706	179,000
0.1715	29,335	171,000
0.1855	30,965	167,000
0.2060	32,595	158,000
0.2180	32,595*	149,000
0.2440	34,225	140,000
0.3335	32,595†	98,000

by the solid line. Beyond that point, the compressions are not recorded; but I have added a dash-and-dot line, to indicate what may perhaps be taken as the probable approximate compressions, in our case, up to 40,000 lbs. per sq. in., the load causing rupture; arriving at these figures by comparison of our curve with that representing the experiments of the United States Board.

It will be noticed that, as with ductile materials in general, there is glorious liberty of choice as to elastic limit and modulus of elasticity; and that, ordinarily, there is no ultimate limit of strength; the material (at least when in short pieces) generally flattening down indefinitely, as remarked by Stoney, although our relatively slender column 1 in. square, 4 ins. long, "broke into pieces" at 40,000 lbs. per sq. in. The United States Board arbitrarily designates, as "crushing" load, that which reduces the original length by 10 per cent. (22,000 lbs. per sq. in., in the Board's case). At 32,505 lbs. per sq. in. (after reduction from 34,225) the Board's cylinder, $\frac{5}{8}$ in. in diameter, 2 ins. long, bent into a reversed curve, and was removed. Burr (*Materials of Engineering*, p. 389), referring to his statement of the United States Board's experiments upon copper-zinc alloys in compression, says: "The values of E_1 (ratios of stress over strain) are computed for about one-quarter the ultimate resistance. This ratio is so very variable for different intensities of stress that these alloys can scarcely be said to have a proper elastic limit."

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* Continued one minute.

† Piece bent, removed.

Mr. Quimby states, in the tests of the Philadelphia Bureau of Surveys, above mentioned, "the material showed some elasticity at every reduction of pressure," and that, according to his recollection, in crushing the 4-in. cube down to about $2\frac{3}{4}$ ins. thickness, the pressure went up to about 900,000 lbs. gross, or 56,250 lbs. per sq. in. of original section. He adds, "the block was then still almost square in horizontal section, but bulged at mid-thickness, say $4\frac{1}{2}$ ins. square at the contact faces, and 5 ins. square at mid-thickness."

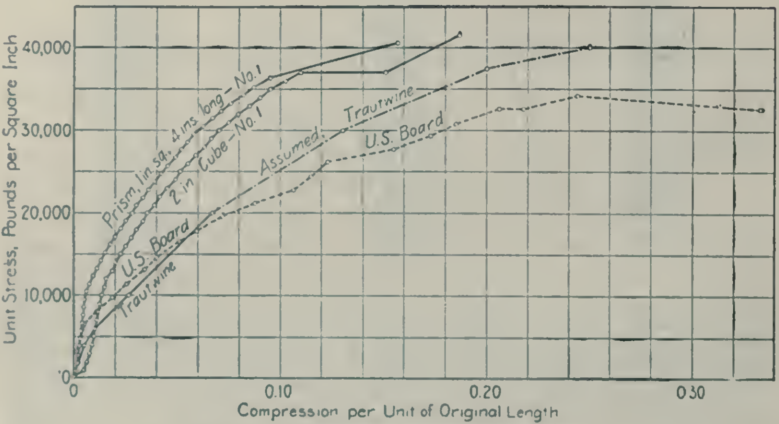

FIG. 1.—Compression-Stress Diagrams.

In order to obtain further light upon this question, the writer had two 2-in. cubes and two prisms 1 in. square and 4 ins. long, cast from pigs and carefully planed. These pieces were cast by the McHatton Smelting Company, Fifteenth Street and Washington Avenue, Philadelphia, June 24, 1911; planed by John Eppler Machine Works, 629 Filbert Street, Philadelphia, June 26, 1911. These were tested by Messrs. W. Purves Taylor and G. Wise, in the cement-testing laboratory of the Philadelphia Bureau of Surveys, on June 27, 1911. As shown by Tables III to VI, inclusive, and as plotted in Fig. 1, they showed considerably higher resistances than did the tests above mentioned; the prisms, curiously, showing higher resistances than the cubes, although Prism 1, which was the more severely tested of the two prisms, bent badly, in both directions, and was evidently failing as a crippled column. Neither of these four samples was broken.

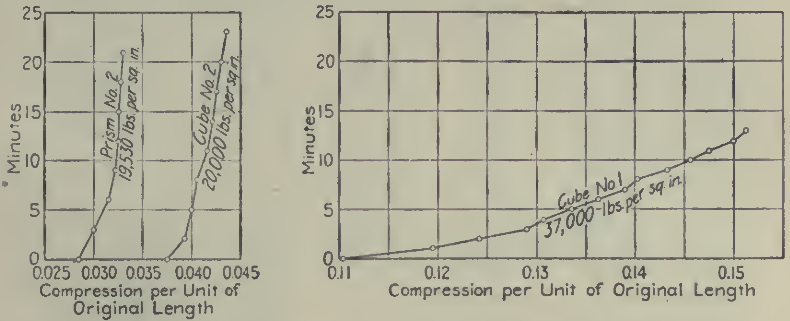

FIG. 2.—Compression-Time Diagrams, Constant Load.

Time tests, under constant loads, showed some increase of resistance with time. (See Tables III to VI and Fig. 2.)

As with ductile materials in general, no well-defined elastic limit was observed; and the modulus of elasticity diminished, throughout the test, as the load increased.

Mr. R. H. Chapman, Jr., of the Bureau of Surveys Testing Laboratory, analysed material cut from these specimens. The analysis resulted as follows:

Zinc	95.04	per cent.
Lead	2.08	"
Copper	1.28	"
Tin	0.89	"
Iron	0.51	"
Antimony	0.06	"
Cadmium	0.08	"
Total	99.94	"

TABLE III.—CAST-ZINC PRISM 1 (1 IN. SQ., 4 INS. LONG).

Diameters, ins., 1.05, 1.04, 1.02, 1.02, 1.01. Average 1.028
1.01, 1.01, 1.01, 1.04, 1.03. Average 1.020

Length, 4.012 ins. Section area, sq. in., 1.0486

Time.	Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.	Time.	Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.
9.50	0	95.4	10.01	0.03600	22,900
	0.00050	954		0.03900	23,830
	0.00175	1,808		0.04200	24,800
	0.00200	2,862		0.04500	25,750
	0.00250	3,816		0.04850	26,700†
				0.05175	27,660
9.53	0.00300	4,770			
	0.00350	5,724	10.04	0.05500	28,610
	0.00400	6,678		0.05840	29,550
	0.00450	7,632		0.06250	30,540
	0.00475	8,586		0.06700	31,450
	0.00525	9,540		0.07125	32,410
	0.00600	10,500			
	0.00750	11,450	10.08	0.07625	33,400
				0.08150	34,350‡
9.56	0.00900	12,400		0.08700	35,300
	0.01075	13.360		0.09475	36,280
	0.01275	14,330			37,200§
	0.01500	15,270			38,150
	0.01750	16,210*	About		
	0.02000	17,170	10.15	0.15750	40,550¶
	0.02275	18,110			
9.59	0.02525	19,080			
	0.02775	20,030			
	0.03050	21,000			
	0.03300	21,920			

Specimen removed. Length 3.37 ins. Badly bent; not broken.

* Blistering noticed. † Bending noticed. ‡ Reverse bending noticed.
§ Deflectometer removed. ¶ Maximum.

TABLE IV.—CAST-ZINC PRISM 2 (1 IN. SQ., 4 INS. LONG).

Diameters, ins., 0.998, 1.010, 1.010, 1.010, 1.030. Average............ 1.0116
1.000, 1.000, 1.010, 1.020, 1.030. Average............ 1.0120

Length, 4.012 ins. Section area, sq. ins., 1.0237

Time.	Interval, Mins.	Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.
10.29	..	0	97.8
10.31	..	0.02150	17,070*
10.32	..	0.02550	18,570
10.33	0	0.02850	19,530
10.36	3	0.03010	19,530
10.39	6	0.03150	19,530†
10.42	9	0.03210	19,530
10.45	12	0.03240	19,530
10.48	15	0.03250	19,530
10.51	18	0.03275	19,530
10.54	21	0.03300	19,530

Test discontinued; length, 3.88 ins.

* Blistering noticed. † Slight reverse bend noticed.

TABLE V.—TWO-INCH CAST-ZINC CUBE 1.

Diameters, ins., 1.990, 1.995, 1.997, 1.998, 1.995. Average............ 1.995
2.000, 1.995, 1.996, 2.002, 2.000. Average............. 1.999

Length, 1.9936 ins. Section area, sq. ins., 3.988

Time.	Interval, Mins.	Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.
11.07		0	25.4
11.09		0.00452	1,000
		0.00602	2,000
11.10		0.00750	3,000
		0.00853	4,000
		0.00954	5,000
		0.01030	6,000
		0.01055	7,000
		0.01156	8,000
		0.01255	9,000
		0.01357	10,000
11.12		0.01460	11,000
		0.01560	12,000
		0.01856	13,000
		0.02010	14,000
11.13		0.02260	15,000
		0.02510	16,000
		0.02760	17,000
11.14		0.03010	18,000
		0.03320	19,000
		0.03570	20,000
		0.03890	21,000
		0.04170	22,000
		0.04510	23,000
		0.04870	24,000
		0.05150	25,000
		0.05520	26,000
11.17		0.0592	27,000
		0.0630	28,000
		0.0662	29,000
		0.0704	30,000*
		0.0748	31,000
		0.0797	32,000
		0.0848	33,000
		0.0903	34,000†
		0.0956	35,000
		0.1024	36,000
11.22	0	0.1104	37,000
11.23	1	0.1195	37,000
11.24	2	0.1241	37,000
11.25	3	0.1290	37,000
11.26	4	0.1306	37,000
11.27	5	0.1335	37,000
11.28	6	0.1363	37,000
11.29	7	0.1390	37,000
11.30	8	0.1410	37,000
11.31	9	0.1433	37,000
11.32	10	0.1457	37,000
11.33	11	0.1475	37,000
11.34	12	0.1500	37,000
11.35	13	0.1512	37,000
		0.1868	41,500‡

Length, 1.62 ins.

* Blistering noticed. † Bending noticed. ‡ Maximum.

TABLE VI.—TWO-INCH CAST-ZINC CUBE 2.

(Imperfect casting, with blowholes.)

Diameters, ins., 2.001, 2.003, 1.999, 2.002, 2.002. Average............. 2.001
1.997, 1.993, 1.994, 1.999, 1.997. Average............. 1.996

Section area, sq. ins., 3.994

Lengths, ins., 2.001, 2.001, 1.997, 1.996, 1.997. Average............... 1.9984

Time.	Interval, Mins.	Compression per Unit of Original Length.	Unit Stress, lbs. per sq. in.
11.50	..	0	25
11.51	0	0.03750	20,000
11.53	2	0.03925	20,000
11.56	5	0.04000	20,000
11.59	8	0.04050	20,000
12.02	11	0.04150	20,000
12.05	14	0.04200	20,000
12.08	17	0.04250	20,000
12.11	20	0.04300	20,000
12.14	23	0.04350	20,000

The compressions, per unit of original length, and the unit stresses, are given in Tables III to VI.

Fig. 1 shows, for all the cases mentioned, the unit stresses corresponding to given compressions per unit length. Fig. 2 shows the compressions, per unit length, of three of the last-mentioned specimens under constant stress.

PRACTICAL TESTS OF SAND AND GRAVEL PROPOSED FOR USE IN CONCRETE.

By R. S. Greenman.

That almost all failures of concrete are caused by poor cement has been a popular opinion. After some failures the "post mortem" examinations have shown that poor workmanship—due to incompetent supervision—has sometimes been the responsible cause of the failures. Those experienced with concrete have, however, found that, in addition to these two factors, the other constituent elements in concrete may be the causes for poor results or failures. These are truths which have almost become axiomatic.

The relation of intelligent workmanship to good concrete is solely a matter for consideration between the workman and the owner; it is not a question of general interest. If the owner of the proposed structure is content with poor work, that is simply his own business. The question of proper materials is, however, of general interest, for many structures are often constructed from the same group of concrete materials. The selection of those elements which, with proper and intelligent workmanship, will produce good concrete is certainly important.

How to secure and select the best materials is often a problem. Apparently good sands or gravels have been selected, but not always have lasting results been secured. The method of securing for the consumer a good and safe cement has been provided. The question of securing good aggregates has been provided for in many individual cases, but a campaign of education as to the need of careful and proper selection of the aggregates is still generally needed. If it is true that the bulk of the cement now produced is used by the small consumer, then this applies especially to the smaller maker of concrete and not so much to those in charge of large public works. Even here, in many cases, surprising ignorance of the proper materials has been shown.

The selection of the materials for the concrete should be made only after careful examinations and tests, either in the laboratory or on the site of the work, or perhaps both. To aid this selection certain tests have been suggested and made. It will be found, however, that there are no standard tests or methods which can be used as a basis for comparable results. It is true that some of the laboratories of large public works and some departments of our state and national government make such tests—as for example the Structural Materials Testing Laboratory of the National Department of the Interior—but almost all the tests made are individual in character and have not the endorsement such as the American Society of Civil Engineers has given to the method of testing cement.

We have before us then the question of tests and methods. These may be resolved into two classes: laboratory tests and practical field tests. The laboratory tests should be such that practical results and conclusions can be secured; the field tests should be made supplementary to the laboratory tests and confirmatory to them, but not substitutes.

The selection of the coarse aggregates, crushed stone or gravel, is the more easily made, but sands and other fine aggregates do not so easily divide into good and poor materials. How such a division may be practically made is the topic we are to consider and this paper is presented with the sole idea of starting a discussion as to the practical tests which should be made on materials proposed for use in concrete. When reference is made to sands it will include, of course, all materials for the fine aggregate such as natural sands, screenings, iron-ore tailings, etc. Gravels can generally be considered as sands and facts concerning the one usually apply to the other.

SAMPLING.

It is, of course, generally conceded that any testing of the constituent materials is useless unless the samples submitted are thoroughly representative. It is certainly no easy proposition to take, for instance, a sample of sand directly from the sand bank or pit; but if the tests are to be of real value, provision must first be made for securing a representative sample. What should be considered the best way, and from what place is it best to take the sample? Shall it be taken from the pit or after delivery? Sampling at the pit means that either the pit must be uniform or that extraordinary judgment must be used in selecting a sample typical of the sand or gravel generally in that pit. Undoubtedly the best sample from any pit is that taken from a blend made of samples taken at various places in the pit; but in some cases, even with the most conscientious sampling, the pit sample may not be thoroughly representative of the sand as it is delivered. Therefore, whenever possible, the sample should be drawn from the sand hopper or taken from the sand pile after delivery. This sample, if accepted, should be kept as the standard, and future samples, whether taken at the pit or after delivery, should be expected to equal in quality the accepted standard.

LABORATORY TESTS.

Having secured the sample, what are the most practical laboratory tests? The test for strength of the mortar made with the sand and a "standard cement" is always a desirable test, but will not the tests for percentages of silt and of voids, and for size and grading of the sand grains, give results that form equally as good a basis for the acceptance or rejection of the sand?

In order to consider the test more fully without entering into a detailed consideration of the methods used in determining each result, let us adopt, for the present, as our standard the apparatus and the methods used by the Structural Materials Laboratory to which reference has already been made. It is very probable that most of the larger laboratories follow the same general methods and vary only in details. Now, having selected a definite plan of testing, do these results have a practical value in determining the relative values of the different sands? To discuss this phase of our topic we shall use the results secured in the laboratory of the Department of the State Engineer and Surveyor of the State of New York. Tables I to IV, inclusive, give the results of tests on thirty-four typical sands, selected practically at random from over two hundred tested during the past two years.

A study of these results will show that a sand can be judged by its percentage of voids or of loam and by the size and grading of its grains. As a general rule it would be impracticable to make long-time tests for tensile strength, so these tables give only the 7- and 28-day tests. Longer tests are, of course, desirable, but as we are now only considering practical tests we will endeavor

TABLE I.—TENSILE STRENGTH, LBS. PER SQ. IN.

No. of Sands Tested.	Class.	Natural.		Washed.		Per Cent.	
		7 Days.	28 Days.	7 Days.	28 Days.	Voids.	Loam.
10	A	217	305	201	301	32.0	4.1
13	B	167	271	155	249	33.4	3.3
11	C	121	208	123	212	36.6	2.2

TABLE II.—GRANULOMETRIC TEST.

No. of Sands Tested.	Class.	Per Cent. passing Sieves No. —.											
		2	4	6	10	20	30	40	60	74	100	140	200
10	A	100	99	98	89	69	47	29	14	8	3.8	2.3	1.6
13	B	100	99	98	96	87	72	56	31	17	5.8	2.6	1.6
11	C	100	100	99	98	95	79	60	38	29	7.5	3.6	1.6

TABLE III.—EFFECT OF VOIDS.

Per Cent. of Voids between	No. of Sands Tested.	Average Voids, per cent.	Average Tensile Strength,* lbs. per sq. in.		Per Cent. passing No. 100 Sieve.
			7 Days.	28 Days.	
25–30	5	27.8	192	310	2.8
30–35	17	33.0	164	256	6.4
35–40	11	37.8	140	220	6.7

* Results were taken of washed samples only, so that effect of loam would be eliminated.

TABLE IV.—EFFECT OF LOAM.

Per Cent. of Loam between	No. of Sands Tested.*	Average Loam, per cent.	Average Tensile Strength, lbs. per sq. in.			
			Natural.		Washed.	
			7 Days.	28 Days.	7 Days.	28 Days.
0–2.0	7	1.0	128	254	148	261
2.5–3.5	6	3.0	181	273	178	271
5.0–6.0	5	5.6	222	324	205	310

* Sands were selected which had approximately the same percentages of voids.

to draw some conclusions from the tests at hand. We find that in Table I the highest tensile strength corresponds to the lowest percentage of voids and the highest percentage of loam; and that the natural samples containing more than 3 per cent. of loam are stronger than the washed sand. From Table II we note that the smallest percentage of voids is secured by the sands having the best grading of the grains and that the poorest sands are those with the largest percentages of fine grains. Table III emphasizes the fact that sands are stronger when the percentage of voids is smaller and that the less the percentage of very fine grains the higher the strength. Table IV shows that loam to a certain amount is rather an aid to strength. This statement must not, however, be construed to mean that it would be advisable to permit large percentages of loam.

From this brief statement of the results secured and conclusions drawn, can it not be seen that each test is a very important aid in determining the quality of the sand under inspection? There are, of course, other laboratory tests which are of great importance in the selection of a sand, among which is notably the careful examination of the character of the sand grains.

Foreknowledge of what qualifications are necessary and of what effect certain conditions may have, is a vital point in interpreting the results; but with this foreknowledge we will then admit that results of tests like these do have a very practical value. With a knowledge of the best methods of conducting tests, and with a higher appreciation of the value of these tests among the general concrete contractors and frequently among engineers themselves, the laboratory tests can be utilized to their full value. Scoffers at laboratory tests have only to examine results of the tests with concrete in which the sand has been used, to see that the tests do forecast what takes place in the concrete.

FIELD TESTS.

While it may be most practicable and best to have the sands first tested in the laboratory, the greatest ultimate good to concrete in general would be to convince the contractor, and often the engineer, that simple field tests will also be of material benefit to the work. For example, frequent tests for density and for proportioning would have a most beneficial result. The contractor would not necessarily have to use more cement, but rather he might be able, by proportioning, to produce a more dense concrete simply by addition of either fine or coarse aggregates.

Such simple tests as those for percentage of loam by use of a graduated cylinder, or for the percentage of voids by means of the percolator, should be frequently made. Neither of these, for example, requires elaborate apparatus and either one can be made in any field office, and yet how seldom in general work is any effort made to learn how much loam a sand contains or its percentage of voids.

In discussing laboratory tests reference was made to tests of sand and cement for tensile strength. Cubes and cylinders made from the concrete as it is mixed make possibly one of the most satisfactory methods of securing good concrete. It is of course true that, to complete these tests, the specimen must be tested in a laboratory. The results obtained would, however, result in one soon learning whether or not the concrete is as good as can be secured. It may be claimed that this is not a practical field test. It may not be for small work, but on large work it should be more generally used.

In conclusion it should be again stated that the object of this brief paper is but to call the attention of "concrete" men to the practicability of testing the sands and gravels proposed for use in concrete, and to the tests and methods generally followed. Detailed descriptions and results can only be presented in more lengthy articles.

DISCUSSION.

The Chairman. VICE-PRESIDENT R. W. LESLEY (*in the Chair*).—Mr. Greenman's paper presents a subject of great interest to all who have to do with either cement or concrete. The fact that the appointment of a committee has been recommended is a proof of the recognition of the fact that sand as well as cement has to be judged; and in one of the suggested building codes in New York that same recognition is given. It seems to me that too much attention cannot be paid to the aggregates while so much attention is being paid to the cement.

We have Mr. Thompson with us, who is, I am sure, thoroughly familiar with this subject. I wonder whether we cannot start some discussion on this paper, which I consider a most important one.

Mr. Thompson. MR. S. E. THOMPSON.—At the present time practical men are just beginning to realize that sand ought to be tested and that it ought to be tested just as carefully as cement is tested.

The various points that have been brought out in the paper are very valuable as illustrating qualities of sand that affect the relative strength of mortars. A continuation of these tests using different proportions of cement to sand would be of further interest. The richness of the mortar will influence the effect of the fine material contained in the sand. For instance, Series No. 4 probably was made in proportions one part cement to three parts sand. The fine material then tended to fill the voids and to increase the density as well as the strength of the mortar. A corresponding series of tests in proportions 1 : 2 or 1 : 1 would show a different result. The increase in the proportion of fine material in these cases beyond certain points would tend to reduce the strength of the mortar, because, with the cement, the fine material would be in excess so as to more than fill the voids.

Another very important consideration is the character of the fine material. The author of the paper has termed this "loam." In most parts of the country the term "loam" implies the

Mr. Thompson. presence of a considerable proportion of vegetable matter. For this reason where the character of the fine material is undetermined, I prefer the term "silt;" where it is known to be strictly of mineral nature, the term "clay" may be used.

Where the fine material is made up of mineral particles, the mechanical analysis tests and void tests are important as distinguishing between different sands and as an aid to their selection. On the other hand, if the fine material contains organic or vegetable matter, the problem is entirely different. Within the past few years I have had occasion to test many sands that looked all right and had a satisfactory mechanical analysis, but which produced worthless mortar and in some cases caused actual failures of the structures. The tensile strength of 1 : 3 mortar with these apparently good, coarse sands ran as low as 20 to 40 lbs. per sq. in. at the age of 7 days. Further investigation showed the trouble to lie in a minute quantity of vegetable or organic matter which acted either chemically or mechanically in affecting the strength of the mortar, especially at early ages. Sometimes the result was produced by not more than 0.1 or 0.2 per cent. of this organic matter.

The field tests suggested by Mr. Greenman, especially the mechanical analysis and density tests of mortar, are valuable provided the sand contains no deleterious material.

Presence of organic matter may be determined by washing the sand and determining by ignition the amount of organic matter left in the wash water. For the present, however, from a practical standpoint, the surest test of a sand, and one that will indicate its quality both as regards fineness and any deleterious material, is the ordinary test of tensile strength. The sand in question is made into mortar briquettes in proportions 1 : 3 and at the same time, with the same cement, briquettes are also made with standard sand. The proportion of water for the work sand should be such as will produce the same consistency required for the standard sand mortar. The present requirements of the Joint Committee on Concrete and Reinforced Concrete are that mortar made with the sand in question shall test at least 70 per cent. of the strength of the standard sand mortar.

Another interesting point has come up in the course of investigations of concrete materials. I have found that with certain sands a change in the brand of cement will effect a decided improvement. A sand that with one brand of cement will give practically worthless results may test with another brand as high as standard sand mortar. **Mr. Thompson.**

MR. MONT SCHUYLER.—The harmful effects which lignite, an organic substance found in certain river sands, may have on mortars made from it, would not be shown by the tests just outlined. **Mr. Schuyler.**

Lignite is volume constant when saturated with water, but on drying out expands and exerts a considerable force in so doing. Though it is unlikely that mass concrete would be seriously affected, if at all, by the presence of lignite in the sand, finishing mortars are rendered very unsightly by the formation of craters, sometimes 2 or 3 ins. in diameter and ½ in. deep.

Were the proposed test modified to include the making of a parallel series of briquettes which should be allowed to dry for three or four days before breaking, this objection would be eliminated.

THE CHAIRMAN.—What Mr. Schuyler has just stated reminds me that many years ago in Philadelphia when there were a number of tanneries located on the upper Delaware, the sand that was gathered on the river bottom would contain small pieces of tanbark. These particles of tanbark would work all sorts of trouble in the cement. Of late years—whether it is because there are fewer tanneries, or because they use less tanbark, I do not know—we do not find that trouble any more. But it operated just as the lignite did which Mr. Schuyler spoke of. **The Chairman.**

MR. R. L. HUMPHREY.—I understood Mr. Thompson to state that a sand that gave good results with one cement would give worthless results with another. Can he give us more detailed information on that point? **Mr. Humphrey.**

MR. THOMPSON.—The problem is an extremely intricate one, and I have as yet reached no conclusion as to whether the variation in the influence of different cements is due to chemical characteristics or to the mechanical treatment of the cement at the factory. Similar results have been obtained with duplicate sets of tests, using six or eight different brands of cement. The results have also been corroborated by two or three other experimenters. **Mr. Thompson.**

Mr. Humphrey. MR. HUMPHREY.—It seems to me improbable that the cement would furnish the explanatory cause of the difference, or that the use of two different brands would produce any variation in the strength of mortar made with a given sand. While I have no doubt that the data obtained as Mr. Thompson stated would seem to indicate that any variation in the strength of the mortar was due to the brand of cement used, nevertheless it seems to me that such an explanation would be improbable.

Just a word as to the lignite to which Mr. Schuyler alluded. Lignite is a low form of coal and oxidizes in the air. This oxidation causes a swelling and forces off the mortar around the particles of the lignite. If, however, the lignite is protected by a dense skin of either cement or mortar such oxidation does not occur. I fancy that the disintegration that he refers to, or the splitting of the material, was caused by the fact that it was imbedded in mortar which was porous, and thereby the air had access to it.

I think the question under discussion is one of the most important that has ever come before the Society. It is a matter that should receive the earnest consideration of every constructing engineer. In former years looking at sand and rubbing it over the palms of the hands was employed to determine whether it was good or bad. Such a test should be relegated to the past, and we should apply more scientific methods of determining the value of sand used with cement, either in mortar or concrete. The proper results, whether in mortar or concrete, are what we are after. It is absurd to test cement with refinement, and use sand without any test whatever. We should apply careful tests to determine the value of every ingredient. Where laboratory tests are impracticable, there arises the necessity for a field test which can be readily applied and which will enable the engineer to make quick tests which will tell him whether the sand submitted is good or bad. It seems that a simple density test, perhaps something similar to what has been proposed by Mr. Fuller, together with an inexpensive compression test, will give two elements which will determine practically all that is required. There are cases of course where laboratory experiments were made, and the results were discouraging. In connection with the Charles River bridge in

Mr. Humphrey. Boston, a complete series of tests was made on crushed granite, and the results did not bring out what was expected. I believe that the Joint Committee on Reinforced Concrete, which is working on these standard methods, is doing a good work. I believe there is further work for us in the ascertainment of some simple field test which will be inexpensive, and at the same time practicable.

Mr. Chapman. MR. C. M. CHAPMAN.—In the laboratory of Westinghouse, Church, Kerr and Company, we have been making for some time about the same series of tests on sand as described in this paper and with rather similar results; but I want to call attention to the fact that tests showing the amount of voids, the granulometric analysis, amount of silt, etc., do not give a basis for predicting what the tensile strength is going to be; what is wanted is a test for sand that will enable one to tell in the case of a poor sand what the matter is.

As regards another point that has been brought out in the paper, namely, the question as to where the sand should be sampled, whether at the pit or at the work, our experience has been that most sandbanks vary so materially in the different portions of the bank that to get a sample from the pit that represents future deliveries is practically impossible; the most reliable samples are taken at the work after the sand is delivered and cannot be changed. The sandbank man generally saves his good sand. It is unsafe to rely on samples taken at the pit unless the purchaser has an inspector there to see that the same sand that was sampled is delivered.

The next point is, what does the author mean by loam? The word usually means "top soil" or soil containing much vegetable mold or decayed vegetable matter. It is extremely objectionable in concrete. I presume that in these tests loam is the fine material that remains in suspension in water longer than the coarser part. That in the case of a banksand might be almost anything. In the case of crushed trap or granite it can usually be only one thing, namely, the same material as the remainder of the crushed stone. With a banksand it may be clay or marl. There may be clay in quartz sand. It might be material very radically different from the coarser part. So I do not like the use of the word "loam" as meaning the fine

Mr. Chapman. particles that remain in suspension; because in the case of compressed granite that fine material is not objectionable—not as objectionable as it may be in the case of sand, which may contain a larger amount of vegetable matter, or some kinds of mineral matter. I prefer the term "silt" to loam in referring to the matter which remains in suspension in water.

As to the point brought up by Mr. Thompson, that a change in the brand of cement caused radical differences in results with the same sand, we recently had a failure of some concrete work where the sand appeared to be very good but the concrete had failed to set in making sills and lintels, which were cast on the ground and afterwards put in place. We were not able to move them in three or four days because they had not hardened sufficiently, and the blame was placed on the cement. In order to go on with the work a few wagon loads of cement of another brand were purchased. An examination proved that the cement first used was a good cement, and with standard sand gave excellent results; while the sand that had been used in the work that failed showed upon examination a high percentage of loam, and the loam adhered to the respective particles of the sand. Pouring some of this sand through water did not cloud the water very much, but upon stirring it in the water the attrition released the small particles of the loam from the surface of the sand, and showed a high percentage of loam; so that there must be something in the question of a change in the brand of cement affecting the results with the same sand. We simply changed the cement because we thought it was at fault, and got results with the second cement that were much better; that is, work could be handled on the following day.

The Chairman. THE CHAIRMAN.—Was there any change in the weather?

Mr. Chapman. MR. CHAPMAN.—No, the work had been going on for some time, and the weather was generally pleasant.

Mr. Kinney. MR. W. M. KINNEY.—Care should be exercised in the interpretation of tests of sand made in the laboratory, and it is usually advisable to combine them with field tests before approving any particular aggregate.

Very often with relatively fine sand, the thorough mixing of a small batch in the laboratory will produce higher strength results than a coarse and more uniformily graded sand, but because of the difficulty of mixing a fine material either with a normal number of revolutions of the mixer or turns by hand, the resulting concrete is inferior in strength to that made with coarser sand. **Mr. Kinney.**

The same applies, of course, to fine and coarse aggregate containing a high percentage of loam or clay. It is true that loam or clay up to a rather high percentage will improve the strength of laboratory-made concrete or mortar, but it must not be overlooked that the presence of such material hinders the thorough mixing of a batch of concrete in a normal length of time. Very often, too, the presence of foreign material of this kind will delay the hardening of the concrete and this is particularly disadvantageous in work done when the temperature is around the freezing point. Concrete deposited during the day at a temperature well above freezing may be frozen by a drop in temperature at night, because the hardening has been delayed by fine and loamy sand. The subsequent thawing out of this concrete on the following day may result in its destruction. Concrete made with clean, coarse aggregate would probably have sufficiently hardened to withstand the cold turn without protection.

Mr. Greenman. MR. R. S. GREENMAN.—What Mr. Thompson has said regarding the different cements and their influence on the sand has been a hobby and theory of mine for the last two or three years; but it is a difficult matter to prove. I still believe it, and I am glad that he has brought it out. That is one of the reasons why I think we ought to emphasize the necessity of field tests of the mortar or concrete as it is made up and goes into the work. Laboratory tests are good guides in the acceptance or rejection of cement or sand; but I think the final test is obtained in the field work. If we can test the mortar then we are getting down to the real basis of good concrete. I am hoping that somebody will be able to prove the truth or otherwise of Mr. Thompson's theory. I am working along the same line and hope to have some figures myself before long.

Mr. Cushman. MR. A. S. CUSHMAN.—I should like to ask if any one can explain how it is that dirty sand containing organic matter delays or restrains the set? I can understand why it should weaken mortar under test, but I cannot understand why it should delay

Mr. Cushman. the set; this is an important point, especially with reference to cinder concrete. Some difficulty has been encountered with cinder concrete owing to delayed set. In winter time particularly it has been found impossible to knock down the forms within the time that the economical handling of the work demands. If any one has an explanation to offer for delayed set I think it should be brought out.

Mr. Blair. MR. W. P. BLAIR.—I have been interested in this discussion, because I have been interested in the use of cement mortars for many years in connection with their use in brick street pavements in which cement enters into the grout between the bricks. The Paving Brick Association by the employment of expert investigators made an investigation of sand in order to find a reason for the failure of cement grout in many localities. Experiments were made on sands of Ohio, Illinois, Michigan, and Indiana, which showed that good results could be obtained in the laboratory, and we were able to trace 90 per cent. of the failure to the method of application.

It is unfortunate that engineers are subject to political conditions which make it necessary to employ untrained inspectors who do not use care in the application of the cement. The failures referred to could be traced to the faulty application and the faulty mixing of the material. The materials were used in wrong proportion, or the water was poured on without any previous dry mixing. In the latter case the inspector was asked why he had allowed this, and he said that he had really never before heard of mixing the materials dry.

Mr. Larned. MR. E. S. LARNED.—One phase of this question not touched upon in this discussion is the influence of varying amounts of water upon concrete. Laboratory tests are usually made of normal consistency, whereas in the work the concrete may contain from barely enough water to a large excess, and if field samples are taken direct from concrete mixed on the work, it must be expected that the results will differ considerably at the 7-day and 28-day periods. Some sands if made in mortar of normal consistency will show very satisfactory results, but if mixed with an excess amount of water show markedly inferior results.

It is well known that loam, clay, and in fact any very fine material even of a siliceous nature, retard the set of concrete.

Possibly this is due to the fact that with excess water a colloidal condition is brought about in the presence of which the cement is incapable of setting readily. **Mr. Larned.**

There is one determination of sand which I think would be a valuable one to make, namely, its coefficient of hardness. In the laboratory the sand does not get the same treatment that it receives in mechanical mixing. This was well brought out a few years ago in a paper submitted to the Society by Mr. Spackman and Mr. Lesley, wherein it was shown that the operator by changing his method of manipulation with a given sand could vary his results tremendously. He can by hard manipulation produce results which often occur in mechanical mixing where the sand is exposed to the attrition of the coarse hard aggregate. In this way a further percentage of fine material is produced which has an important influence on the tensile and compressive strength. I believe that field-made specimens, taken from concrete going into construction, will lead to more valuable and more conclusive determinations than can be had from laboratory tests, and will undoubtedly suggest changes in the methods and selection of materials that would not be found in laboratory tests alone.

As to the matter of grading sand, while we have a good material, free of deleterious ingredients, the reduction of voids by grading will unquestionably produce better results, but physical tests of the sand are, in my judgment, more important than an attempt to grade the sand without complete physical tests. Some sands that show very excellent composition will give very poor results compared to sands of poorer composition.

I think the consumer and the engineer in charge of the work could with profit direct their attention to the source from which the sand is procured. In my experience few sand pits are properly stripped of overburden; some, in fact, are not stripped in advance at all, the vender of sand undercutting the bank until the overburden falls in, and then depending upon removing this as being a cheaper operation than stripping in advance. This is sometimes not done with any degree of thoroughness, and again, where pits are stripped in advance, care is not exercised to get below the sub-soil. The amount of loam remaining might

Mr. Larned. have an important influence on the sand, this depending of course upon the depth of the sand pit.

Mr. Carpenter. MR. A. W. CARPENTER.—This discussion has brought out the fact that the only way to be sure that we are getting the concrete that will give the results we want is to test the materials, the sand and cement to be used, together, and to depend upon the results of that test rather than of individual tests of the sand or the cement. It seems to me that an analysis of the sand alone will not tell you whether your concrete is going to harden or not.

DESTRUCTION OF CEMENT MORTARS AND CONCRETE THROUGH EXPANSION AND CONTRACTION.

By A. H. White.

One of the most important of the standard tests for Portland cement is that for "Constancy of Volume"—a lulling title which carries with it the implication that our cements are actually constant in volume. It requires no very keen observer to find numerous instances where the implication has not been justified. Cracked copings and walls, sidewalks warped and split, and stucco dropping from houses, are sights which are so frequent as to force upon the observer the feeling that the phenomena must be due to some property characteristic of many if not all concretes which are exposed to the weather.

The change in volume of concrete due to temperature change has been determined with considerable accuracy to be for unit length 0.0000055,* or 0.00055 per cent., per degree Fahrenheit. There are, however, other changes due to the chemical processes of setting and hardening which are barely mentioned in even the more important treatises, and other variations due to the wetting and drying of the concrete whose very existence is practically unknown. It is with these two latter classes of changes that this paper will deal.

Review of Literature.

The literature of the subject is brief.

Quantitative measurements of the change of volume of cements while hardening commence with the classic experiments of Bauschinger,† who in 1879 reported on the change in volume of eight different cements made into small cubes of neat cement and 1:3 and 1:5 mortar. One set of cubes was kept in water and one in

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*Pence. *Engineering News*, Vol. 46, 1901, p. 380.

†Bauschinger. *Mitth. Mech. Tech. Lab. d. Tech. Hochschule in München*, 8th Heft, 1879.

TABLE I.—PER CENT. CHANGE IN VOLUME OF BARS OF NEAT CEMENT AND CEMENT MORTAR.

EXPANSION AFTER LYING CONTINUOUSLY IN WATER.

	Neat Cement.	1 : 3 Cement and Sand.	1 : 5 Cement and Sand.	Duration.
Bauschinger, average of 8	+0.046	+0.009	+0.001	16 weeks.
Schumann, average of 7	+0.150	+0.028	....	1 year.
		+0.029	....	2 years.
		+0.027	....	5 years.
Tomëi, average of 8	+0.025	+0.027	....	90 days.
Considere	+0.079	+0.028	....	63 days.
Gary, average of 10	+0.063	+0.008	....	1 month.
	+0.078	+0.013	....	3 months.
	+0.094	+0.012	....	1 year.
	+0.092	+0.020	....	2 years.
Campbell and White	+0.190	....	....	5 years.

CONTRACTION AFTER LYING CONTINUOUSLY IN AIR.

Bauschinger, average of 8	−0.214	−0.132	−0.124	16 weeks.
Schumann, average of 4	....	−0.042	....	1 week in water, then 3 weeks in air.
Tomëi, average of 8	−0.201	−0.094	....	90 days.
Considere	−0.132	−0.050	....	63 days.
Campbell and White	−0.390	....	....	5 years.

air and the changes in length of one side observed for 16 weeks. The most important study of the subject was made by Schumann,* who in 1881 and 1889 reported to the German Association of Portland Cement Manufacturers the result of expansion measurements made on blocks not only of neat cement and cement mortar but also on blocks of building stone. Gary† in 1899 reported from the Royal Mechanical Experiment Station at Gross Lichterfelde the results of the tests on ten Portland cements carried out jointly by the Experiment Station and a committee of the Association of German Portland Cement Manufacturers. The tests included measurements of expansion bars of neat cement and of 1 : 3 mortar placed in water. No tests were made of bars in air. The average results of the expansion tests at the experiment station are given in Table I. A few results by Tomëi‡ were quoted by Gary before the International Engineering Congress in 1893. Campbell and

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*Schumann. *Tonindustrie Zeit.*, Vol. 5, 1881, p. 184; and Vol. 13, 1889, p. 415.

†Gary. *Mitth. a. d. kgl. tech. Versuchsanstalt zu Berlin*, 1899. Ergänzungsheft 1.

‡Tomëi. *Trans. Am. Soc. Civil Eng.*, Vol. 30, 1893, p. 16.

TABLE II.—PER CENT. CHANGES IN VOLUME OF BRICK AND STONE WHEN ALTERNATELY WET AND DRIED, ACCORDING TO SCHUMANN.

Material.	Two Weeks in Water.	Two Weeks in Air.
Brick, light burned, red	+0.016	−0.015
" hard burned, white	+0.010	−0.009
" hard burned, white	+0.019	−0.010
" very hard burned, black	+0.006	−0.008
Sandstone, red, fine grained	+0.006	−0.018
" red, coarse	+0.016	−0.023
" green, fine grained	+0.050	−0.050
" red, fine grained	+0.050	−0.050
" red, very fine grained	+0.206	−0.178
Limestone, white	+0.004	−0.008
" Lothringer Lias	+0.007	−0.008
" Challe	+0.011	−0.009
" dense, containing clay	+0.026	−0.026
Granite	+0.006	−0.015
Basalt, *a*	+0.041	−0.050
" *b*	+0.026	−0.027
" *c*	+0.048	−0.050
" *d*	+0.023	−0.057

White* in 1906 presented a study of 56 expansion bars of neat cement which had been under observation for five years. A number of these were pathological specimens of unsound cement containing free lime and magnesia, but their experiments showed as much as 0.19 per cent. expansion with sound neat cement kept in water for five years and a shrinkage as high as 0.39 per cent. with bars of sound neat cement kept in air. A reliable and inexpensive micrometer and a satisfactory method of inserting the glass plates in the expansion bars were described in that paper. These improvements obviate the difficulties found by some of the earlier German investigators in getting concordant results. Table I presents the condensed data of the investigators mentioned.

The tests in Table I were all made on small bars without metal reinforcement. So far as the author has been able to discover, only two investigators have ever attempted to study the most important subject of volume changes in reinforced concrete. Considere† in 1899 reported the behavior of small bars of neat cement and of mortar made with 600 kilograms of cement to each cubic meter of sand. The prisms were 60 by 25 by 600 mm. Half of them were reinforced with a round steel bar 10.2 mm. in diameter. The measurements extended over a period of 63

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*Campbell and White. *Jour. Am. Chem. Soc.*, Vol. 28, 1906, p. 1273.

†Considere. *Comptes rendus*, 1899, pp. 129, 467.

days and at the close of the period showed the following changes reported as millimeters per 100 mm.

	In Water.	In Air.
Neat cement, not reinforced........	+0.079	−0.132
" " reinforced...........	+0.022	−0.025
Mortar, not reinforced.............	+0.028	−0.050
" reinforced.................	+0.006	−0.010

Emerson* in 1904 studied the change in length of the reinforcement. Bars of neat cement and of concrete of various proportions were made 8 by 8 ins. by 3 ft., each bar carrying a steel rod ½ in. square and 3 ft. 6 ins. long. The progressive changes in the length of the rods were measured for 3 months and the resulting stresses in the steel calculated from Hooke's law. The final measurements on bars kept constantly in water showed a tensile stress on the steel amounting to about 8,000 lbs. per sq. in. when bedded in neat cement and decreasing to a little over 1,000 lbs. in 1:3:6 concrete. On bars kept constantly in air the steel was shown to be under a compressive stress which was usually greater than the tensile stress developed in bars kept under water. When bars taken from water were placed in air the stresses in the steel changed from tension to compression as the bars dried. No direct measurements were made on the volume changes in the concrete.

Only one experiment seems to have been made on a really large scale. Binnie† constructed a block of 4-to-1 granite concrete 100 ft. in length and 1 ft. square, built on rollers so as to be free to expand. He dismisses the subject of effect of moisture with the unsatisfactory statement that in wet weather the expansion due to moisture was often as much as that due to summer heat.

The experiments of the investigators cited above agree in showing that neat cements hardening under water expand at a decreasing rate for several years, that neat cements hardening in the air contract in a similar manner and that cement-sand mortars change in the same direction as the neat cements, but to a less extent.

The effect of alternately wetting and drying concrete is given qualitatively by Binnie and may also be deduced qualitatively from

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*Emerson. *Engineering News*, Vol. 51, 1904, p. 222.

†Binnie. *Proc. Inst. Civil Eng.* Vol 160, 1905, p. 21.

the experiments of Emerson. Quantitative measurements are entirely lacking. It is worthy of comment that although several investigators have reported on the behavior of bars in water for periods of several years, none have reported on the behavior of bars kept in air for a period of more than four months, with the single exception of Campbell and White who studied one commercial cement during four years.

This curious laxness in the study of the properties of Portland cement, which next to steel is the world's most important building material, arises probably from two causes. In the early days of the Portland cement industry the material was used almost entirely for structures such as foundations where it was kept constantly damp. It justified itself under these conditions and the assurance with which it was used in damp places transferred itself unthinkingly to conditions where cement is less stable. The other reason for the oversight is probably to be found in the fact that few engineers who use cement have had the chemical training which would enable them to study the complex phenomena involved in these changes.

The following pages show especially the effect of alternately wetting and drying bars of neat cement and sand mortar. There are also included the behavior of various bars of neat cement kept continuously in water and in air. These are included not only because more data on the subject are desirable, but because the Portland cement now being used is a different material from that tested by the European investigators, who undoubtedly worked with cements made in the old vertical kilns. The modern American cement made in rotary kilns differ from the older not only in method of manufacture but also in chemical composition and is worthy of separate study.

EXPERIMENTAL METHOD.

The method of making and measuring the expansion bars was that of Campbell and White.* The dimensions of the bars are approximately 1 by 1 by 4 ins. and they contain bevelled glass plates cast into the end to ensure a smooth surface for the micrometric measurements. The Portland cements used were all commercial samples which passed successfully the standard tests

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**Jour. Am. Chem. Soc.*, Vol. 28, 1906, p. 1273.

for soundness. Standard methods were used in proportioning the water and in mixing. The initial measurement was made after the bar had stood 24 hours in the damp box.

Campbell's perfected micrometer is described and illustrated in the paper referred to. It is possible to make three successive readings with it with a maximum error of about 0.005 mm. Since the bars, after the thickness of the glass plates imbedded in the end is deducted, are almost exactly 100 mm. long, each 0.001 mm. equals 0.001 per cent. In making measurements three successive readings are taken and the mean recorded. The bars and micro-

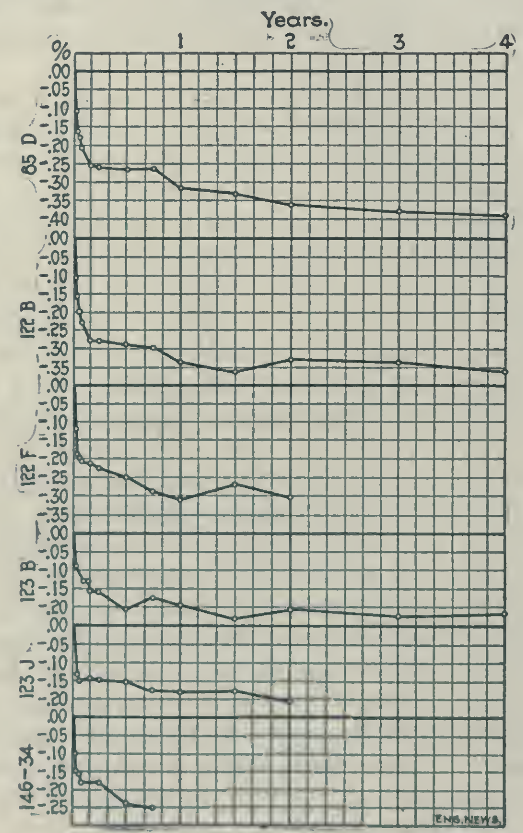

FIG. 1.—Linear Contraction of Bars of Neat Cement kept in Air for Four Years, expressed in per cent.

meter are kept continuously in a room surrounded on all sides by other rooms in a building whose temperature in winter is controlled by automatic thermostats. During cold weather the temperature of the room will not vary more than two or three degrees from 70° F. In summer the temperature may rise to 80° F. Since the difference in the coefficients of expansion of the cast-iron yoke of the micrometer and of concrete is only 0.0000005, temperature variation in the room introduces merely a negligible error.

CONTRACTION OF NEAT CEMENT IN AIR.

The curves of Fig. 1* show the linear shrinkage of bars of neat cement kept constantly in air. Four different brands of commercial cement all passing standard specifications for constancy of volume are represented. The volume of these bars varies with the humidity of the air but on the whole the shrinkage of all the bars is reasonably uniform and averages:

7 days (average of 6)	−0.109	per	cent.
28 days (average of 6)	−0.190	"	"
6 months (average of 6)	−0.236	"	"
1 year (average of 5)	−0.270	"	"
2 years (average of 5)	−0.289	"	"
4 years (average of 3)	−0.322	"	"

It is evident that one-third of the shrinkage comes in the first week, more than half of it in the first month, and almost all of it in the first year. A shrinkage of 0.322 per cent. amounts to nearly 4 ins. in 100 ft. and indicates clearly why it is not possible to use neat cement for interior floors and wall finishes. The change in volume of cement and sand mortars will be treated later.

It is often held that the use of fresh cement is responsible for cracks in finished work. It is therefore of particular interest to note that freshness cannot be held responsible for the changes shown here. Bar 85 D was made from cement which had been stored in our laboratory for six months before use. Bar 122 B was made from an inspector's sample of commercial cement when it was received at the laboratory and 122 F was made after it had stood in an open bottle in the laboratory for 2 years and 5 months. Bar 123 B was made from a commercial cement the day after it

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*Acknowledgment is made to the *Engineering News* for the cuts used in this paper.–ED.

had been ground at the mill and 123 J after it had been aged 2 years and 6 months. In neither case can any effect of ageing be noticed. The shrinkage at the end of 2 years is practically identical in the aged and fresh cement.

CHANGES IN VOLUME OF BARS OF NEAT CEMENT IN WATER AND ALTERNATELY WET AND DRY.

The first four curves of Fig. 2 show the changes in four bars of neat Portland cement when kept under water at approximately constant temperature for almost 3 years, allowed to dry in air for 2 months at the same temperature, and then put back in water again. The mills represented are: 131 C, one of the largest Lehigh Valley mills; 131 E, a small Michigan mill of good reputation using the wet process with marl and clay; 131 G, one of the large mills using slag and limestone; and 131 I, another Michigan mill using the wet process. The fifth curve of Fig. 2 (131 A) shows the changes in a bar of natural rock cement from one of the leading mills of the country. The last curves (146 A3 and A4) show the variations in two bars of cement from still another Michigan mill.

The behavior of the first four Portland cements kept constantly in water for 3 years is quite in accord with the results obtained by European investigators. The linear expansion at the end of the first year amounts to from 0.07 to 0.15 per cent. and the expansion after the first year is very slight. The high expansion of the natural rock cement, 0.49 per cent. after 3 years in water, is worthy of note as being quadruple that of the average of the Portland cements.

Attention is especially directed to the contraction in these bars after they were removed from the water and allowed to dry in the air at room temperature. The change in volume was slow and had not entirely ceased after 65 days when the test was terminated. The values were, however, remarkably concordant, the largest contraction of the four Portland cements being 0.15 and the smallest 0.13 per cent. The bars not only shrank to their initial volume but three of the four contracted further. When the bars thus air-dried were again placed in water the expansion was rapid. In one day they recovered 90 per cent. of the length which they had lost in 65-days' air-drying and in the course of the next month they slowly expanded further until three of the four were longer than they had ever been. The exception, Bar 131 C, changes very slowly and it appears from the curve that it had not reached equilibrium either on drying or wetting.

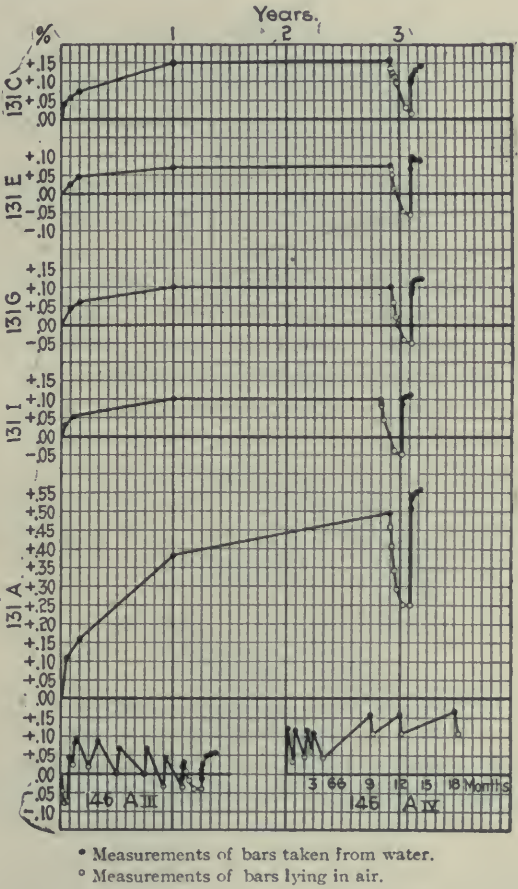


FIG. 2.—Linear Expansion and Contraction of Bars of Neat Cement when Alternately Wet and Dried, expressed in per cent.

From the standpoint of structural stability it is rather disquieting to learn that these cements which had remained in water without any measureable change in volume from their first to their third year shrank 1.7 ins. in 100 ft. when they were dried in air and in 24 hours when again brought in contrast with water expanded 1.5 ins. and within a month had expanded 1.8 ins., calculated on an 5nitial 100-ft. measurement. The question of the ultimate limit to tihese changes is discussed later.

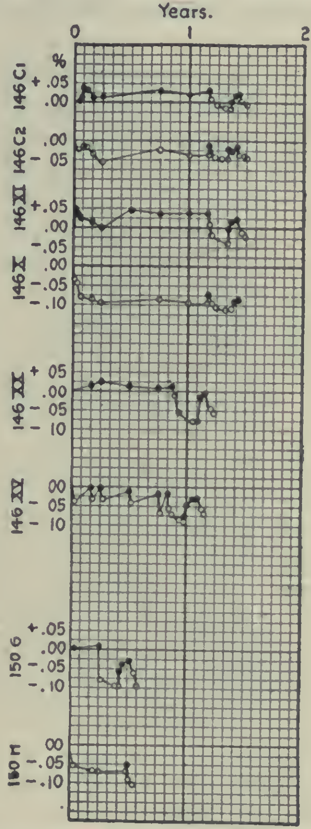
The natural-rock cement 131 A changed similarly but in more marked degree. Its linear contraction on drying for 60 days amounted to 0.24 and its expansion after subsequent immersion for 24 hours in water to 0.25 per cent. The total expansion after 30 days in water was 0.30 per cent.

A more complete illustration of the magnitude and regularity of the changes in volume as the cement passes from the wet to the dry state is shown by the last curves of Fig. 2, Bars 146 A III and A IV. These bars have during their 18 months' existence been systematically alternated in air and water. The points make a fairly regular sawtooth curve which would probably have been more regular had a longer time been allowed for equilibrium in the early portion of the test. The last change of 146 A III, which was the most carefully watched, shows a variation from −0.038 to +0.058, a total change of 0.096 per cent., as it passed from the dry to the wet state for the seventh time. The behavior of these companion bars, 146 A III and A IV is treated more fully in the discussion of the behavior of sidewalks.

CHANGE IN VOLUME OF 1:3 SAND MORTARS, ALTERNATELY WET AND DRY.

Fig. 3 shows the changes in length of eight bars of Portland-cement mortar mixed in the proportion of 1 part by weight of cement to 3 of standard Ottawa sand. The curves are quite like those of the neat cement but the changes are on a smaller scale. The bars kept continuously in water expand within the first few weeks as much as 0.05 per cent., but decrease after that and usually expand again later to a figure that is no greater than 0.05. Bars kept continuously in air show greater volume changes than those kept in water, the contraction after 3 months averaging about 0.08 per cent. and running as high as 0.10 per cent. When the bars change from the dry to the wet state and vice versa the change in length is about 0.06 per cent.

One of the best known commercial waterproofing compounds, used in accordance with the manufacturer's directions, was incorporated with the four middle bars of Fig. 3 (146 X, XI, XV, XX) in the hope that the value of such compounds might be demonstrated. There is no evidence of favorable effect, the change between the wet and dry states being fully as large and apparently as prompt as in the bars not waterproofed.



• Measurements of bars taken from water.
° Measurements of bars lying in air.

FIG. 3.—Linear Expansion and Contraction of Bars of 1:3 Cement Mortar when Alternately Wet and Dried, expressed in per cent.

VARIATIONS IN VOLUME OF COMPOUND BARS.

In order to get a direct comparison between bars of neat cement and those made with sand under identical conditions, compound bars were made by casting a bar of neat cement integrally on a freshly formed bar of the same dimensions made of 1 part cement to 3 sand. Four of these bars were made from the same lot of cement at approximately the same time. Their variations in volume are shown graphically in Fig. 4, the initial measurement of each being plotted as zero.

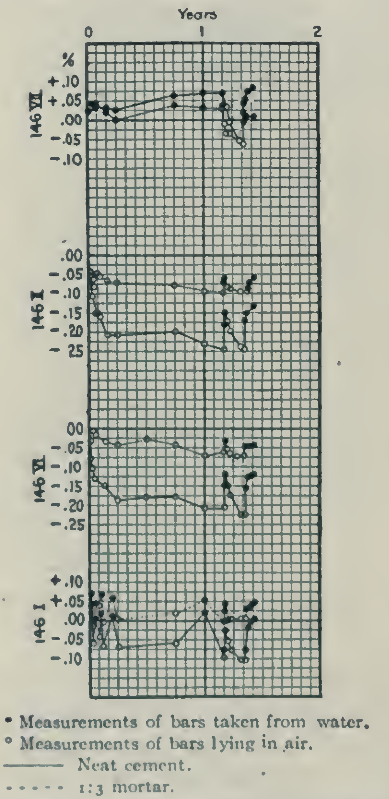


FIG. 4.—Linear Expansion and Contraction of Compound Bars of Neat Cement and 1:3 Sand Mortar when Alternately Wet and Dried, expressed in per cent.

Compound bar 146 VII was kept continuously in water for 14 months. Both portions expanded but at unequal rates, so that at the end of this period the neat bar was 0.038 per cent. longer than the one containing sand. On drying in air both shrank, but the mortar bar contracted the more rapidly so that at the end of a week the neat bar was 0.072 per cent. longer than the other. As the mortar bar reached equilibrium the neat bar caught up with it and even passed it, so that at the end of 2 months it was the shorter by 0.012 per cent. On placing in water both expanded rapidly, the mortar 0.050 per cent. in 24 hours and the neat 0.086 per cent. The neat bar kept increasing until, as was the case with the four neat cements previously noted, it overtopped any previous record and at the end of a month was 0.075 per cent. longer than its companion, whereas after 14 months' previous immersion in water it had only been 0.038 per cent. longer.

Compound bars 146 II and 146 VI were made to test the effect of fineness of sand. The mortar bar of 146 II was made with Standard Ottawa sand while 146 VI contained river sand from which everything coarser than 40 mesh had been sifted. Both bars were kept in air for over a year and then immersed in water. No marked effect of the difference in sand can be noted. The contraction of the bar with the fine sand is rather less than that with the standard sand but the difference is not definite enough to be certain. The neat cement portions of the bars behaved alike and similarly to the others noted, contracting over 0.20 per cent. in one year. At the end of the year the neat cement bar 146 II was 0.147 per cent. shorter than its companion mortar bar and the neat bar 146 VI, 0.150 per cent. shorter than its companion. On placing in water there was the usual sharp expansion followed by a contraction as the bars were again dried in air and another expansion as they were placed in water, and so on.

Compound bar 146 I was changed every week during its first few weeks and shows the usual sharp alternations with no sign of decrease in intensity with age.

ILLUSTRATIONS FROM ACTUAL PRACTICE.

It may be urged that the foregoing tests were all made on small bars manufactured in the laboratory and that such large values might not be obtained in actual practice. To meet this objection and also to make the case stronger the results shown in Fig. 5 are introduced. The scale in this plate is purposely changed in order that the steady nature of the changes in volume may be shown by daily measurements. The ordinates remain the same but the abscissas are magnified thirty times, each horizontal unit representing a day.

Bar 156 A is made from a part of the cement sidewalk on the north side of the campus of the University of Michigan. It was laid in 1890 from imported German cement by the University workmen who were paid by the day and under no temptation to slight the work or skimp cement. Most of the slabs of this walk are now warped slightly so that shallow puddles of water stand in them after a rain, and even after twenty years the expansion is still occasionally causing adjacent blocks to heave and form an inverted "V." The top layer is split from the bottom in many places and it was from one of these pieces of the top coat that bar 156 A was sawed. An examination of its lower surface shows that the split was not due to faulty bonding, for the break has occurred not at the exact junction of the top coat and base, but just below the upper surface of the base. The top coat carries with it a thin layer of the base. This bar became dampened in the sawing process and was therefore put into water and allowed to become saturated and fully expanded before the measurement recorded as the initial was made. It will be noted that it then contracted slowly for 14 days in the air, the total shrinkage being 0.05 per cent., and that on putting it into water it expanded to practically its initial volume in 4 days. When removed from water it contracted again as before.

Another instance to which attention is called is bar 156 E, cut from a piece of cement stucco which came loose from a brick porch after only two years' service. The stucco is well mixed, has a good ring and is hard. The brand of cement used and the proportions are unknown, but it is certainly a rich mixture, probably as high as 1:1. The stucco in coming off did not split off from the brick but in almost all instances carried a thin skin of the brick adhering to it, showing that the fault did not lie in failure to bond the materials together. The behavior of a bar sawed from this stucco is shown in the curves for 156 E. It was sawed to shape without moistening, and when placed in water for 24 hours it expanded 0.07 per cent. and reached 0.08 per cent. after 4 days in water. On drying in air it slowly returned to its original volume. It is not to be wondered at that a stucco which expands practically 1 in. in 100 ft. whenever it gets thoroughly wet, should fail.

Another instance of change of volume in material which has been in use many years is afforded by the study of a section of cement sidewalk which was taken up in good condition after 20 years' service. From this was sawed a bar approximately 1 by 2 by 4 ins., consisting of the top coat 1 by $\frac{3}{4}$ by 4 ins. on a section of the bottom portion 1 by $1\frac{1}{4}$ by 4 ins. This bar (156 B) is therefore similar in many ways to the compound bars of neat cement and 1:3 mixture before referred to. It showed the usual expansion of both layers in water and the contraction in air which was expected. When placed in water the bottom layer expanded practically its whole amount (0.033 per cent.) in 15 minutes, while the richer top layer expanded in the same time only 0.010 per cent. At the end of 24 hours the top portion had expanded 0.028 per cent., and after 3 days had become practically constant with almost the identical expansion 0.035 per cent. which the bottom showed. This is interesting partly because of the evidence of alternate bending stress in the concrete due to the more rapid expansion of the lower layer and partly because of the ultimate agreement in expansion of the top and bottom portion. Note that this cement sidewalk was in good condition after 20 years' service. It is possible that its satisfactory condition is due to this agreement in coefficient of expansion of the two layers.

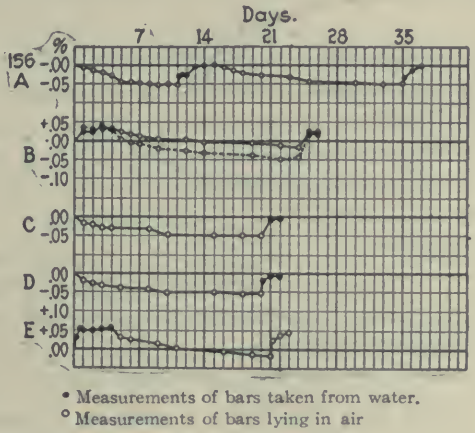


FIG. 5.—Linear Expansion and Contraction of Bars Cut from Stuccos and Sidewalks when Alternately Wet and Dried, expressed in per cent.

Another bar was sawed from the same piece of this sidewalk immediately adjacent to the first one, but instead of being left as a compound bar it was again sawed at the line of demarcation between the top and bottom coats, so that the top portion (Bar 156 C) was separated from the bottom (Bar 156 D). These two bars were then alternately wet and dried, as the others had been, to determine whether in the compound bar the change in volume of one portion had been influenced by the other. This has apparently not been the case in this bar, for, as shown by the curve, the two portions C and D expand and contract to about the same extent and at the same rate as in the compound bar B.

SUMMARY OF EXPERIMENTAL DATA.

In the foregoing pages there have been presented data on the sadly neglected subject of expansion and contraction of hydraulic cements as they age in water and in air and as they are alternately wet and dry. It should be emphasized, however, that the number of experiments is entirely inadequate to make it proper to quote them as average results. They are to be regarded merely as illustrations of how some acceptable commercial cements have behaved. It is, however, permissible to summarize the extremes to show the range of fluctuation.

Neat cement bars hardening under water.—After 1 year, expansion 0.07 to 0.15 per cent.; 1 year to 4 years almost no change; on drying after 3 years in water, contraction 0.13 to 0.15 per cent.; on wetting again, expansion 0.13 to 0.17 per cent.

Neat cement bars hardening in air.—Contraction after 3 months, 0.14 to 0.28 per cent., after 1 year, 0.18 to 0.34 per cent., with slight increase to 4 years.

Cement-sand bars, 1:3.—Hardening under water, expansion 0.01 to 0.05 per cent., the greatest changes being in the first few weeks; hardening in air, contraction 0.06 to 0.09 per cent., most of the contraction being in the first three months; on wetting and drying, expansion 0.04 to 0.09 per cent. on wetting, and contraction within same limits on drying.

Compound bars made of one layer of neat cement on a layer of 1:3 sand expand and contract together but at different rates and to different degrees. The differential expansion varies from 0.00 to 0.15 per cent.

The one natural rock cement tested showed twice as much variation in volume as Portland cement.

These variations in volume with change of moisture content do not disappear with lapse of years, for changes of 0.05 and 0.06 per cent. have been observed in bars cut from sidewalks which have been laid 20 years.

CHANGES IN MONOLITHIC CEMENT AND CONCRETE.

These changes noted above may seem small, but when converted into other figures their real seriousness becomes apparent. The following hypothetical illustrations are presented to show the magnitude of the possible stresses.

Imagine a wall of neat Portland cement 100 ft. long anchored at each end to absolutely immovable supports. Let it be kept under water for 3 years. At the end of that time it would, had it been free, have expanded 0.1 per cent. or 1.2 ins. Since it is supposedly not free to expand the compression will be 0.1 per cent. and the compressive stress, on an assumed modulus of elasticity of 5,000,000, will be 5,000 lbs. per sq. in. Perhaps the cement could withstand this pressure.

Imagine now that this cement wall becomes dry. It should contract 1.7 ins., but since it is not free there will be developed a tensile stress of 7,500 lbs. per sq. in., which is several times more than it can stand. The wall would have to crack.

Suppose this neat cement wall had been originally allowed to harden in the open air. After 2 years it would, if free, have contracted 0.25 per cent., but since it is anchored at the ends there will be (assuming the same modulus) a tensile stress of 12,000 lbs. per sq. in. Is it not evident why neat cement is not used in practice?

If the above wall had been built of concrete with the same volume changes as the 1 : 3 sand bars and as the old sidewalks whose measurements are given above, its change in length between the wet and dry states would probably have amounted to 0.05 per cent., which with a modulus of elasticity of 2,500,000 lbs. per sq. in. corresponds to a stress of 1,250 lbs. per sq. in. The concrete could withstand this in compression but not in tension. Not even our best steel will stand indefinitely repeated alternate bending stresses nearly to its elastic limit. Why should we wonder then if a rich cement mixture should ultimately crack when exposed to the stress resulting from a volume change of 0.05 per cent. every time it is exposed to rain or sunshine?

There is no experimental evidence which shows the changes in volume of leaner concrete. It is evident that the properties of the sand and rock become of increasing importance as their percentage increases. The tests by Schumann on volume changes in building stones, quoted at the commencement of this paper, show small values for limestones but variable values for sandstones, the upper figures being almost as high as those quoted for neat cement. Whatever the values might be for crushed stone as used in concrete, it is hardly conceivable that gravels and sands which have withstood the action of the weather for centuries without disintegration can change their volume much when wet and dried. It seems probable that in gravel concrete the volume changes decrease as the proportion of cement decreases, until a certain lower limit is reached where the change in volume is purely a capillary phenomenon.

If this line of reasoning is correct those concrete structures should be most stable when exposed to the weather in which there is just sufficient cement to give strength but not enough to cause excessive volume changes. Practical experience apparently confirms this view.

Stucco.

There are many instances where stucco has remained perfect even when exposed to the weather for half a century. There are probably more instances, especially in recent years, where stucco has failed miserably in 5 years. The excellence of old stucco is sometimes laid to the natural rock cement with which most of it was made. It is not safe to deny this on the evidence of a single experiment, yet attention may be drawn again to the one natural rock cement which has been tested in our laboratory, whose volume changes were in every way much greater than those of Portland cement.

Is it not more probable that the old stucco has survived because of its lower proportion of cement? When it was simply a question of providing a rough brick or stone wall, entirely durable in itself, with a smooth surface, and when in addition hydraulic cement was expensive, it was natural to use a mortar containing just enough cement to adhere to the wall. With the introduction of metal lath as the support for stucco, and the necessity of protecting the metal from corrosion, coincident with the drop in price of Portland cement came the use of richer stuccos.

The step was apparently an unfortunate one, for a volume change of 1 in. in 100 ft. as shown by the stucco 156 E as it changes from the wet to the dry state cannot but cause cracks. They may be only hair cracks, for with a crack every 12 ins. each crack need only be 0.01 in. wide to take up the whole change in volume. The elastic metal lath will probably yield without mechanical injury, but the protection against corrosion which the stucco is supposed to afford disappears with the first hair crack, and unless the lath has been better protected, by galvanizing or painting, than most sorts are by the manufacturers, its days are numbered. It would have been better to have used a leaner stucco and kept it free from cracks.

A stucco rich in cement may also split off in time from brick where a lean stucco would have remained good. The change in volume of brick when wet and dry is, according to Schumann, less than 0.02 per cent. If the change in the stucco is 0.08 per cent. there is opportunity for a stress corresponding to a volume change of 0.06 per cent. which might be 1,500 lbs. per sq. in. The fact that the stucco has not split off in 1 or 2 years does not mean that it may not split off in 5 or 10 years.

INTERIOR FLOORS.

The question of reinforced concrete is discussed in a later paragraph. Here only the changes in the concrete itself will be considered. The usual practice will be assumed in which there is a base of rather lean concrete covered with a wearing coat of mortar sometimes as rich as 1 : 1.

The first case assumed will be that of a cellar floor where the base is tamped down and the top coat at once put on, giving the best conditions for bonding. The whole mass gradually dries out and shrinks, but the top coat, because of its high content of cement, shrinks more, so that the conditions will somewhat resemble those represented by Curves 142 II and VI of Fig. 4. The top coat will tend to separate from the base, but since a cellar is always relatively damp there will probably be little trouble.

The situation is not so favorable when a top coat is to be laid on an upper floor of a concrete building. The main portion of the floor has been poured for possibly 3 months and has already completed most of its shrinkage. Its surface is also probably dirty so that the freshly poured mortar will not readily unite with it. Both of these circumstances are unfavorable. The fact that the main portion of the floor has already dried out and shrunk causes an even greater differential shrinkage of the top coat. The advice is usually given to wet the floor thoroughly before pouring the top coat. It would be better to keep it wet for 24 hours beforehand in order to give it an opportunity to expand to somewhere near its initial volume.

It is not surprising if under these circumstances the top splits off the base, shrinkage cracks appear, and the individual slabs curl up somewhat at the edges in the effort to relieve the shrinkage strains. In case electric conduits, etc., are bedded in the top coat their course will be indicated by shrinkage cracks following the lines of weakness which they cause. Since these floors remain continually dry, conditions will become constant after a few months, and there will be little further change.

SIDEWALKS AND PAVEMENTS.

The chief factor which influences the behavior of sidewalks and pavements is the weather. The influence of temperature is well known and will not be dwelt upon. A sidewalk usually consists of a base of rather lean concrete covered with a wearing coat of cement mortar which may be as rich as 1:1, and is not usually poorer than 1:2. The walk is cut into blocks and is sometimes provided with expansion joints.

As the walk dries out the top coat shrinks more than the bottom, as shown in the experimental study of compound bars. The effect can often be seen in sidewalks where the contraction has dished the entire slab so that shallow puddles of water stand in them after a rain. When the walk is wet by the rain the top expands more than the base. The alternate bending stresses thus developed all too often show themselves after a few years when the top coat splits off the base.

The necessity of expansion joints is now well recognized, although they are put in principally to take care of expansion due to changes of temperature. The experiments presented in this paper on both neat and sand briquettes indicate that the expansion of cements kept wet ceases at the end of a year, and that the total expansion does not amount to over 0.1 per cent. This would require a ¼-in. expansion joint every 20 ft. In addition there must be the joints to care for the expansion due to summer heat, which for a rise of 100° F. is 0.05 per cent., about half of what may be expected from moisture. To take care of the volume changes from both these causes a ¼-in. expansion joint every 10 ft. should be adequate, although it does not include much margin of safety. In hot weather after a long rain these joints would be almost closed. In dry or colder weather they would be open.

According to the experimental figures any evils due to expansion should make themselves evident during the first two years, but practice does not always bear out this assumption. Reference was made at the commencement of this article to the cement walk laid around the campus of the University of Michigan twenty years ago, which is still showing expansion and giving mute evidence of the pressure generated by occasionally thrusting up two adjacent slabs in an inverted "V". This is not a phenomenon due to summer heat for it occurs usually in the spring. What is the cause which is still making this walk expand after twenty years?

An explanation may be suggested, although it is not possible to prove it completely. It is well known that the glass of thermometers expands when heated and contracts when cooled, but not to its original length except after a long lapse of time. Thermometers which are used frequently at high temperatures keep growing longer. If they are laid away for a long period of time they shorten again and may even become shorter than when first calibrated. Metals behave similarly, so that it is not possible to utilize their expansion for making pyrometers which will not need frequent recalibration. The warping of the grate bars of fireboxes due to expansion is another illustration. It would be natural to expect by analogy that concrete would tend to a permanent expansion when it is kept wet longer and oftener than it is dry, and that it would tend to become shorter under the opposite conditions. The experiments of this paper in general confirm this theory. Of the five bars of neat cement of Fig. 2 which had been kept under water for three years and were then dried out and re-immersed, all but one were longer after the re-immersion than they had been, and the one exception is apparently susceptible of explanation.

A more striking illustration is afforded by the two neat bars 146 A III and A IV which are shown at the bottom of Fig. 2. These bars were made on the same day from identical materials, but after the initial measurement A III was left in the air and A IV placed in water. They were then alternated between air and water, at first at the end of each week and afterwards at longer intervals. There is however a definite difference in the treatment of the two, for with A III each cycle has consisted of a period in air of two or three months followed by an immersion in water of usually a week. It has therefore been dry most of its life and its mean length since the end of the first month has been steadily diminishing, as the curve clearly shows. The companion bar A IV was subjected to the opposite treatment in that its long periods were in water and its short periods in air. Its mean length and also its length when wet has been steadily increasing for the 18 months of its existence, although the rate of increase since the ninth month has been slow. At its last measurement it was slightly longer than another bar of the same cement which had been kept constantly in water, but it will require a longer time to decide whether the increase is material.

This theory of the continued expansion of cement walks in a rather damp climate, such as that of Michigan, is the only one which seems to adequately explain the compression ridges still occasionally developing in the twenty-year-old walk on the campus of the University of Michigan which has been referred to before. If the theory is correct it would indicate that there would be more trouble with cement sidewalks and pavements in wet countries than in those which are reasonably dry, and that there is need of much wider expansion joints in damp climates.

REINFORCED CONCRETE.

It is evident from the data presented here that concrete undergoes a notable shrinkage as it drys. If a concrete beam is formed rigidly fixed at both ends, as is practically the case in skeleton concrete construction, it is evident that it must on hardening be under an initial tensile stress due to contraction. Conditions will not be changed by the presence of steel reinforcement provided the ends are absolutely rigid. If, however, the ends of the beam yield at all, and the concrete adheres to the steel, the strains will be adjusted between the two. The steel will be in compression and the concrete in tension, but to a lesser extent. In the review of the literature at the commencement of this paper attention was directed to the experimental work of Considere and to the experiments of Emerson, who found that even with mixtures as lean as those used in practice, compressive stresses of 2,500 lbs. per sq. in. might be developed in the steel through the volume changes in the concrete of bars only 3 ft. long and not anchored at either end. The paucity of information on this subject should be emphasized. There is every probability that there are important stresses in all reinforced concrete work due to volume changes in the concrete of which the designers have had no knowledge. These stresses may have been to blame for some of the collapses which have occurred in reinforced work, and the small number of such collapses may be more a tribute to the value of the factor of safety than to scientific knowledge.

It is encouraging to note, however, that in buildings, where the beams remain dry, most of the contraction will have taken place at the end of the first three months and that from that time on the volume changes will be slight. Concrete floor beams are treacherous during the first few weeks not only because the concrete itself is new and weak but because of the tensile stresses developed.

There is danger that a rather slight load on a green beam may cause cracking if not total failure, for it is entirely possible that the whole of the concrete is in tension and its tensile strength at that stage is very small. Several weeks should be allowed to elapse even in warm weather before the forms are taken off and the supports removed. Fortunately these tensile stresses seem to practically reach their maximum in three months, while the concrete gains in strength for a much longer period, so that if a concrete building is successfully completed there does not seem to be a probability of future failure due to changes within the concrete.

CAUSES OF CHANGE OF VOLUME.

There is no experimental work to account for these volume changes. Their progressive nature as the cement hardens in water or air makes it evident that they are connected with the chemical processes of hardening. Since the hardening of cement is recognized as being essentially a process of colloid formation and change, it may seem not unreasonable to hold that the changes are connected with the volume of the colloid water. The nature of the volume changes as cement passes from the dry to the wet state is in harmony with this view. It is probable that the volume changes are also due in part to purely capillary phenomena, for sandstones and other building stones expand when wet and contract when dry. The amount of the expansion and contraction of concrete decreases as less cement is used, so that it appears that the changes are more a function of the cement than of the mineral aggregate.

The subject has not been sufficiently studied to show how far such factors in the manufacture of cement as chemical composition, temperature of burning, fineness of grinding, and rate of setting, influence the result; nor how far conditions surrounding its use, such as amount of water, temperature of air, etc., affect it. Until the influence of these factors has been determined it will not be possible to make intelligent progress towards lessening the volume changes and increasing the reliability of structures containing Portland cement. The subject is of such importance as to merit most thorough investigation from both the chemical and mechanical side on a larger scale than has yet been attempted.

CONCLUSIONS.

Concrete hardening in a moist place and remaining continually moist throughout its life expands slightly, but the compressive stress developed in the concrete is probably not large enough to be injurious.

Concrete hardening in air and remaining continually dry contracts. The contraction is roughly twice as much as the expansion of concrete continually damp. This greater stress is harmful not only on account of its magnitude but because it puts the concrete in tension, where it is weakest. It introduces an error of unknown magnitude into the calculations for all reinforced beams. It is responsible for the cracks in the top coat of interior cement floors. Fortunately most of the bad effects become evident within three months.

Cement sidewalks, pavements, and stuccos exposed to the weather contract when dry and expand when wet. The richer the mixture is in cement, the greater is the volume change, so that there is a differential expansion between the top coat and base of sidewalks which is often sufficient to split the two apart. So also rich stucco may be split from brick. In damp climate irregular expansion occurs apparently for as long as twenty years. Its harmful effects may not appear for many years. Large expansion joints are necessary in sidewalks and pavements.

Rich stuccos will inevitably crack through expansion and contraction and may be ruined. The few experiments made indicate that integral waterproofing compounds do not prevent or lessen the changes in volume. The only safety seems to lie in the use of lean mixtures whose volume change is slight. A stucco coat is inadequate as a protection for metal lath. If it is rich enough to keep water out it will crack. If it is lean enough to avoid the cracks water will go through it freely. In a dry climate the tendency is for sidewalks and stuccos to shrink more than they expand. Under these circumstances they will be much more durable, and because of the slower corrosion stucco may be permissible as a protection for wire lath.

DISCUSSION.

Mr. Humphrey.

MR. R. L. HUMPHREY.—Mr. Chairman, I should like to compliment Mr. White on the extremely interesting work that he is doing. I have had the good fortune to visit the University of Michigan and have seen this work and that upon related lines. One cannot but be impressed by the somewhat startling results stated in this paper. If these statements were to be taken in a strict sense I am afraid we should all become deserters of concrete.

I should like to make a few remarks on some of the points made by Mr. White, referring first to the concrete in Boston harbor. I have inspected the wall mentioned by Mr. White, and after a careful examination of the work, and an interrogation of the men responsible for its construction, I learned that the failure was the result of bad workmanship. I believe that is a matter of record in the Bureau of Yards and Docks. The concrete was very badly made. Its permeation by sea water, coupled with the action of the frost in winter, caused the sloughing off of the concrete. If the theory advanced by Mr. White to explain such failures is true, then we should apparently expect similar failures in other concrete walls under similar conditions; but we do not find that to be the case. It has been my good fortune to visit the principal concrete works not only in this country but in Europe; and it is the conclusion of everyone I have interviewed who has had much of that work to do, and of those who have had charge of investigations of the effect of sea water on concrete, that when concrete is properly made and of the requisite density, it is not materially affected, if at all, by the action of sea water. It is certainly a fact that there is no evidence of disintegration under such conditions.

There is no doubt that the question of expansion and contraction has a vital effect on our concrete structures; and it seems to me that one phase of this matter might be further developed by Mr. White if he would take a larger mass. These little specimens are about an inch in section and about four inches long, and those who have experimented on the penetration of water know that water will penetrate through such small masses, and the effect of the water penetrating a small mass will by very much greater than if the mass were of larger size. Therefore, while these laboratory experiments are interesting they require to be borne out by tests on larger test pieces before the results should be accepted. **Mr. Humphrey.**

There is no doubt as to the fact that troweling the surface and thus producing a dense skin of greater strength perhaps than the material underneath, would cause the concrete to be less affected by the action of moisture; but temperature stresses would nevertheless arise. The net effect is due to a combination of several causes.

Mr. Cushman. MR. A. S. CUSHMAN.—I think that this is the most interesting paper and discussion which I have heard for a long time. It opens up a very attractive line of research. We are all familiar with changes of volume from expansion and contraction under temperature conditions, but here we are discussing changes in volume of very considerable magnitude under the influence of wetting and drying. Now we all know that there is considerable doubt about the reason for the setting of Portland cement. There have been a great many explanations offered, none of which are entirely satisfactory. Here we have a plain case of expansion of set concrete when it is wet. Now what can it be that expands after soaking in water? It is certainly not the sand; for we know that sand grains do not expand on wetting. It certainly cannot be crystalline bodies in the cement mass because crystalline bodies do not expand when they are wet with water. It is characteristic of colloids that when they are wet with water they expand; when they dry out again they contract. Some years ago while I was studying the plasticity of clays and the action of water on rock powders, I noted that when materials like plastic clays are in an air-dry condition and are put into moist atmospheres, they absorb water and gain in weight. If, however, these clays are first heated gently in a hot-air bath at a temperature of say 100° F., they to some extent lose the power of absorbing water. I further noted that successive heatings of the same sample of clay reduced the power of absorbing water and losing it again on air drying. By continuing the

Mr. Cushman. heat to a certain critical temperature which was far below the clinkering point the absorbent property of the clay could be quickly destroyed, due to the destruction of the colloids. This was also found to apply in the case of rock powders. I found that a rock powder that had been ground wet produced a condition of the powder that enabled it to absorb water in a moist atmosphere, and lose it again in a dry atmosphere; but if this same wet ground rock powder were heated in an air bath to some critical temperature it entirely lost the power of absorbing, and losing, and reabsorbing moisture.

I reached the conclusion which I then published, that the cementing value of rock dust in road building and in other ways was due to the formation of colloidal decomposition products under the action of water.

Now, if I am right in applying these observations to the theoretical discussion of the subject before us, it should be possible to put the problem to a critical test. Why not take these test bars and heat them in an air bath or oven to various temperatures, and then measure the expansion and contraction from alternate wettings and dryings? If the theory holds, we should by heating reduce the power of expanding when wet and contracting when dry. In short we have a simple means of submitting to critical experimental examination a theory that has a very broad application to a number of industrial problems.

I am particularly interested in this subject from the theoretical point of view, but from the engineer's point of view it is also important. We know that colloids once formed are not going to last forever. That is to say, old concrete is undoubtedly not so rich in colloidal material as new concrete. It will be admitted, I think, that concrete even a year or two old will probably be richer in colloidal material than old concrete that has set for many years in dry atmospheres, because these colloids are gradually destroyed at considerably lower temperatures than the critical temperature I have spoken of.

This explanation may apply to a number of problems that have puzzled us; and I fully believe that it has given us a clew to a very interesting field of investigation. It is possible that some of the difficulty met with in concrete work in arid regions may be explained by the too rapid destruction of the cement colloids. **Mr. Cushman.**

MR. HUMPHREY.—I should like to say a word in answer to Mr. Cushman's theory about trouble with concrete in the arid regions. If that is really true, then the disintegration of concrete due to that cause would be the same in Montana as in Arizona, or any other portion of the arid regions. That, however, is not the case. The disintegration of concrete by alkalies is found only in certain territories where the black alkali is found, and is probably due to the rapid formations of crystals of the double salts that are found there, through rapid evaporation. So I think that that theory cannot be applied to the alkali territory. **Mr. Humphrey.**

MR. S. E. THOMPSON.—Measurements of various walls, subjected to ordinary weather conditions of heat and moisture, indicate that, in concrete structures, the changes in volume can be explained simply by the expansion and contraction to be expected from the temperature variation. For example, measurement of the opening of joints in a long retaining wall and, in another case, in a freight house several hundred feet in length, showed the contraction to be almost identical with that figured from the ordinary coefficient of expansion. In ordinary concrete structures made with a good dense mixture, I am of the opinion that no consideration whatever need be given to the swelling or shrinkage of concrete from moisture except during the initial hardening. **Mr. Thompson**

Referring to the concrete in Boston Harbor, an examination of the disintegrating concrete shows that the water penetrated entirely through the mass. Furthermore, it is wet all the time both at high and low tide. It seems scarcely possible, therefore, that the disintegration can be caused in any way by the alternation from wet to dry.

VICE-PRESIDENT R. W. LESLEY (*in the Chair*).—There are two thoughts that come to me in connection with this matter. The first is in reference to stuccos. I do not know whether Mr. White has examined at all the modern lime stuccos which are applied to a great number of houses, all of which show lime cracks. I do not know whether the principle of colloids will apply there, or whether any experiments have been made to determine why that cracking goes on. **The Chairman.**

The Chairman. Another matter is in the line of Mr. Page's discoveries regarding oil concrete, which might open up a very wide field for further investigation on the same line. Then mortars probably might not be affected by a change from dry to wet, as ordinary waterproof compounds, such as shown in one of these tables.

Mr. Humphrey. MR. HUMPHREY.—I should like to ask Mr. White in reference to the sidewalk question which he brings up in this very interesting paper, as to whether or not there is any information available as to the character of the workmanship. It seems to me that the inference from the paper is that the disintegration of these sidewalks was due entirely to alternate drying and wetting. Is it not probable that it may have been due in part to faults in workmanship?

Mr. Boynton. MR. C. W. BOYNTON.—I should like to have Mr. Cushman state how he harmonizes his colloidal theory with the fact that the cement was burned at a high heat.

Mr. Cushman. MR. CUSHMAN.—I do not think you quite understood me. Of course there is no colloid in the cement before it is wet; neither is there in an igneous rock powder, which is nothing more than a material clinkered or burned by nature; but when water acts on the cement particles, the decomposition products appear in a colloidal form. There is no colloidal material in the burned cement, but there is colloidal material formed after water acts upon the cement. There seems to be a growing consensus of opinion that the set of Portland cement is due to the formation of colloidal material and the reinforcement of the colloidal material by crystalline substances. Calcic hydroxide, and also probably monocalcic silicate, are the crystalline substances formed during the hardening processes. Much remains, however, unexplained in regard to the reactions which cause the hardening of cement mortars.

Mr. Douty. MR. D. E. DOUTY.—I should like to ask Mr. White how he measures the changes in lengths.

Mr. White. MR. A. H. WHITE.—Several questions have been asked me and I shall endeavor to reply to them in their order, but I wish to say first that the change in length was measured by a micrometer which was designed by Mr. Campbell of the University of Michigan, reference to which you will find in the paper. It is quite a simple one, and we find that it is extremely accurate. It is also due to Mr. Campbell to say that he perfected another device for inserting glass plates in the expansion bars which obviates a great deal of the difficulty in making expansion measurements of this kind. **Mr. White.**

With regard to the suggestion of Mr. Humphrey that we ought to try it out on a larger scale, of course we ought; I hope we will. But it takes a great deal of time and a considerable means. I hope that we shall get to it eventually.

In regard to Mr. Cushman's colloidal theory, I agree with him entirely that it is almost certainly colloidal action that is responsible for the changes in volume discussed here. I purposely omitted all theoretical discussion from the paper, because I wanted to focus attention on the practical features, and the theoretical discussion will appear at some other time.

Mr. Kinney. MR. W. M. KINNEY.—Mr. White made a statement with reference to a sidewalk or a section of a sidewalk at the University of Michigan which did not show satisfactory results, in which he found that expansion took place. Now it is a very simple thing to obtain a lot of samples from sidewalks that are entirely satisfactory and that have been in place for the same length of time. Therefore I should like to ask whether Mr. White has made any tests to determine if the expansion in the samples of satisfactory sidewalks was less than in the case of the walk that failed; similarly with regard to stucco.

Mr. White. MR. WHITE.—I have one sample that I got from a sidewalk that was entirely good, approximately twenty years old, but which had not been laid in as long lengths as the other walk, so that there was no failure due to expansion. The particular point which interested me was that the contraction of the base and the contraction of the top are almost identically the same in spite of the fact that the top contained only fine sand and was, I presume, very much richer than the base. I do not know how to explain this fact.

Mr. Humphrey has asked whether the workmanship on the original walk was good. I was not there, but it was laid by the University's own workmen and no doubt it was put in as well as they knew how. I know that it was laid without any expansion joints the entire quarter of a mile. The peculiar thing about it is that upheavels due to expansion still persist after twenty years.

Mr. Cushman. MR. CUSHMAN.—I should like to say a word on this. We must not lose sight of the fact that expansion and contraction is continually going on in cement sidewalks, a great deal of which has nothing to do with the particular point on which we now have our attention focused. Temperature changes ranging between the extreme below-zero temperatures of winter and the maximum summer heat of 120° to 130° F., will produce contraction and expansion. In addition to these there is the diurnal expansion and contraction due to the sun shining on the walk by day and its cooling down at night. But we must not confound these temperature effects with the one which we are now discussing, namely, the expansion and contraction due to alternate wetting and drying.

Mr. Humphrey. MR. HUMPHREY.—I do not want to dwell too much on this sidewalk question, but I think that inasmuch as two practical examples were cited as possible examples illustrating this theory, it should be brought out clearly that there is no evidence that this expansion and contraction which Mr. White has observed accompanying the alternate wetting and drying, was the absolute cause of the failure of the sidewalks mentioned. There are many good sidewalks all over the country laid by skilled workmen that do not show those effects. It seems to me that the workmen may have been lacking in skill, although conscientiously trying to do good work, and that the failure might have been due to bad workmanship and is not explainable directly on the theory advanced.

THE EXPANSION AND CONTRACTION OF CONCRETE WHILE HARDENING.

By A. T. Goldbeck.

Although it is understood in a general way that concrete upon hardening in the air exhibits shrinkage and that when kept moist this shrinkage is either decreased or changed to expansion, published quantitative results in connection with these phenomena are very meager. Numerous measurements have been taken on neat cements and mortars with the use of the Bauschinger or similar apparatus, but the amount of expansion and contraction of concrete mixtures is less definitely known. The problem of the shrinkage of concrete being considered of importance in connection with the study of the development of cracks in concrete pavements and roadways, the present series of measurements were undertaken under the direction of Mr. Logan Waller Page, in the testing laboratory of the Office of Public Roads, United States Department of Agriculture, to determine the amount of this contraction.

Materials.—All concrete mixtures were proportioned by weight, the materials being "Old Dominion" Portland Cement, river sand and crushed gneiss. The cement passed the specifications of this Society. Mechanical analyses of the sand and stone used are given below:

Sand.

Sieve	Per cent. retained.
¼-in	3
⅛ "	11
No. 10	17
" 20	42
" 30	66
" 40	87
" 50	93
" 80	96
" 100	99

Stone.

Sieve	Per cent. retained.
¾-in	28.4
½ "	66.3
¼ "	92.1
⅛ "	98.4

The sand contained 37 per cent. of voids; the stone 43 per cent.

Specimens.—The specimens were made 8 ins. in cross section and 5 ft. long, and were molded as columns so that one end would be absolutely free to expand and contract, due to any cause. Concrete of very wet and of very dry consistency was used, mixed in proportions of 1 : 2 :4 and 1 : 3 : 6. The specimens of very dry consistency required hard tamping to consolidate the concrete in the mold, while those of wet consistency were made by simply puddling the mixture into place. The amount of water used for the dry mixtures was 8.5 per cent. of the weight of the dry materials. For the wet mixtures from 10 to 12 per cent. was used.

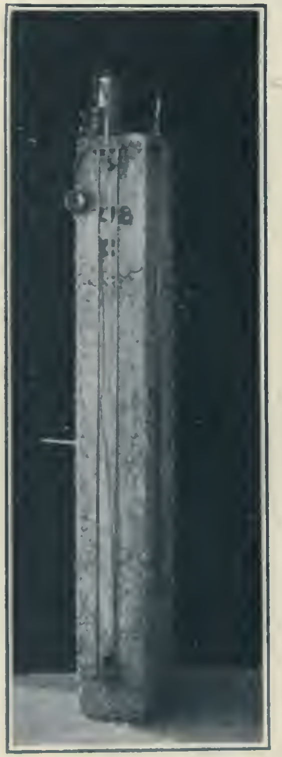


FIG. 1.—Contraction Specimen with Measuring Apparatus Attached.

Measuring Apparatus.—In order to measure the expansion and contraction, ½-in. square bars 11 ins. long were molded in the specimens, at a distance of 50 ins. apart. Upon removal of the wooden molds, these bars projected 1½ ins. on each side of the specimens, and to each of them two steel plugs, ¼ in. in diameter and having slightly rounded conical points, were fastened. Between these points measurements were taken by means of a Brown & Sharpe micrometer head, fastened to one end of a rectangular steel frame, consisting of two long bars ¼ in. in diameter connected by transverse end blocks. One of these blocks carried the micrometer head while the other block carried a square-ended steel plug. For ease of manipulation, the micrometer head at one end and the plug at the other end of the frame were surrounded by hard rubber guide collars, which served to maintain the frame in position while readings were being taken. Electric contact was used during the early part of the tests, a telephone receiver being placed in circuit. With this apparatus, readings could be taken checking each other to 0.0001 in. As the tests progressed, it was found that by careful handling of the micrometer it was not necessary to use the electric contact. A specimen with the measuring apparatus attached is shown in Fig. 1.

The initial readings on all specimens were taken as soon as the concrete had hardened sufficiently to remove the molds. At the same time the temperature of the air in the vicinity of the specimens, and that of the specimens, were taken. The temperature of the specimens was obtained by inserting a thermometer in a small hole molded in the concrete, that of the air being read from a thermometer hung alongside of the specimen. Before taking any readings, the micrometer was read on a gage bar of constant length. In this way, errors due to disturbance of the micrometer head in the frame or to wear in the micrometer screw were eliminated. All the readings were reduced to 20° C. so that the plotted deformations represent those due to changes other than temperature changes.

Storage.—Specimens 195, 288, 272, and 271 were not moistened from the date they were made, but were simply allowed to dry out in the air, the temperature of which varied from about 20° to 29° C., the average being about 24°. Specimens 278, 236, 277, and 248 were enveloped in a double thickness of burlap which was kept moist by sprinkling twice a day for a period of about 15 days. Specimen 284 was likewise covered with burlap and kept moist continuously.

Results of Measurements.—Referring to the curves, Fig. 2,* it will be seen that the specimens which were allowed to dry out immediately after molding began to shrink almost from the day they were made, although the very wet mixtures for the first few days showed a tendency towards slight expansion, amounting to about 0.00005 in. (not plotted).

The rate of shrinkage of the dry mixtures is very slightly higher at earlier periods than that of the wet mixtures. There is, however, very little difference between the shrinkage of the different mixtures except that the contraction of the wet specimens is delayed. The unit contraction of the oldest specimen, No. 195, of very dry mixture, is 0.00052 in. at the age of 3 months, and the rate of contraction after this age is very small. The contraction

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*Acknowledgment is made to the *Engineering Record* for the cut of Fig. 2.—ED.

curves of the other mixtures follow closely along that of No. 195, as far as readings have been taken. These results indicate that the approximate contraction of concrete of the proportions and consistencies ordinarily used is 0.05 per cent. at the age of 3 months.

Assuming the coefficient of expansion of concrete to be 0.0000055 per degree Fahrenheit, a value obtained by Professor Norton and others, the fall in temperature of concrete required to produce the same contraction as that caused by drying out is $0.0005 \div 0.0000055 = 91°$ F.

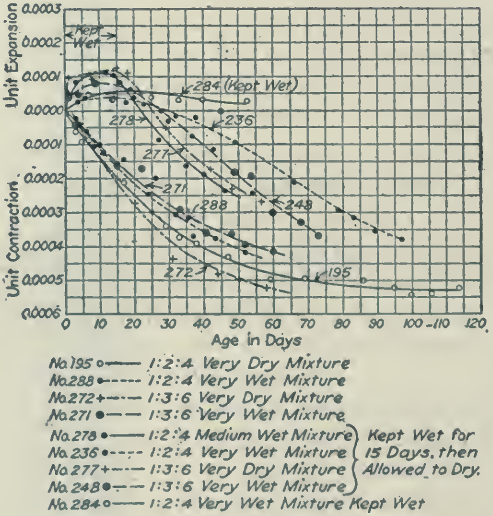

FIG. 2.—Expansion and Contraction of Concrete while Hardening.

If the ends of a concrete construction were confined, the tensile stress produced by shrinkage would equal 2,000,000 multiplied by 0.0005, or 1,000 lbs. per sq. in., the value of the modulus of elasticity of concrete in tension being assumed at 2,000,000 lbs. per sq. in. Since the tensile strength of concrete varies from only 100 to 200 lbs. per sq. in., under proper conditions, cracks due to shrinkage should occur long before the end of 3 months. In fact shrinkage cracks should develop, under the conditions to which these specimens were subjected, during the first few days of curing.

The specimens which were kept moist for 15 days (Nos. 278, 238, 277, and 248) showed expansion as long as they were wet.

However, as soon as they were allowed to dry out contraction began. The indications are that dry mixtures are more sensitive to expansion upon being moistened than are the wet mixtures. The amount of expansion is, however, quite small, reaching as a maximum approximately 0.0001 in. per inch of length. This corresponds to a compression of 200 lbs. per sq. in., assuming confined ends and a modulus of elasticity of 2,000,000. It will be seen that the rate of shrinkage of the moistened specimens, when allowed to dry out, approximates that of the air-cured specimens, so that one of the effects of moistening the concrete for the first few days is to delay the shrinkage. It is probable that ultimately the amount of contraction of both air- and water-cured specimens will be identical. Referring to the curve for Specimen 284, which was kept in a moistened condition continuously, it will be seen that although this specimen does not expand progressively, an expansion is maintained up to the date of the last measurement.

It was noted during the progress of the measurements that specimens which were allowed to dry out and were accidently moistened by heavy rains, expanded, showing by actual measurement that concrete behaves very much like timber, swelling when moistened and contracting upon drying.

CONCLUSIONS.

1. Concrete upon hardening in air contracts.
2. When kept moistened during the setting process, concrete expands and if allowed to dry out after a small initial period of water curing, it will then contract.
3. The contraction of mixtures of wet consistency is delayed somewhat beyond those of dry consistency.
4. The amount of contraction of air-cured concrete is influenced very little by the proportions or consistency of the mixture.
5. For practical purposes the maximum contraction of air-cured concrete seems to be reached at the age of 3 months and equals approximately 0.05 per cent.
6. If moistened after drying out, concrete will expand.
7. It is probable that the shrinkage of concrete, under conditions which do not interfere with its rapid drying out, is a far more potent factor in the formation of cracks than contraction due to external temperature changes.

DISCUSSION.

Mr. Berry.

MR. H. C. BERRY.—At the request of Mr. Goldbeck we made some observations on two samples of the Gneiss used as an aggregate in the concrete from which his expansion specimens were made. For this purpose the Bauschinger apparatus belonging to the Lesley Cement Laboratory of the University of Pennsylvania was used. The samples of stone were dressed to accommodate the range of the apparatus, about 2 by 2 by 10 cm. Instead of using metal contact points at the end, center drill marks were made in each end. The results were as follows:

TABLE I.—UNIT CHANGE OF LENGTH OF GNEISS STONE. (BAUSCHINGER APPARATUS).

Storage.	Sample No. 1. Total Unit Contraction.	Sample No. 2. Total Unit Contraction.
33 days in water	0.00025	0.00031
3 " at 150° F	0.00030	0.00045
7 " " 70° F	0.000375	0.00045
26 " " 70° F	0.000635	0.00053
131 " " 70° F	0.000365	0.00025

Weight decreased 0.06 per cent. but remained constant from 26 to 131 days.

When Mr. Goldbeck found the contraction of the concrete to be greater than expected, he suggested that we test some mortar specimens in order to compare the results with those obtained for concrete. Accordingly we made up specimens of neat cement, and 1 : 2 and 1 : 3 mortar with both Ottawa standard sand and the bar sand used in concrete work about Philadelphia, thirty in all. As the results agree very closely with those given in the paper by Mr. White,* only the average total change for each mortar for a period of four months is reported in Table II. For the early periods, the specimens stored in water showed slight expansion, as indicated on the curves given by Mr. White.

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* See p. 531.—ED.

Mr. Berry. A comparison of the values above with those on the curves given by Mr. Goldbeck indicates that cement mortar stored in air contracts about five times as much as concrete, while when it is stored in water the contraction is about the same as for concrete kept in air. The sand mortars show approximately the same results as concrete when both are stored in air. It is also seen that mortars stored in water suffer less contraction than those stored in air, the ratio being about one to three.

This agrees with the indications from Mr. Goldbeck's curves that concrete which is kept wet contracts much less in hardening than that which is allowed to dry out.

Some advance reports of the very large contraction observed by Mr. Goldbeck together with the fact that the two laboratories had quite different aggregates, led us to make a few specimens of concrete for observation of changes during hardening. Our six specimens differed from those of Mr. Goldbeck in being 9 instead of 8 ins. square, and in having 20-in. instead of 50-in. gage length. The measuring apparatus is of about the same degree of sensitiveness, but of very different type. We took measurements on the four sides of the specimens while he read on two opposite sides only.

TABLE II.—AVERAGE UNIT CHANGE IN LENGTH OF MORTAR SPECIMENS AT FOUR MONTHS. GIANT CEMENT.

	Kind of Mortar.	Stored in Air.	Stored in Water.
	Neat cement	0.0026 (contraction).	0.0006 (contraction).
1 : 2	Standard sand	0.0017 "	0.0006 "
	Bar sand	0.0009 "	0.0001 "
1 : 3	Standard sand	0.0014 "	0.0006 "
	Bar sand	0.0009 "	0.0004 "

Two of the specimens were of 1 : 2 : 4 concrete made with Giant Portland cement, bar sand and 1¼-in. trap rock, crusher run. Two were of 1 : 2½ : 5 concrete using the same materials. One of each was allowed to harden in air in the laboratory and the other completely submerged in a large tank of water. Later we made two specimens of 1 : 2 : 4 concrete with Universal Portland cement and the same aggregates as the others. All the concrete was mixed wet and troweled into the molds.

Since the temperature of the laboratory did not vary more

Mr. Berry. than four or five degrees at the time the readings were made, no temperature corrections have been made in working up the observations. The change in the temperature of the water of the storage tank was less than four degrees.

The initial observations were made within 18 hours after casting the specimens; on two of them within 4 hours. While consistent observations were made each day for a considerable time, only the changes after 5, 10, 30, and 150 days are reported in Table III. The latter were checked by observations 2 days apart.

TABLE III.—UNIT CHANGE IN LENGTH OF CONCRETE SPECIMENS.

(*a*) 1 : 2 : 4 CONCRETE.

Age.	Stored in Air. (3 specimens.)	Stored in Water.
5 days	0.00003 (contraction.)	0.000010 (expansion.)
	0.00006 "	
	0.000055 "	
10 "	0.000047 "	0.000007 (contraction.)
30 "	0.000091 "	0.000093 (expansion.)
	0.000180 "	
	0.000195 "	
150 "	0.000220 "	0.000200 (expansion.)
	0.000220 "	
	0.000250 "	
	(*b*) 1 : 2½ : 5 CONCRETE.	
5 "	0.000002 (contraction.)	0.000006 (expansion.)
10 "	0.000025 "	0.000026 "
30 "	0.000088 "	0.000073 "
150 "	0.000170 "	0.000020 "

These results are considerably smaller than those obtained by Mr. Goldbeck. It would require a change of about 38° F. to cause a change in length equivalent to that noted above for 1 : 2 : 4 concrete kept in air. This would induce a stress of about 500 lbs. per sq. in., assuming the specimen rigidly held at the ends, and using the usual value of the modulus of elasticity. The expansion noted is greater than that reported by Mr. Goldbeck. This may in part be due to the fact that the specimens were immersed continually.

MR. R. J. WIG.—I should like to ask Mr. Goldbeck if the concrete would expand if it were immersed in water, after storage in air? **Mr. Wig.**

MR. A. T. GOLDBECK.—Judging from the way the specimens that were kept moist behaved, I think there would be an expansion, although I have not made any measurements of that kind. I have not actually immersed the concrete in water; the specimens that I kept moist were simply wrapped in a double thickness of burlap, and the burlap kept moist. I have every reason to believe that the concrete within the burlap was moist. **Mr. Goldbeck.**

MR. WIG.—What I should like to know is whether the concrete, after storage in air for say one hundred days, would expand if placed in water. **Mr. Wig.**

MR. GOLDBECK.—During the progress of the test of Specimen 236 I noticed that after a heavy rain which thoroughly wet the specimen, it became slightly longer than it was before the rain. This would seem to indicate that, upon wetting concrete which has become dry, an expansion will take place. **Mr. Goldbeck.**

SOME EXPERIMENTS ON THE INCRUSTATION AND ABSORPTION OF CONCRETE.

BY A. O. ANDERSON.

A few years ago, when the controversy between the makers of cement and clay drain tile was at its height, one of the objections raised to the use of cement drain tile was the formation of incrustations, the continued growth of which it was claimed would eventually cause the stoppage of the drain. Two instances were cited in which incrustations were actually forming in Iowa drains, one being in Calhoun County and the other in Palo Alto County. An examination of the tile from both places was made by the Engineering Experiment Station of the Iowa State College, which showed the incrustations to consist of particles of earth and sand cemented with calcium carbonate. The writer, upon making an examination of the incrustations and the conditions under which they were forming, found that although these conditions seemed dissimilar, yet they were sufficiently alike that the explanation of the one growth would do for the other.

The Calhoun County drain is of twenty-inch and smaller tile, the incrustations occurring in the large tile near the outlet. These tile were installed in the trench about two years before they had a free outlet, since the construction of the dredge ditch into which they discharge was two years late in reaching the tile drain outlet; and during this time the tile were constantly submerged in water ranging from three to seven feet in depth. The drain was located in a natural draw which took care of the discharged surface water, thus allowing the water in the drain to become almost stagnant. The tile at the Palo Alto County drain had not been installed but had been distributed along the line of the drain in the winter. When spring came, the tile sank through the sloughs into bodies of water which, during the greater portion of the year, were stagnant. Other trouble having developed with this drain the tile were allowed to remain several years.

The Calhoun County tile were hand tamped in removable molds which necessitated quite a dry mixture of the concrete, leaving the surfaces rough and porous. The other tile were made in a machine having a revolving forming head which troweled the inner surface of the tile very smooth, leaving the outer surface and ends rough and porous. In both makes of tile a rather lean concrete was employed, approximately a 1:5 mixture. It was principally upon the rough porous surfaces that the incrustations formed, being especially prominent in both instances on fractured surfaces.

Since the tile in both cases were in stagnant water, and in one case were lying on top of the soil, it did not seem probable that the incrustations were due to the solution of soluble minerals from the superimposed soil and their precipitation and deposition upon the concrete from water percolating into the drain because of a cotalytic action upon the dissolved calcium carbonate, as has been suggested by some investigators. Neither does the theory that they were caused by the solution of the cement seem plausible, for one would then expect the concrete to weaken with age; such a weakening would be most pronounced in drains having a free outlet and a large discharge, which has not been found to be the case.

From observations made upon test briquettes stored in stagnant water, it occurred that the incrustations of these tile might be due to the same cause as the formation of the limy crusts found floating upon the water and the incrustations on the briquettes themselves. A number of specimens were cut from the incrusted tile and from other tile of various ages and conditions for the experiment. One set was placed in open jars half filled with clean "rain water," while a duplicate set was placed in similar jars sealed air-tight. All of the specimens were cleaned and scraped free from any adhering deposits or incrustations before being used for this investigation. The results of this test are shown in Table I. As may be observed, practically no incrustations were formed on the specimens kept in the sealed jars, since the water therein was isolated from the air with its content of carbon dioxide; while in the open jars the youngest and most permeable concrete yielded the largest "crop" of incrustations.

From these facts, the following theory as to the formation and growth of incrustations on concrete is presented. The incrustations come from the soluble salts found within the concrete. Since these are due principally to the lime set free during the setting

TABLE I.

RESULTS OF THE INCRUSTATION EXPERIMENTS ON CONCRETE TILE.

Specimen No.	
12	From tile in service for 32 years. Kept in a sealed jar for 670 days. No sediment formed, nor incrustation on specimen.
13	From same tile as No. 12. Kept in an open jar for 670 days. A little dark-colored sediment was found in the jar and a slight trace of an incrustation seemed to appear on the fractured surfaces of the specimen.
17	From a 16-in. machine-made tile about six months old; mixture 1:3.5. Kept in a sealed jar for 670 days. A heavy deposit of dark-colored sediment formed in the jar and the fractured surfaces of the specimen were slightly incrusted, but the smooth inner surface was free from incrustations.
19	From a 20-in. hand-tamped tile about two months old; mixture 1:4. Kept in an open jar for 670 days. The specimen was covered with a heavy incrustation and the sides and bottom of the jar coated with a dense sediment.
20	From the same tile as No. 19. Kept in a sealed jar for 670 days. The water was covered with a thin disk of a white substance similar to that found floating in storage tanks for test briquettes, and a dense sediment was present in the bottom of the jar. The specimen was incrusted but not as much as No. 19.
30	From a 20-in. tile which had been in service for four years and had developed a heavy growth of incrustations while in the ground. These incrustations were all scraped off before the specimen was placed into the jar and sealed. Kept in the jar for 670 days. The specimen was slightly incrusted on the fractured surfaces only, and a white flocculent precipitate floated in the water.
31	Same as No. 30, but kept in an open jar for 276 days. A dark-colored sediment formed in the jar and a slight incrustation formed on the fractured surfaces of the specimen. These two specimens are from a tile drain cited as a case where the drain was in danger of clogging from the growth of incrustations.
32	Specimen from a 10-in. machine-made tile one month old; mixture 1:3. Kept in a sealed jar for 276 days. A dark-colored sediment formed in the jar, but very slight traces, if any, of incrustations were found on the fractured surfaces only.
33	Same as No. 32, but kept in an open jar for 670 days. Traces of incrustations were found on the fractured surfaces of the specimen, with a sediment both floating and attached to the bottom of the jar.
A2	From a 20-in. tile, hand-tamped, mixture 1:5, and had been in drain for 3½ years. Kept in a sealed jar for 670 days. No incrustations were found on specimen, but there was a flocculent precipitate floating in the jar.
A4	Same as A2, but kept in an open jar for 276 days. No incrustations were found on the specimen, but a fine white sediment had formed in the jar.
A6 and A8	From an 18-in. tile in same drain as A2 and A4. The results were correspondingly the same.

of the cement, which in fresh concrete may occur as calcium hydrate and the calcium sulphate used as the retarder, the youngest concrete or that kept away from the leaching action of percolating water will yield the most incrustations. Table I shows that the specimens from the tile which had been in service for the longest periods yielded the least incrustations both in the open and sealed jars.

The incrustations form only on concrete tile which are in stagnant or very slowly moving water, as shown by the fact that of the many thousand miles of concrete drain tile installed only a few instances have been reported where incrustations have been found. This is due to the fact that in still water the water next to the concrete, becoming charged with soluble salts, remains in contact with it, and if there is enough carbon dioxide held in solution the resulting calcium carbonate is precipitated and deposited as an incrustation, instead of being washed out of the drain as it is when the water is flowing freely.

Smooth dense surfaces on concrete inhibit the formation of incrustations, because the necessary troweling action brings neat cement to the surface, forming a dense, impermeable covering which prevents the transference of the soluble salts from the inner portion of the mass to the outer. On the other hand fractured, rough, and porous surfaces promote the growth of incrustations, such surfaces allowing this transference of the soluble salts to the outside where they are precipitated by the chemical action of the gases and minerals dissolved in the surrounding water.

The formation of incrustations and concretions in drains conducting water free from alkali may be prevented by making the tile from a wet mixture so proportioned as to secure a dense concrete. The inner surface should be troweled so as to bring a layer of neat cement to the surface, and the tile should at all times have a free outlet in order to prevent the stagnation of water in the tile. As a matter of fact, the incrustation if formed will be of little consequence, since it will cease upon the solution of the soluble salts in the concrete unless the water itself is highly charged with mineral substances.

In connection with the investigations of the incrustation of cement tile, a large number of absorption tests were made under a variety of conditions, the results of which are given in part in

Tables II to V, inclusive. Data showing the effect of the size of the specimen are given in Table V; the effect of the length of immersion is seen from Tables II and IV, which also show the effect of immersion of the specimen in boiling water.

Other investigations were also carried on, the most important being the determination of the change in weight of a specimen during its immersion. Since concrete contains soluble salts and the cement itself is affected by water, such changes in weight as discovered were not unexpected. New concrete when placed in water is subject to two changes in weight, a decrease due to the solution of the soluble matter and an increase due to the setting of the cement which takes up water of crystallization. The change in weight during the test is liable to cause a net increase or decrease in weight, and the time of immersion should therefore be as short as possible to secure uniform results. The time usually specified in Iowa is twenty-four hours, but even in this short period

TABLE II.—RESULTS OF ABSORPTION EXPERIMENTS.

(Values given are Percentages of Absorption.)

SERIES VII.

Specimen No.	Usual Test in Cold Water.			In Boiling Water.	Usual Test in Cold Water on Specimens after Boiling Test.				
	48 Hrs.	72 Hrs.	96 Hrs.	6 Hrs.	10 Hrs.	20 Hrs.	48 Hrs.	120Hrs.	360Hrs.
1..........	6.43	6.48	6.50	12.75	8.88	8.89	9.20	9.57	10.51
2..........	5.38	5.41	5.43	11.95	8.40	8.47	8.67	8.92	9.38
3..........	6.35	6.46	6.64	12.29	8.34	8.38	8.52	8.86	9.37
4..........	6.35	6.42	6.44	10.07	8.75	8.78	9.20	9.28	9.73
5..........	3.94	4.50	4.30	9.84	8.51	8.64	8.80	8.98	9.37
6..........	3.68	3.68	3.78	10.50	7.33	7.49	7.56	7.93	8.58
7..........	4.24	4.27	4.36	9.84	7.46	7.60	7.80	8.00	8.70
8..........	5.30	5.34	5.26	12.80	8.18	8.39	8.74	9.18	9.95
9..........	6.86	7.66	7.19	13.85	9.14	9.54	9.45	10.45	11.19
10..........	4.12	4.43	4.45	9.78	6.60	6.60	6.89	7.05	7.29

SERIES IX.

Specimen No.	Usual Test in Cold Water.				In Boiling Water, 1 Hr.
	26 Hrs.	55 Hrs	100Hrs.	292Hrs.	
13.....	6.05	6.16	6.18	6.25	7.35
15.....	6.29	6.33	6.47	6.71	8.06
17.....	9.10	9.50	9.59	10.75	12.47
19.....	9.65	9.53	9.59	10.70	12.30
21.....	9.70	10.53	10.80	11.19	13.54
23.....	10.32	11.62	11.64	12.31	12.70
25.....	7.97	8.07	8.14	8.40	9.85
27.....	8.75	8.84	8.96	9.25	9.88

SERIES X.

Specimen No.	Continued Boiling.		
	2 Hrs.	3 Hrs.	40 Hrs.
14.....	8.84	8.25	9.37
16.....	7.79	7.12	7.89
18.....	10.76	10.10	11.10
20.....	12.45	11.61	12.60
22.....	13.71	13.00	13.80
24.....	13.40	12.82	14.31
26.....	9.85	8.45	9.83
28.....	9.27	8.89	9.20

changes in the weight of the specimen have been noticed which would cause a difference of from ten to fifteen per cent. in the result, depending upon whether the dry weight before or after the immersion of the specimen was used as the divisor in calculating the percentage. Most specifications state that the original dry weight

TABLE III.—COMPARISON OF COLD-WATER AND ACCELERATED TESTS.
SERIES XI.

Specimen No.	Percentage of Absorption.		Reduction Factor, $\frac{\text{Column 2}}{\text{Column 3}}$	Corrected Percentage of Absorption, (Column 3) ×0.722
	Cold-Water Test, 48 Hrs.	Accelerated Test, Boiled 1 Hr.		
1	2	3	4	5
A1	8.70	12.32	0.705	8.90
A2	8.28	11.46	0.723	8.27
A3	8.72	12.89	0.676	9.28
B1	6.93	10.70	0.647	7.72
B2	7.37	10.72	0.688	7.75
B3	7.45	10.90	0.653	7.87
C1	7.68	10.58	0.727	7.65
C2	7.14	9.63	0.742	6.95
C3	6.56	9.08	0.724	6.55
D1	7.14	9.60	0.748	6.92
D2	6.61	9.04	0.733	6.53
D3	6.63	9.01	0.735	6.50
E1	5.54	7.96	0.683	5.75
E2	8.30	11.97	0.694	8.53
E3	5.50	7.26	0.768	5.23
F1	6.15	8.60	0.715	6.72
F2	5.28	7.78	0.680	5.63
F3	6.56	10.41	0.629	7.54
G1	6.76	8.91	0.759	6.43
G2	6.48	8.63	0.752	6.23
G3	7.45	9.55	0.780	6.88
H1	10.25	14.10	0.728	10.15
H2	9.80	13.52	0.723	9.78
H3	9.50	13.45	0.706	9.70
I1	7.63	10.39	0.734	7.50
12	9.50	12.90	0.737	9.32
13	7.01	9.65	0.725	6.96
Average Value of Reduction Factor			0.722.	

shall be used as the divisor, but from the data at hand more satisfactory results would be obtained if the specimen was dried after immersion and this weight used instead of the original dry weight. Errors such as those due to the loss of weight by the loosening of sand grains, the absorption of impurities by the water, etc., would thus be eliminated.

The usual absorption test requires at least 24 hours for completion but it would often be convenient to make such a test in a shorter time. Two methods are suggested for making such accelerated tests: first, the exhaustion of the air from the vessel in which the specimens are immersed, and second, the immersion of the specimens in boiling water. The first method involves the use of an air pump and apparatus which are not always available, while a method based upon boiling the specimens is quite simple. A large number of absorption tests were made for the purpose of developing the boiling method, a few of which are given in Tables II, III, and IV. As will be noted, a short boiling causes a larger absorption than a long-time immersion in cold water, so that in order to compare the results of the accelerated test with those of the regular test, a reduction factor must be determined from a large number of tests by both methods upon identical specimens.

The method at present recommended for the accelerated test is as follows: A standard-size specimen of the material to be tested is thoroughly dried to constant weight, brushed with a stiff brush to remove any loose or adhering particles which might be detached during the test, and placed into boiling water for one hour. It is then immersed in cold water for five minutes and weighed. The increase in weight divided by the original dry weight gives the percentage of water absorbed during the accelerated test. This percentage must be multiplied by a reduction factor to give results comparable with those obtained in the cold-water test.

Table III shows results obtained by the use of both methods on the same specimens. Three specimens cut from each concrete tile were tested in the usual way, then dried and tested by the accelerated method. The results are given in Columns 2 and 3. The numbers obtained by dividing the percentages of "cold-water absorption" by the corresponding percentages of "accelerated absorption" are tabulated in Column 4. An average of these values gives the reduction factor of 0.722 by which the results of the accelerated test were multiplied in order to obtain the values given in the last column. A comparison of the values thus obtained with those of the regular 48-hour absorption test shows results well within the limits of accuracy, especially if those variations be compared with the variations found in the results from several tests on specimens from the same tile.

TABLE V.—SHOWING EFFECT OF SIZE OF SPECIMEN UPON ABSORPTION.

Specimen No.	Thickness, ins.	Large Specimen.			Small Specimen.		
		Size, ins.	Weight, grams.	Absorption, per cent.	Size, ins.	Weight, grams.	Absorption, per cent.
1.....	0.9	6 x 12	2413	6.5	3 x 4	414	7.0
2.....	"	7 x 12	2592	5.2	3 x 4	368	7.9
3.....	"	8 x 12	3392	5.9	3 x 3.5	292	7.5
			Average	5.9		Average	7.5
4.....	1.0	10 x 12	5445	8.0	3 x 3	403	7.9
5.....	"	8 x 12	3762	8.8	3.5 x 3.5	378	9.6
6.....	"	7.5 x 12	3582	8.0	3 x 4	414	9.4
7.....	"	6 x 12	2582	7.7	3.5 x 3.5	392	10.0
8.....	"	7.5 x 12	3300	7.3	3 x 3	342	10.0
9.....	"	7 x 12	3441	9.1	3.5 x 3.5	360	10.9
10.....	"	6 x 12	2519	8.8	3 x 4	425	7.8
11.....	"	4.5 x 12	2168	8.7	3 x 3	318	12.9
12.....	"	8 x 12	3644	7.6	3.5 x 4	391	9.7
13.....	"	9 x 12	4065	8.4	3 x 3	300	11.3
14.....	"	7 x 12	3374	8.4	3 x 3.5	299	11.0
15.....	"	8 x 12	3463	8.1	3 x 3	315	10.8
16.....	"	10 x 12	4253	7.9	3.5 x 3.5	415	8.7
17.....	"	8.5 x 12	3671	7.7	3.5 x 3.5	463	6.9
18.....	"	8 x 12	3583	9.2	2.5 x 3.5	308	6.2
19.....	"	8 x 12	3859	7.4	3.5 x 4	383	10.4
20.....	"	6.5 x 12	2853	8.0	4 x 4	335	11.4
			Average	8.2		Average	8.5
21.....	0.5	5 x 12	1502	7.2	4 x 4	322	6.8
22.....	0.5	3.5 x 7	564	8.3	4 x 4	351	7.4
			Average	7.7		Average	7.1
23.....	0.5	5 x 12	1504	7.70	3.5 x 4	281	6.7
24.....	0.5	5 x 12	1684	6.89	3.5 x 3.5	337	6.23
			Average	7.3		Average	6.5
25.....	2.30	6.5 x 8	4034	7.75	4 x 4.5	1333	8.20
26.....	2.80	8 x 11	8612	7.75	4 x 4.5	1784	7.70
27....	2.70	7 x 11	8155	10.00	4 x 4.5	1744	9.20
			Average	8.5		Average	8.4

At present enough tests have not been made to fix a reliable value for the reduction factor, but sufficient data are at hand to warrant the assertion that this accelerated method of testing can be used when necessary. Different materials will require different factors, but these can be determined by the person having occasion to use this accelerated test.

Criticisms and suggestions relative to the ideas stated herein are invited.

THE EFFECT OF HIGH-PRESSURE STEAM ON THE CRUSHING STRENGTH OF PORTLAND-CEMENT MORTAR AND CONCRETE.

By R. J. Wig.

It is a well-known fact that the hardening of Portland cement is accelerated by exposure to heat after hydration, but the value of this acceleration and the possibilities of its application to commercial purposes have not been extensively investigated.

In the manufacture of cement products such as building blocks, architectural stone, drain and sewer tile, fence posts, etc., it is highly desirable to produce material of uniform and known quality. Under present methods of manufacture the product is not of uniform quality, for even under laboratory conditions of storage and curing a variation of 50 per cent. in strength between maximum and minimum values is not excessive for similar concrete mixed and molded under apparently the same conditions.

At the present time many manufacturing plants find it to be economical and highly desirable to cure their products by exposing them to heat in the form of steam at atmospheric pressure. By this means it is found that the concrete becomes sufficiently hard in the course of several days to permit handling and shipment. The heat accelerates the setting and also aids in the production of a more uniform quality of material.

The results reported in this paper are summarized from a series of tests made in 1907 and 1908, in the Structural Materials Testing Laboratories of the United States Geological Survey at St. Louis, Mo. The purpose of the investigation was to determine what acceleration in hardening could be obtained by using steam under pressure with corresponding higher temperatures.

Outline of Investigations.

There were two series of investigations:

1. To determine the effect of different steam pressures on the hardening of Portland-cement mortar.
2. To determine the effect of duration of steam exposure on the hardening of Portland-cement mortar.

All tests were made on one mortar mixture composed of 1 part by volume of Portland cement to 4 parts by volume of well-graded sand. Two percentages of water were used in mixing, corresponding to those employed in molding cement products. One has been termed a damp consistency, the mixture containing only sufficient water to permit molding a ball in the hands and holding between the thumb and finger. Water could not be drawn to the surface by troweling. The other, designated a "quaking" consistency, contained all the water it would hold and permit of making a ball in the hands and holding between the thumb and finger. Water could easily be drawn to the surface by the pressure of a trowel.

Unfortunately the second series of tests was not made until eight months after the first series. The same cement was used in both series. It was stored in galvanized iron cans during this period but became slightly lumpy. The large lumps were screened out by passing the cement through a $\frac{1}{4}$-in. mesh sieve and an endeavor was made to crush the small lumps, but it would appear from the results that the tests were affected somewhat by this difference in the quality of the cement due to its age and condition, and the results of the second series are not directly comparable with those of the first series. However, each series is complete in itself and gives the desired information.

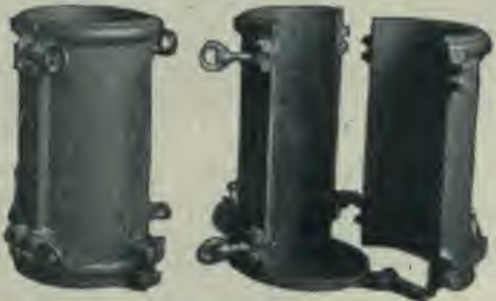


FIG. 1.—Cast-Iron Cylinder Mold.

DETAILS OF TESTS.

Although proportions are stated by volume, they were converted into their weight equivalents for greater accuracy, allowance being made for the moisture contained in the sand.

The materials were mixed in a cubical batch mixer. They were mixed dry for five minutes, when the water was weighed and added and the mixing continued another five minutes.

The test piece was cylindrical in shape, 8 ins. in diameter by 16 ins. in length. The cast-iron mold in which these test pieces were formed is shown in Fig. 1. It is in three sections held together by brass eye-screws and iron wedges. The inner surfaces are machined true.

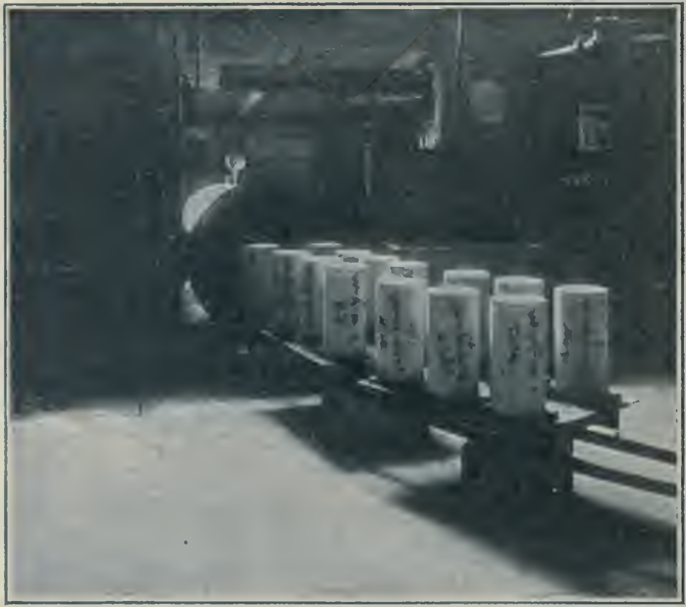


FIG. 2.—Steam-Curing Equipment.

The mold was filled one-quarter full and hand-tamped systematically with a 13-lb. peen-shaped tamper, care being taken to cover the entire surface. This operation was repeated until the mold was filled, when it was leveled off with a plasterer's trowel.

The molds were placed in the moist room immediately after filling and the following day they were removed from the test pieces. After steam treatment the test pieces were stored in the moist room (with a few exceptions) until tested. The test pieces

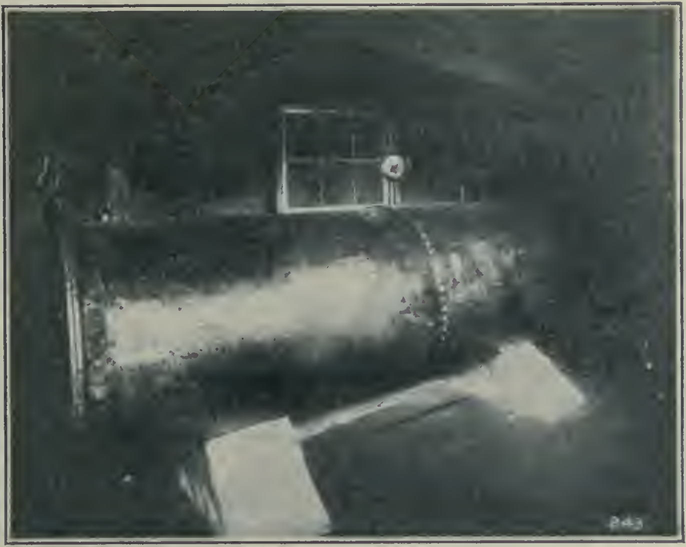


FIG. 3.—Steam-Curing Tank.

which were not steam treated were not removed from the moist room until the day they were due to be tested. All test pieces in the moist room were sprinkled every eight hours.

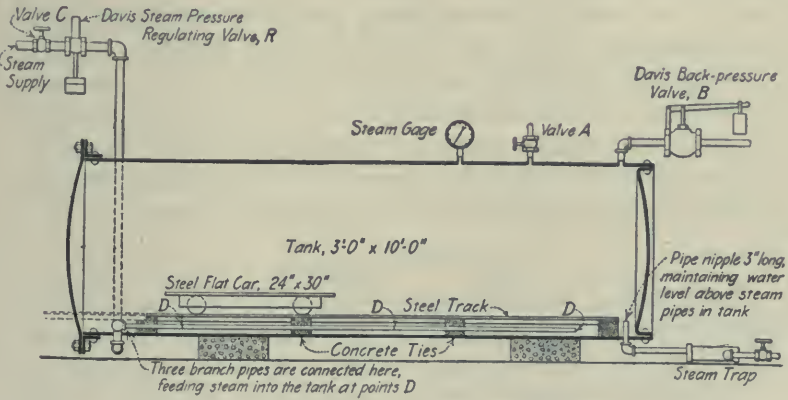

Valve C
Davis Steam Pressure Regulating Valve, R
Steam Supply
Davis Back-pressure Valve, B
Steam Gage
Valve A
Tank, 3'-0" x 10'-0"
Pipe nipple 3" long, maintaining water level above steam pipes in tank
Steel Flat Car, 24" x 30"
D
D
Steel Track
D
Concrete Ties
Three branch pipes are connected here, feeding steam into the tank at points D
Steam Trap

FIG. 4.—Steam-Curing Tank and Piping.

STEAM TREATMENT.

The steam-curing equipment is shown in Figs. 2 and 3. The details of the construction of the tank, piping, etc., are shown in Fig. 4. The equipment consisted of a curing tank 36 ins. in diameter by 10 ft. in length with one head removable, three steel flat, cars 24 ins. wide by 30 ins. long, track, steam-pressure regulating valve, back-pressure valve, steam trap, steam gage, and necessary piping. The steam was obtained from the power house of the laboratory.

The method of steam treatment was as follows:

Twenty-four hours after the test pieces were molded the molds were removed and the test pieces placed on the flat cars. The cars were rolled into the tank, the section of the track at the entrance of the tank was removed, and the head of the tank which was suspended on a rail was rolled into place and bolted. Valve *A* and the back-pressure valve *B* were opened to permit the air to exhaust with the entrance of the steam. Valve *C* was opened, permitting the steam to flow through the pressure-regulating valve *R* into the tank. It required from 15 to 45 minutes to exhaust the air from the tank and bring the steam up to the desired pressure. As soon as the air was exhausted valve *A* was closed and the back-pressure valve was set for the desired pressure. For low pressures a mercury column was attached to the tank for greater accuracy in reading. The high pressures were read directly from the steam gage. When the desired pressure was reached the regulating valve *R* was set. After the desired period of steaming, valve *C* was closed and valve *A*, back-pressure valve *B*, and the steam trap were opened, permitting the steam to exhaust from the tank. When the pressure was removed, the head of the tank was unbolted and removed. The cars were drawn from the tank, the test pieces removed, and either tested immediately for compressive strength or stored in the moist room until they were due to be tested.

COMPRESSION TESTS.

The ends of the cylindrical test pieces were smoothed off with plaster of Paris at right angles to the axis a short time before testing. The method of applying this plaster cap was as follows:

The glass-top table used for this purpose was oiled, plaster of Paris was spread on it about ¼ in. thick, and the cylinder was

TABLE I.—RESULTS OF STEAM CURING OF PORTLAND-CEMENT MORTAR, SERIES I.

1 Part Portland Cement to 4 Parts River Sand—"Quaking" Consistency (9 Per Cent. Water).

Test Piece No.	Steam Treatment.			Age when Tested, days.	Ultimate Strength, lbs. per sq. in.			Percentage Gain in Strength over Untreated.	Modulus of Elasticity, lbs. per sq. in.	Yield Point, lbs. per sq. in.	Weight per Cu. Ft. of Concrete, lbs.	Character of Failure.
	Gage Pressure, lbs. per sq. in.	Temperature, degrees Fahr.	Duration, hours.		Maximum.	Minimum.	Average.					
1	0	0	0	2	676	567	613	0	1,480,000	250	133.3	2—Bulge at center. 1—Diagonal shear.
4	Atmos.	212	48	3	1,267	1,195	1,267	107	....	...	.....	
7	2	218	24	2	1,870	1,743	1,808	195	1,900,000	800	130.5	3—Bulge at center.
10	10	239	24	2	1,934	1,649	1,786	191	1,460,000	700	130.4	3—Bulge at center.
13	20	258	24	2	2,235	2,070	2,139	249	2,000,000	1,100	132.1	2—Bulge at center. 1—Diagonal shear.
16	40	286	24	2	3,481	3,143	3,292	437	2,900,000	800	129.9	3—Bulge at center.
19	80	323	24	2	*3,966 +	3,959	*3,964 +	547 +	2,840,000	1,500	130.1	
22	80	323	24	2	4,840	4,185	4,487	632	....	....	129.8	
25	0	0	0	8	1,345	1,240	1,296	0	1,995,000	500	.....	3—Bulge at center.
28	2	218	24	7	1,889	1,671	1,792	38	2,040,000	800	132.4	3—Bulge at center.
31	10	239	24	7	1,622	1,508	1,555	20	1,450,000	1,000	131.5	3—Bulge at center.
34	20	258	24	7	2,351	2,241	2,284	76	2,100,000	1,100	134.3	3—Bulge at center.
37	40	286	24	7	3,583	3,109	3,381	161	3,660,000	700	133.8	
40	80	323	24	7	†3,966 +	†3,966 +	†3,966 +	†206 +	†3,500,000	†1,200	134.7	
43	80	323	24	7	4,529	(*a*) 3,684	4,187	223	....	....	132.9	2—Bulge at center. *a*—One corner sheared.
46	0	0	0	14	1,595	1,454	1,528	0	....	....	134.0	2—Bulge at center. 1—Diagonal shear.
49	2	218	24	14	1,923	1,739	1,805	18	2,160,000	1,300	133.2	1—Bulge at center. 1—Diagonal shear.
52	10	239	24	14	1,712	1,690	1,701	11	2,670,000	300	131.5	
55	20	258	24	14	2,806	2,668	2,740	...	....	.....	.....	
58	40	286	24	14	3,535	3,450	3,484	79	....	.....	134.0	
61‡	80	323	24	14	4,892	4,108	4,433	190	....	.....	.....	
64	0	0	0	28	1,822	1,636	1,727	0	....	.....	133.7	2—Bulge at center. 1—Diagonal shear.
67	0	0	0	90	2,488	2,125	2,286	.....	....	.....	.....	
70	0	0	0	180	2,591	2,156	2,342	.....	....	.....	.....	
73	2	218	24	113	3,493	3,269	3,364	.....	....	.....	134.4	
76	10	239	24	28	2,015	1,832	1,902	.....	....	.....	133.4	
79	20	258	24	110 297	 4,065	 3,815	3,180 3,940	.....	....	.....	132.0	2—Bulge at center.
81	40	286	24	84 105	4,189	3,813	4,001 4,765	.....	....	.....	132.6 133.0	
84	80	323	24	89	5,145	4,900	5,017	.....	....	.....	136.3	
87	80	323	24	77	5,307	4,754	5,054	.....	....	.....	135.4	
90	80	323	24	210	.....	.....	§5,341	.....	....	.....	.....	

NOTE.—Cylinders 8 ins. diameter by 16 ins. long; made in triplicate. Modulus of elasticity and yield point determined on one test piece of each set.

* Capacity of testing machine used at this time exceeded. At 15 days, they failed at 4,655 and 4,524; average, 4,379.

†Capacity of testing machine used at this time exceeded. At 15 days, they failed at 5,220, 4,660, and 4,640; average, 4,840.

‡ Stored in the air in the laboratory, not in the damp room. § Exposed to weather from November to June.

TABLE II.—RESULTS OF STEAM CURING OF PORTLAND-CEMENT MORTAR, SERIES 2.

1 Part Portland Cement to 4 Parts River Sand. Steam Pressure (gage), 80 lbs. per sq. in. (323° F.).

Test Piece No.	Consistency of Concrete.	Duration of Steam Treatment, hrs.	Age when Tested, days.	Ultimate Strength, lbs. per sq. in.			Percentage Gain in Strength over Untreated.	Modulus of Elasticity, lbs. per sq. in.	Yield Point, lbs. per sq. in.	Weight per Cu. Ft. of Concrete, lbs.	Character of Failure.
				Maximum.	Minimum.	Average.					
93	Quaking 9 per cent. Water	0	2	676	567	613	0	1,480,000	250	133.3	2—Bulge at center. 1—Diagonal shear.
96	"	3	2	731	675	701	14	585,000	400	123.1	3—Bulge at center.
99	"	12	2	3,247	2,864	3,073	401	....	...	129.4	3—Bulge at center.
102	"	17	2	3,152	3,105	3,135	411	2,090,000	2,000	129.3	3—Bulge at center.
105	"	24	2	3,288	3,180	3,236	428	1,870,000	2,300	129.5	3—Bulge at center.
108	"	0	8	1,345	1,240	1,296	0	1,995,000	500	133.8	3—Bulge at center.
111	"	3	7	729	717	725	(Loss)	705,000	450	129.4	3—Bulge at center.
114	"	12	7	3,273	2,890	3,119	141	....	...	133.2	3—Bulge at center.
117	"	17	7	3,610	3,100	3,367	160	2,250,000	1,000	131.0	3—Bulge at center.
120	"	24	7	3,612	3,271	3,421	164	2,220,000	1,900	130.2	2—Bulge at center. 1—Diagonal shear.
123	"	48	7	4,235	3,379	3,931	303	2,380,000	1,800	130.2	2—Bulge at center. ....
126	"	0	14	1,595	1,454	1,528	0	3,700,000	400	134.0	2—Bulge at center. 1—Diagonal shear.
129	"	3	14	858	847	852	(Loss)	980,000	200	129.3	3—Bulge at center.
132	"	12	14	3,194	2,938	3,084	102	3,260,000	450	134.8	2—Bulge at center. 1—Diagonal shear.
135	"	17	14	4,048	2,718	3,271	114	3,410,000	350	133.7	3—Bulge at center.
138	"	24	14	3,471	3,083	3,311	117	2,880,000	500	132.3	3—Bulge at center.
141	"	48	14	4,269	4,045	4,147	171	2,530,000	900	131.9	3—Bulge at center.
144	"	0	28	1,822	1,636	1,727	0	3,700,000	300	133.7	2—Bulge at center. 1—Diagonal shear.
147	"	3	28	1,140	1,071	1,108*	(Loss)	1,890,000	300	129.7	2—Bulge at center. 1—Diagonal shear.
150	"	12	28	3,341	2,947	3,180	84	3,570,000	500	133.0	2—Bulge at center. 1—Diagonal shear.
153	"	17	28	3,716	3,504	3,635	110	3,570,000	500	135.9	3—Bulge at center
156	"	24	28	4,212	3,307	3,812	121	2,900,000	900	133.9	3—Bulge at center.
159	"	48	28	4,332	4,026	4,136	139	2,670,000	1,500	132.8	3—Bulge at center.

TABLE II.—(*Continued.*)

Test Piece No.	Consistency of Concrete.	Duration of Steam Treatment, hrs.	Age when Tested, days.	Ultimate Strength, lbs. per sq. in.			Percentage Gain in Strength over Untreated.	Modulus of Elasticity, lbs. per sq. in.	Yield Point, lbs. per sq. in.	Weight per Cu. Ft. of Concrete, lbs.	Character of Failure.
				Maximum.	Minimum.	Average.					
162	Damp 5½ per cent Water	0	2	576	543	557	0	1,990,000	100	125.8	1—Bulge at center. 2—Diagonal shear.
165	"	3	2	513	496	507	(Loss)	525,000	300	115.2	3—Bulge at center.
168	"	6	2	579	478	545	"	585,000	400	117.6	
171	"	12	2	2,573	2,228	2,418	334	2,170,000	400	126.0	3—Bulge at center.
174	"	24	2	1,593	1,557	1,571	182	1,510,000	800	129.8	3—Bulge at center.
177	"	0	7	1,367	1,231	1,293	0	1,910,000	600	126.7	2—Diagonal shear. 1—. . . . .
180	"	3	7	586	483	542	(Loss)	615,000	350	121.2	2—Bulge at center. 1—Diagonal shear.
183	"	6	7	609	569	590	"	545,000	500	125.7	
186	"	12	7	2,377	2,252	2,329	80	1,870,000	900	131.0	1—Bulge at center. 2—Diagonal shear.
189	"	24	7	1,755	1,617	1,669	29	1,600,000	800	128.7	2—Bulge at center. 1—Diagonal shear.
192	"	48	7	1,876	1,690	1,768	36	1,470,000	700	128.9	3—Bulge at center.
195	"	0	14	1,625	1,311	1,483	0	2,540,000	500	127.8	
198	"	3	14	699	657	677	(Loss)	755,000	400	124.0	
201	"	6	14	614	552	583	"	570,000	400	125.9	
203	"	12	14	2,485	2,298	2,402	62	2,490,000	500	132.4	3—Bulge at center.
206	"	24	14	1,575	1,396	1,499	1	1,390,000	800	128.3	2—Bulge at center. 1—Diagonal shear.
209	"	48	14	1,932	1,584	1,813	22	1,710,000	600	128.1	2—Bulge at center. 1—. . . . .
212	"	0	28	2,350	1,830	2,032	0	2,720,000	600	130.1	
215	"	3	28	715	680	697	(Loss)	780,000	400	123.4	3—Bulge at center.
218	"	6	28	697	667	684	"	885,000	350	125.1	3—Bulge at center.
221	"	12	28	2,657	2,190	2,477	22	2,740,000	500	131.7	3—Bulge at center.
224	"	24	28	1,912	1,518	1,762	(Loss)	1,425,000	1,000	130.1	2—Bulge at center. 1—Diagonal shear.
227	"	48	28	1,952	1,672	1,850	"	1,770,000	800	127.9	2—Bulge at center. 1—Diagonal shear.

NOTE.—Cylinders 8 ins. diameter by 16 ins. long; made in triplicate. Modulus of elasticity and yield point determined on one test piece of each set.

forced into it so that all but a layer about $\frac{1}{16}$ in. thick was squeezed out. The sides of the cylinder were kept vertical by means of a spirit level. After the plaster was set, the test piece was removed from the glass and the other end was treated in the same way, both ends being made parallel, and perpendicular to the axis.

The test piece was placed in the testing machine without further bedding. The load was applied continuously at a speed of about $\frac{1}{40}$ in. per minute until failure. The gross deformation readings were taken on one test piece from each set of three similar test pieces, readings being taken at 5,000-lb. intervals or about 100 lbs. per sq. in. The ultimate breaking load and the appearance of the ruptured cylinder were recorded. The capacity of the largest testing machine available at the time some of the earlier tests were made was 200,000 lbs., which was not sufficient to cause the failure of the test pieces. A machine of 600,000 lbs. capacity was available later, however, in which all test pieces could be crushed to failure.

RESULTS OF TESTS.

All test pieces were calipered and weighed previous to testing. The weight per cubic foot of mortar was computed by dividing the weight by the volume computed from actual measurements of each test piece. The ultimate strength in pounds per square inch was obtained by dividing the total breaking load by the sectional area computed from the actual measured diameter.

Only averaged results are recorded in Tables I and II for all properties excepting the ultimate compressive strength, for which maximum and minimum values are also given. The results of the tests are shown diagrammatically in Figs. 5 to 9 inclusive, each point on the curve being the average value of three test pieces unless otherwise stated.

Figs. 5 and 6.—Examining these diagrams, in which pressure and temperature, respectively, have been plotted with the ultimate compressive strength, it is observed that the acceleration in strength is very marked with the increase in the steam pressure and temperature. The gain in strength of the mortar exposed for 24 hours to steam at 80 lbs. per sq. in. over the untreated mortar is 632 per cent. Lower steam pressures give a corresponding increase in strength over the untreated mortar, and from the slope of the

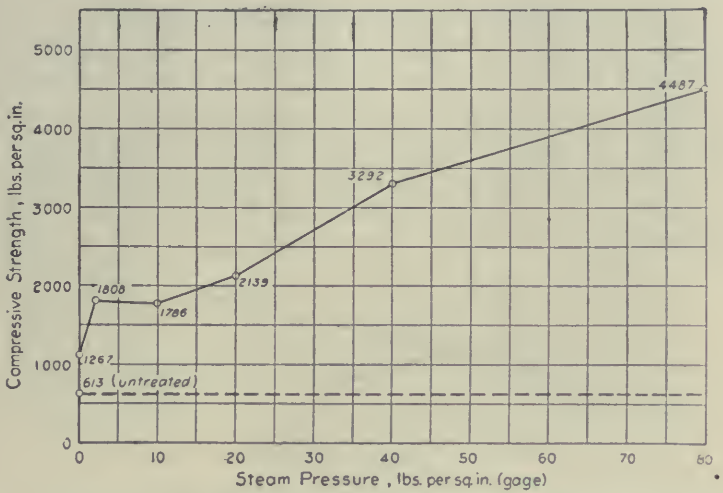


FIG. 5.—Effect of Various Steam Pressures on the Compressive Strength of Portland-Cement Mortar.

Age when Tested, 2 Days.
"Quaking" Consistency, 9 per cent. Water.
Subjected to Steam for 24 Hours after having set for 24 Hours in Moist Air.

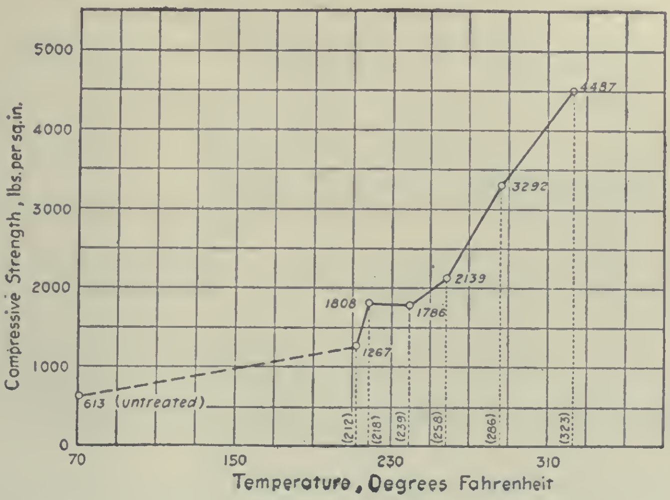


FIG. 6.—Effect of Steam of Various Temperatures on the Compressive Strength of Portland-Cement Mortar.

Age when Tested, 2 Days.
"Quaking" Consistency, 9 per cent. Water.
Subjected to Steam for 24 Hours after having set for 24 Hours in Moist Air.

curves it would appear that the increase in strength would be correspondingly higher for higher steam pressures and temperatures.

Fig. 7.—In this diagram the age of the mortar when tested is plotted with the ultimate compressive strength for various steam pressures. It will be observed that there is no retrogression in strength with age in any case. The untreated mortar does not equal in strength any of the steam-cured mortar at any period, and at the age of 180 days it has less than 50 per cent. of the strength of the steam-cured mortar under 80 lbs. steam pressure for 24 hours.

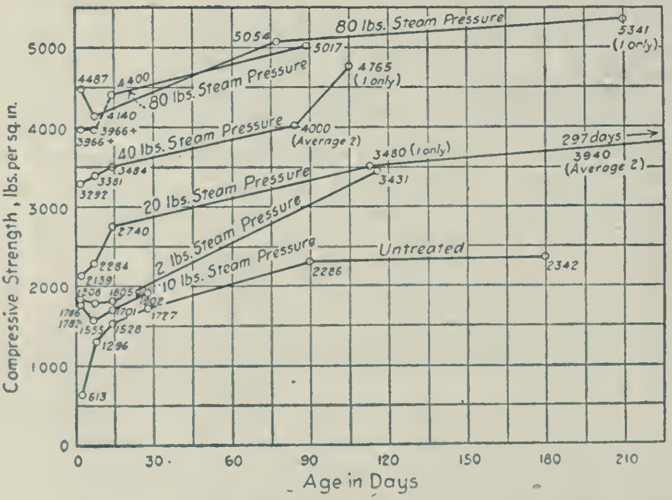


FIG. 7.—Effect of Various Steam Pressures on the Compressive Strength of Portland-Cement Mortar.

"Quaking" Consistency, 9 per cent. Water.
Subjected to Steam for 24 Hours after having set for 24 Hours in Moist Air.

Fig. 8.—This diagram shows how the length of time of exposure to steam affects the compressive strength, the steam pressure being maintained constant. All mortar exposed to steam under 80 lbs. pressure for from 12 to 48 hours resulted in an increase in compressive strength over the untreated mortar. The increase in strength is from approximately 400 per cent. at the age of 2 days to 100 per cent. at the age of 28 days. None of the steam-cured

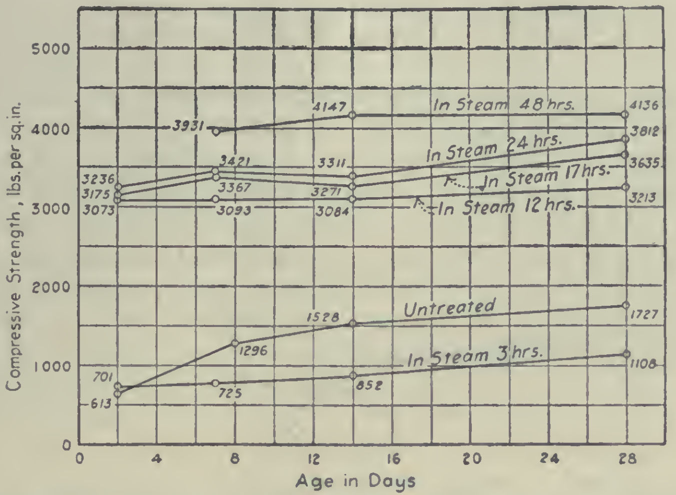


FIG. 8.—Effect of Duration of Exposure to Steam on the Compressive Strength of Concrete.

Steam Pressure between 75 and 80 lbs. per sq. in (Gage).
"Quaking" Consistency, 9 per cent. Water.
Concrete stored in Moist Air 24 Hours before Steaming.

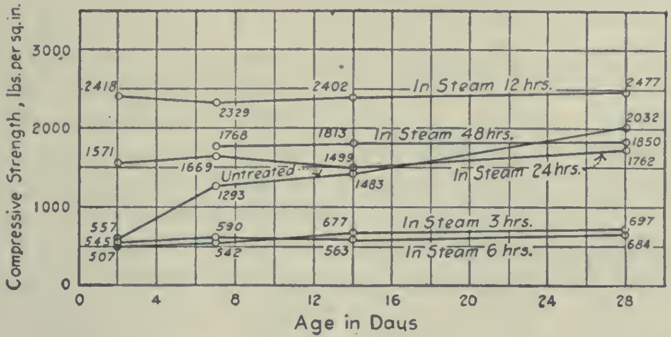


FIG. 9.—Effect of Duration of Exposure to Steam on the Compressive Strength of Concrete.

Steam Pressure between 75 and 80 lbs. per sq. in. (Gage).
"Damp" Consistency, 5½ per cent. Water.
Concrete Stored in Moist Air 24 Hours before Steaming.

mortar showed retrogression in strength between 2 and 28 days. The steam treatment of only 3 hours' duration apparently had an injurious effect upon the hardening of the mortar, as it developed only 60 per cent. of the strength of the untreated mortar at the age of 28 days. Comparing the 24-hour curve of Fig. 8 with the 24-hour curve of Fig. 7, it will be observed that the former lies below the latter for all periods, although, if the mortar were of the same quality in both cases, they should have developed equal strength. As previously explained, eight months elapsed between the making of the test pieces of these two series, and the cement had become slightly lumpy during this interval.

Fig. 9.—The concrete of this series was mixed very dry, only 6 per cent. of water being used. The untreated mortar of this series is approximately equal in strength to the untreated mortar of "quaking" consistency shown in Fig. 8, but the steam-cured mortar of this damp consistency has developed only about 50 per cent. of the strength of the steam-cured mortar of the "quaking" consistency shown in Fig. 8. There is no apparent explanation why the mortars subjected to 12 hours' steam treatment should exceed the strength of the mortars exposed to steam for 24 and 48 hours.

Considering all of the above tests, it is apparent that steam under pressure will greatly accelerate the hardening. In order to determine the effect of steam on a lean mixture, one set of cylinders was molded of 1 part Portland cement and 8 parts of sand, using 12½ per cent. of water. They were permitted to obtain an initial set in moist air for 24 hours when they were exposed to steam under 80 lbs. pressure for 24 hours. They were tested immediately after removing from the steam, being 2 days old, with the following ultimate strength in compression (lbs. per sq. in.): 2038; 2307; 2040; average, 2128.

From these tests it would appear that the strength of a steam-cured mortar is directly proportional to the cement content, but of course too much dependence should not be placed on a few results. One set of limestone concrete cylinders, made in the proportion of 1 part Portland cement, 3 parts sand and 6 parts limestone, was tested. They set in moist air for the first 24 hours, obtaining an initial set, after which they were exposed to steam at 80 lbs. pressure for 24 hours. They were tested directly after taking from the steam, being 2 days old, with the following ultimate strength in compression (lbs. per sq. in.): 2429; 2615; 2320; average, 2455.

Several sets of cylinders were exposed to steam immediately after molding without permitting the mortar or concrete to obtain an initial set. In every case the test pieces were cracked longitudinally by the steam, but the cement was hard and they developed about half of the strength of those which were permitted to obtain an initial set before exposing to steam. It was found that if the mortar or concrete was mixed to a creamy consistency, although it was permitted to set in the air for 4 days previous to exposing to steam, it would be cracked longitudinally by the action of the steam.

CONCLUSIONS.

The conclusions drawn on tests of this character must be understood as being only directly applicable to materials similar to those tested and they must be limited by the limitations of the investigation. However, the test pieces were of good size and considering the large number made and tested, the reliability and accuracy of the results may be accepted.

1. Steam up to 80 lbs. per sq. in. has an accelerating action on the hardening of Portland-cement mortars or concrete.

2. The compressive strength increases as the steam pressure is increased.

3. The compressive strength increases with the increase in the time of exposure to steam.

4. A compressive strength considerably (in some cases over 100 per cent.) in excess of that obtained normally after ageing for six months, may be obtained in two days by using steam under pressure for curing the mortar or concrete.

5. Steam under pressure, if of sufficient duration, permanently accelerates the hardening of the concrete, giving constant increase in compressive strength with age.

6. The steam-cured mortar or concrete is of much more uniform appearance and much lighter in color than normally aged mortar or concrete made from the same materials.

7. The mortar or concrete should obtain an initial set before it is exposed to the steam treatment.

8. For steam curing, a "quaking" or medium consistency is preferable to a very dry or very wet consistency.

9. The initial modulus of elasticity and the yield point of the mortar increase directly with the steam pressure.

10. The initial modulus of elasticity and the yield point of the mortar increase directly with the duration of steam treatment.

11. The initial modulus of elasticity does not increase in direct proportion to the increase in the ultimate compressive strength of a steam-cured mortar.

12. Results indicate that the compressive strength obtained by steam curing is directly proportional to the cement content of the mortar or concrete.

THE DETERMINATION OF STRESSES IN A REINFORCED CONCRETE MEMBER SUBJECT TO AXIAL LOAD AND FLEXURE.

By S. Ingberg.

Introduction.

The use of plain and reinforced concrete in arch bridges has necessitated the deduction of formulas for obtaining the stresses in steel and concrete when subjected to combined thrust and flexure. An experimental study of the behavior of reinforced concrete under these conditions had not, to the writer's knowledge, been made until the present series of tests was undertaken during the year 1909-10 as a part of the investigations of the Engineering Experiment Station of the University of Illinois. The work was under the immediate charge of Professor A. N. Talbot, to whom, as well as to other members of the staff, acknowledgments are due for valuable suggestions and aid.

Theory.

As an aid toward defining the experimental problem the following theoretical considerations will be given.

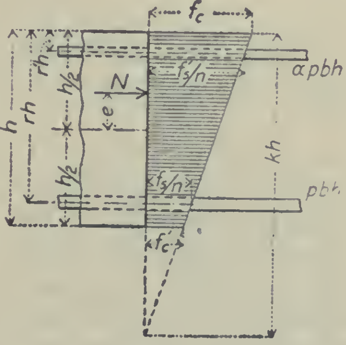


Fig. 1.

Fig. 1 represents a section of a reinforced concrete member in which the concrete is assumed to take stress over the full section, f'_c being compression or any allowable tension. Letting p represent the ratio of lower or tensile steel area to that of the concrete, α the ratio of compressive to tensile reinforcement, and n = modulus of elasticity of steel divided by the initial modulus of elasticity of concrete, we have from conditions of equilibrium,

$$\tfrac{1}{2}bh(f_c+f_c')+\alpha pbhf_s'+pbhf_s-N=0.$$

Also, taking moments about N,

$$\tfrac{1}{2}bh(f_c+f_c')\left[e-\frac{h(f_c-f_c')}{6(f_c+f_c')}\right]-apbhf_s'\left(\frac{h}{2}-e-r'h\right)$$
$$+pbhf_s\left(rh-\frac{h}{2}+e\right)=0.$$

The eccentric distance, e, from the geometric center of the section may be an actual eccentricity of the normal force N or it may be a virtual eccentricity due to a moment M produced by forces acting in lines parallel with the section, or

$$e=\frac{M}{N}.$$

Substituting in the above equations for f_c', f_s', and f_s in terms of f_c, considering stresses to be proportional to their distances from the plane of zero stress, distant kh from the upper fiber, the following equations result for what will be termed Case I:

$$k=\frac{1+6\dfrac{e}{h}-6pn\left[ar'\left(1-\dfrac{2e}{h}-2r'\right)-r\left(\dfrac{2e}{h}+1-2r'\right)\right]}{12\dfrac{e}{h}-6pn\left[a\left(1-\dfrac{2e}{h}-2r'\right)-\left(\dfrac{2e}{h}+1-2r'\right)\right]}$$

$$\frac{N}{bhf_c}=1-\frac{1}{2k}+\frac{pn}{k}\left[a(k-r')+(k-r)\right].$$

This affords a solution for the compressive unit stress f_c, hence also for the other stresses.

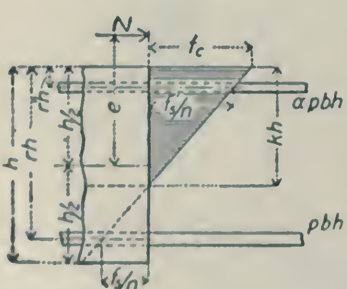


FIG. 2.

Referring to Fig. 2, Case II considers the concrete on one side of the neutral plane to be broken in tension, both deformations and stresses being as before assumed proportional to their distances from the neutral axis.

In a manner similar to that used for Case I, the following solution is obtained:

$$k^3+k^2\left(\frac{3e}{h}-\frac{3}{2}\right)+3apnk\left(\frac{2e}{h}-1+2r'\right)+3pnk\left(\frac{2e}{h}+2r-1\right)$$

$$-3apnr'\left(\frac{2e}{h}+2r'-1\right)-3pnr\left(\frac{2e}{h}+2r-1\right)=0.$$

$$\frac{N}{bhf_c}=\frac{k}{2}+apn\left(1-\frac{r'}{k}\right)+pn\left(1-\frac{r}{k}\right)$$

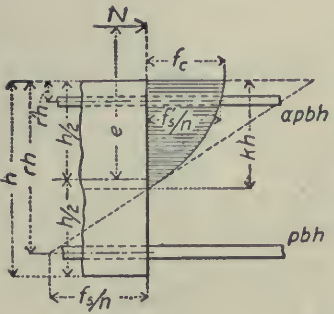


FIG. 3.

Referring to Fig. 3, Case III, while considering the section to remain plane after bending, assumes deformation and stress in the concrete to be related by the law of the parabola whose vertex is at the outer fiber. This assumption gives the modulus of elasticity of the concrete in the outer fiber a value of one-half of that which obtains near the neutral axis. Then, as in Case II, neglecting tension in the concrete, the following solution results:

$$k^3+k^2\left(\frac{8e}{3h}-\frac{4}{3}\right)+4apnk\left(\frac{2e}{h}-1+2r'\right)+4pnk\left(\frac{2e}{h}+2r-1\right)$$

$$-4apnr'\left(\frac{2e}{h}+2r'-1\right)-4pnr\left(\frac{2e}{h}+2r-1\right)=0.$$

$$\frac{N}{bhf_c}=\frac{2k}{3}+2apn\left(1-\frac{r'}{k}\right)+2pn\left(1-\frac{r}{k}\right).$$

TEST PIECES.

Twenty-two test beams 8 by 11 ins. in section were made and tested. Table I gives their dimensions and reinforcements. Beams 450 were plain concrete, Beams 451 had two ½-in. rods tensile reinforcement, Beams 452 had two ½-in. rods tensile reinforcement and four ½-in. rods compressive reinforcement, Beams 453 had two ¾-in. rods tensile reinforcement, and Beams 454 had two ¾-in. rods tensile reinforcement and two ¾-in. rods compressive reinforcement. To prevent a possible failure

TABLE I.—DATA OF BEAMS.

ALL BEAMS 12′ 6″ LONG. CONCRETE, 1 : 2 : 4.

Beam No.	Section of Concrete, ins.	Per cent. of Cement Used.	Age, Days.	Per cent. Steel.		Proportionate Distance to Steel.		Web Reinforcement.	
				Top.	Bot'm.	Top.	Bot'm.	Per Cent	Disposition.
450 . 1	8.187 x 11	12.2	67	Plain		.....	.....	.....	....
450 . 3	8.125 x 11.125	15.8	70	Concrete.		.....	.....	.....	....
450 . 4	8.06 x 11.31	15.0	69	"		.....	.....	.....	....
450 . 5	8.12 x 11.125	14.6	70	"		.....	.....	.....	....
451 . 1	8.25 x 11.312	13.2	63	0	0.400	.....	0.882	None	....
451 . 2	8 x 11.062	15.4	70	0	0.447	.....	0.888	"	
451 . 3	8 x 11	13.2	65	0	0.450	.....	0.900	0.482	10—⅜″ in each end, spaced 5″
451 . 4	8 x 11.5	15.0	69	0	0.443	.....	0.903	0.465	"
451 . 5	8 x 11	14.8	69	0	0.438	.....	0.900	None	... .
452 . 1	8.12 x 11.37	13.1	63	0.856	0.428	0.132	0.812	"	....
452 . 2	8.06 x 11.37	15.3	70	0.382	0.441	0.129	0.900	"	....
452 . 3	8 x 11.12	15.1	69	0.882	0.441	0.173	0.895	0.465	10—⅜″ in each end, spaced 5″
452 . 4	8 x 11.375	14.9	70	0.862	0.431	0.151	0.890	0.470	"
452 . 5	8.06 x 11.5	15.1	69	0.846	0.423	0.125	0.860	None	....
453 . 1	8.25 x 11.25	14.9	70	0	0.950	.....	0.890	0.460	10—⅜″ in each end, spaced 5″
453 . 2	8.12 x 11.37	14.9	71	0	0.952	.....	0.880	0.465	"
453 . 3	8 x 11.25	14.6	71	0	0.982	.....	0.890	0.922	8—½″ in each end, spaced 6½″
453 . 4	8 x 11.25	14.5	71	0	0.975	.....	0.872	0.570	12—⅜″ in each end, spaced 4″
454 . 1	8 x 11.25	15.0	70	0.975	0.975	0.105	0.885	0.570	"
454 . 2	7.75 x 11.12	14.7	71	1.025	1.025	0.125	0.900	0.595	"
454 . 3	8.12 x 11.42	14.9	70	0.945	0.945	0.125	0.880	0.550	"
454 . 4	7.87 x 11	15.0	72	1.022	1.022	0.119	0.895	0.590	"

by diagonal tension, web reinforcement sufficient in amount to take two-thirds of the shear was placed in all beams in which the shear at the calculated maximum load exceeded 50 lbs. per sq. in. To check the quality of the concrete three 6-in. cubes and one control beam 6 by 8 ins. by 3 ft. 4 ins. were made from the same batch as the corresponding beam, and also, for the greater number of beams, one 8 by 16-in. cylinder was made.

The beams and cylinders were stored in air, the cylinders being kept a little more damp than the beams so that the rate of drying of the two might be approximately the same considering the difference in size. The cubes were stored in damp sand.

TESTING APPARATUS AND TESTING.

The beams were placed on supports forming a span of 12 ft. on the weighing table of a 200,000-lb. Olsen testing machine, the vertical loads being applied at the one-third points. At the same time a thrust was applied horizontally at the ends of the beam by a hydraulic jack working against end blocks tied together by two rods which extended along the beam. The arrangement of the apparatus is shown in Fig. 4. The 2-in. rods passed through holes in the cast-iron blocks *A*, and were secured by nuts on their ends. Over the supports and under the load points the rods were passed through slots in the webs of 5-in. I-beams. Between the flange and the beam was placed a piece of rubber belting, and the whole held in place by means of bolts passing through straps bearing against the opposite face. The hydraulic jack was bolted in a horizontal position to the one block *A*, and an auxiliary block *H* to the other. The whole thrust system, blocks, rods and hydraulic jack, was suspended from the ceiling by means of a hoist at each end and kept free from the rest of the apparatus, except for the bearing at strips *G* through which the thrust was transmitted to the beam. Since the deflection of the weighing table produced by the application of vertical load would increase the overhead reactions and thus interfere with the determination of the applied load, a spring dynamometer was placed on each end as shown, and its reading kept constant by adjusting the hoist.

The bearing blocks *H* were bedded in plaster of Paris and on them and central with the height of the beam were placed the ½ by ½-in. bearing strips *G*. For registering the smaller horizontal loads a hydraulic gage reading up to 500 lbs. per sq. in. was used, and for the larger loads a gage reading total load in tons. Deformations were measured on a 40-in. gage length, using roller extensometers. Deflections were read with reference to a thread fixed at the mid-depth of the beam at the supports, and passing over a mirror scale at the center.

To counterbalance the dead load moment of the beam at the beginning of the test, to produce zero moment in the gage length, an upward force was applied at the one-third points by clamping the stirrups *F* to the loading I-beam and running the pulling head up, the loading I-beam being secured to the latter by means of bolts

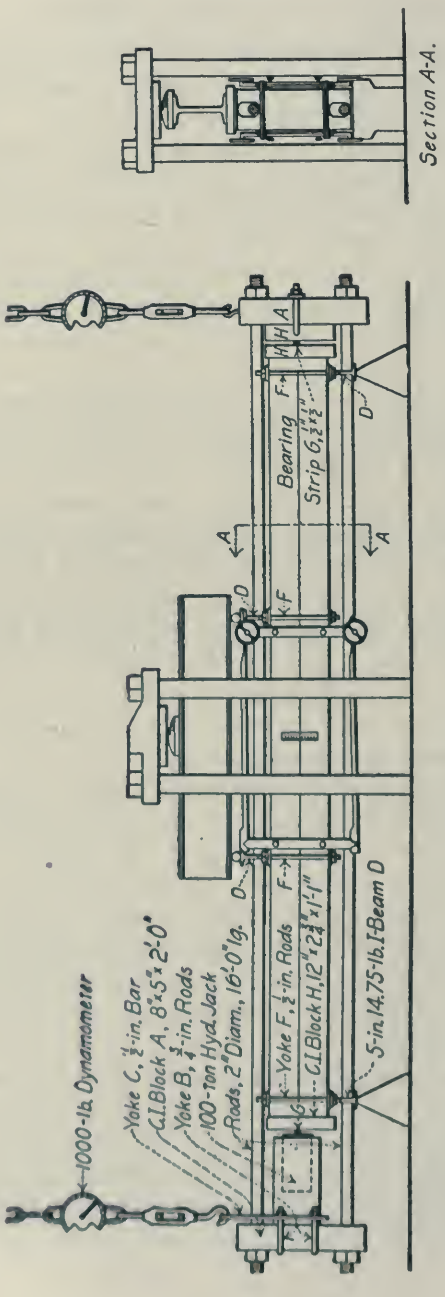


FIG. 4.—Diagram of Testing Apparatus.

passing through its flanges. The amount of uplift required is a function of the weight of beam, the span and overhang, the gage length and minor moments produced by the loading and measuring apparatus. It is determined by expressing the moment in terms of these quantities, integrating it over one-half of the gage length and equating the integral to zero. For the case in hand the required uplift was found to be seven-tenths of the weight of the beam, leaving three-tenths of its weight on the weighing table. From this point readings were taken for load and deformations at each 1,000-lb. increment of vertical load, the end thrust being kept constant during the whole or designated portion of the test.

Two specimens of each type of reinforcement were tested under thrusts that were increased by successive stages to produce 300, 600, 800, and sometimes 900 lbs. per sq. in. of section, the object being to produce failure in compression. The two remaining specimens of each type were tested under a constant end load of approximately 100 lbs. per sq. in., and failed in tension.

DATA AND RESULTS.

Auxiliary Tests.—The average compressive strength of the 6-in. cubes was 2,330 lbs. per sq. in., that of the 8-in. cylinders was 1,740 lbs. per sq. in., and the average modulus of rupture of the control beams was 345 lbs. per sq. in. The average yield point of the steel reinforcement in Beams 451 and 452 was 35,800 lbs. per sq. in., that of Beams 453 and 454, 41,500 lbs. per sq. in., the modulus of elasticity being 29,000,000 lbs. per sq. in.

The modulus of elasticity of the concrete was obtained by taking deformations over a 10-in. gage length in the cylinder tests. The lower curve of Fig. 5 is the average plot of ten tests. As seen, the initial modulus has a value of about 2,270,000 lbs. per sq. in. and the vertex of the parabola in the stress-deformation curve is located at unit deformation 0.0016, where the modulus of elasticity has a value of one-half of the initial modulus. The compression fiber of the beams deformed more than the concrete in the cylinders, the average for six beams for which readings were obtained near failure in compression being 0.0026. The upper curve of Fig. 5 was constructed by modifying and producing the lower so as to locate the vertex of the parabola at unit deformation

0.0026, the initial modulus remaining the same. The change in ordinates was made by swinging it through an arc with reference to its point of zero deformation. This modified curve was used in reducing unit deformations in the concrete of the beams to the corresponding fiber stresses.

Reduction of Experimental Data.—The position of the neutral axis, the unit deformations and stresses, and center deflections, were calculated and the results plotted against the equivalent one-third-point load. The latter is obtained by adding to the applied one-third-point load, (1) the weight of the beam multiplied by the ratio, average moment coefficient for uniform load (in the 40-in. gage length) to that for one-third point loading, or 0.122/0.167, and (2), the one-third-point load that would produce the same moment as that caused by the end thrust acting with a lever arm equal to the average deflection in the gage length.

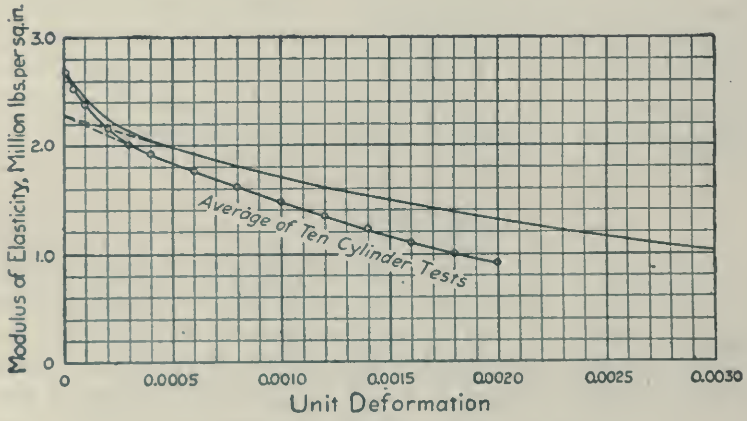


FIG. 5.—Modulus of Elasticity of Concrete.

Comparative Theoretical Values.—With the aid of diagrams worked out for each type of reinforcement, comparative theoretical values were calculated and plotted. Tables II and III give summaries of loads and stresses for a point at or near the maximum load. Figs. 6, 7 and 8 show the plotted values for three tests that fairly represent the agreement that obtained between experiment and theory.

TABLE II.—RESULTS OF TESTS ON PLAIN CONCRETE BEAMS.

Beam No.	Equivalent, One-third Point Load, lbs.		Thrust, lbs. per sq. in.	Compression in Concrete, lbs. per sq. in.		Tension in Concrete, lbs. per sq. in.		Manner of Failure.
	Maximum.	Load Considered.		Experimental.	Theoretical, Case I.	Experimental.	Theoretical, Case I.	
450 . 1	2,316	2,316	101	357	434	230*	234	Failure on taking off end load.
450 . 3	3,177	2,477	102	348	455	307*	248	Vertical load dropped 500 lbs. on cracking in tension.
450 . 4	10,980	8,135	767	1,952	1,890	762*	360	Beam took 1,500 lbs. additional vertical load after cracking in tension.
450 . 5	5,437	4,554	306	820	940	402*	339	Beam took 540 lbs. additional vertical load after cracking in tension.

*Modulus of Elasticity assumed equal in tension and compression.

TABLE III.—RESULTS OF TESTS ON REINFORCED CONCRETE BEAMS.

Beam No.	Equivalent One-third Point Load, lbs.		Thrust, lbs. per sq. in.	Compression in Concrete, lbs. per sq. in.			Tension in Steel, lbs. per sq. in.			Compression in Steel, lbs. sq. in.			Manner of Failure
	Maximum.	Load Considered.		Experimental.	Theoretical.		Experimental.	Theoretical.		Experimental.	Theoretical.		
					Case II.	Case III.		Case II.	Case III.		Case II.	Case III.	
451 . 1	9,418	4,537	100	600	1,020	605*	6,300	19,100	3,690*	......	......	......	Tension in Steel
451 . 2	8,510	7,967	100	1,550	1,910	1,260	36,100	44,800	46,000	......	......	......	,,
451 . 4	14,270	13,473	574	2,340	3,040	2,100	34,300	34,000	40,100	......	......	......	,,
451 . 3	12,850	11,545	600	2,530	2,870	1,992	35,000	28,000	33,100	......	......	......	†Compression
451 . 5	17,120	11,080	800	2,020	2,720	2,080	18,600	14,750	21,200	......	......	......	,,
452 . 2	12,668	10,692	305	1,572	1,900	1,190	33,600	37,200	39,700	17,350	15,500	19,700	Tension in Steel
452 . 5	12,094	10,694	285	1,465	1,870	1,170	38,400	39,000	40,800	14,620	14,900	19,700	,,
452 . 1	18,450	4,382	101	495	790	590*	4,680	18,100	4,960*	4,730	*6,060	4,740	‡Compression
452 . 3	17,660	15,960	882	2,320	2,910	1,940	30,500	26,400	31,800	33,500	27,300	35,000	,,
452 . 4	18,427	16,717	775	2,110	2,940	1,945	32,500	33,500	38,800	30,500	27,600	37,400	§Tension
453 . 1	15,668	14,449	94	1,742	2,530	1,745	38,100	41,200	44,600	......	......	......	,,
453 . 2	15,232	14,536	96.5	1,910	2,530	1,745	37,700	39,800	43,600	......	......	......	,,
453 . 3	18,680	17,590	767	2,980	3,570	2,670	35,300	26,200	31,000	......	......	......	Compression
453 . 4	20,095	18,715	750	2,940	3,670	2,680	35,200	26,900	31,200	......	......	......	,,
454 . 1	16,565	15,530	100	1,685	2,150	1,320	42,300	44,200	46.500	20,000	18,400	23,600	Tension
454 . 4	16,298	15,542	103	1,805	2,220	1,365	41,900	45,700	47,300	21,600	18,100	23,500	,,
454 . 2	24,127	22,807	918	2,810	3,580	2,340	36,700	36,400	40,200	Past Y.P	33,900	44,800	Compression
454 . 3	24,150	23,247	752	2,530	3,390	2,215	40,800	36,600	40,750	39,600	30,900	41,200	§Tension

*Case I.

†Thrust at failure = 900 lbs. per sq. in.

‡Thrust at failure = 805 lbs. per sq. in.

§Followed by compression failure.

Manner of Failure.—The plain beams failed in tension by cracking up to the neutral axis at a load that produced a theoretical tensile stress in the lower fiber a little higher than that obtained from the control beams. The unit elongation of the tension fiber at failure was indicated to be about 0.0002. The ratio of eccentricity, M/Nh, at failure in tension was 0.25, 0.365 and 0.74 for thrusts of 770, 306 and 102 lbs. per sq. in., respectively.

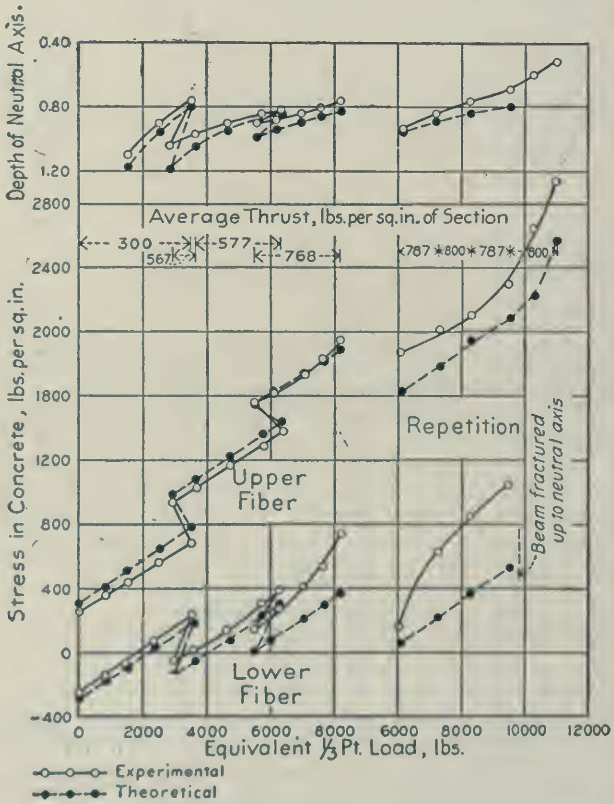


FIG. 6.—Beam 450.4; Plain Concrete.

As noted in Table II, the beams under high thrust took an increment of vertical load after failing in tension. The actual moment on the section was, however, less than before, since the

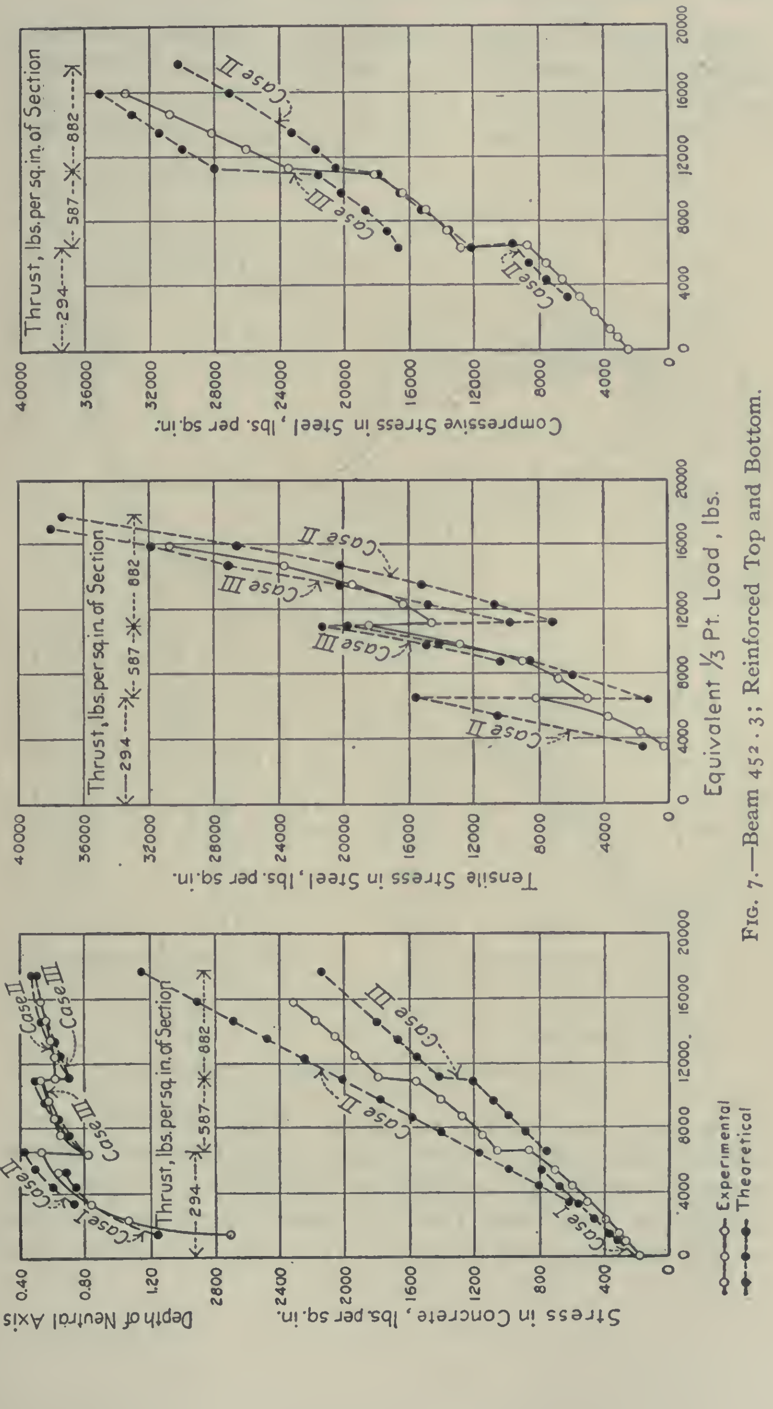


FIG. 7.—Beam 452 . 3; Reinforced Top and Bottom.

thrust produced a negative moment with reference to the intact section, which counteracted the moment produced by the vertical load.

The reinforced beams behaved in a similar manner at maximum load when failing in tension. They continued taking a vertical load, equal or nearly equal to the maximum, until the deflection became so large that the test could not be further continued. In some cases there are indications that an increment of load was taken after the steel had reached the yield point. The

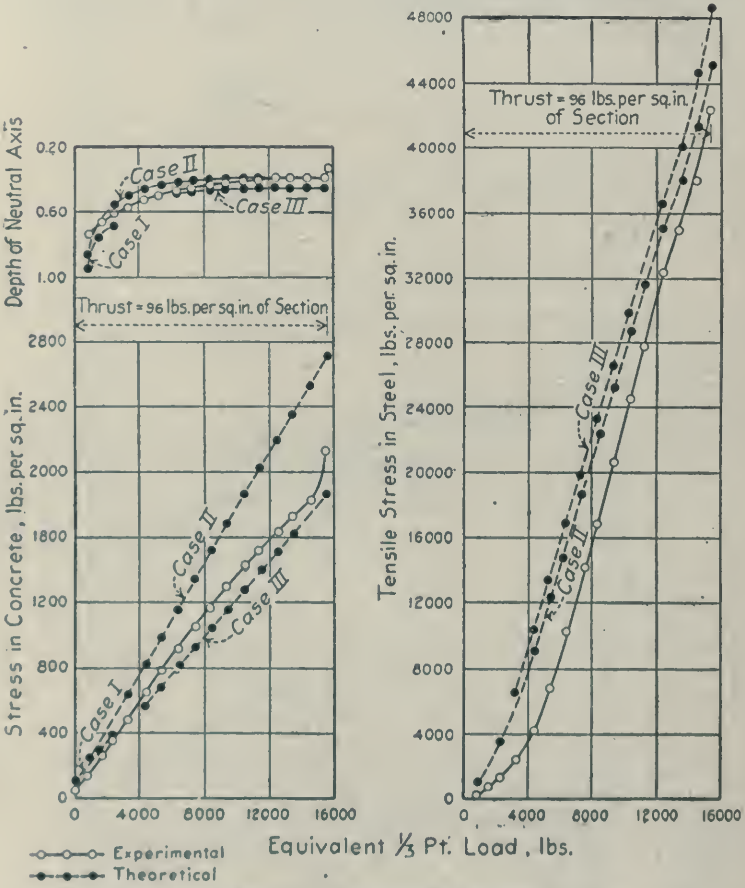


FIG. 8.—Beam 453.1; Reinforced Bottom Only.

compression failures were more sudden and in a few cases produced complete fracture. For the beams with compressive reinforcement the fracture was not complete, but a large drop in the load was obtained.

TABLE IV.—COMPARISON OF EXPERIMENT WITH THEORY.

GROUP I.—ONE-HALF PER CENT. REINFORCEMENT, TENSION FAILURES.

	Tension in Steel, lbs. per sq. in.			
Beam No.	Experiment.	Case II.	Diff.	Per cent.
451 · 2	36,100	44,800	−8,700	−24.1
451 · 4	34,300	34,000	+ 300	+ 0.9
452 · 2	33,600	37,200	−3,600	−10.7
452 · 5	38,400	39,000	− 600	− 1.6
			Average	9.3

GROUP II.—ONE-HALF PER CENT. REINFORCEMENT, COMPRESSION FAILURES.

	Tension in Steel, lbs. per sq. in.			
Beam No.	Experiment.	Case III.	Diff.	Per cent.
451 · 3	35,000	33,100	+1,900	+ 5.42
451 · 5	18,600	21,200	−2,600	−14.00
452 · 3	30,500	31,800	+1,300	+ 4.26
			Average	6.56

GROUP III.—ONE PER CENT. REINFORCEMENT, TENSION FAILURES.

	Tension in Steel, lbs. per sq. in.			
Beam No.	Experiment.	Case II.	Diff.	Per cent.
453 · 1	38,100	41,200	−2,900	−7.62
453 · 2	37,700	39,800	−2,100	−5.56
554 · 1	42,300	44,200	−1,900	−4.50
454 · 4	41,900	45,700	−3,800	−9.05
			Average	6.68

GROUP IV.—ONE PER CENT. REINFORCEMENT, COMPRESSION FAILURES.

	Tension in Steel, lbs. per sq. in.			
Beam No.	Experiment.	Case III.	Diff.	Per cent.
453 · 3	35,300	31,000	+4,300	+12.2
453 · 4	35,200	31,200	+4,000	+11.35
454 · 2	36,700	40,200	−3,500	− 9.95
454 · 3	40,800	40,750	+ 50	+ 0.01
			Average	8.00

Diagonal Tension.—No failure was due to diagonal tension. The highest shear was obtained with Beam 454 . 3 with the thrust at 760 lbs. per sq. in. and the shear at 120 lbs. per sq. in. of section. The neutral axis was 0.39 h from the top and the tension in the steel was 42,000 lbs. per sq. in. The highest shear with low thrust, obtained with beam 454 . 1, was 93 lbs. per sq. in. of section, thrust 100 lbs. per sq. in. Two diagonal cracks four inches long were formed at seven-eighths of the maximum load, but these did not open up farther until failure in tension occurred. These beams had $\frac{3}{8}$-in. vertical U-stirrups spaced 4 ins. center to center.

Comparisons.—Table IV gives a comparison of experiment with theory obtained from the values in Table III. Beams 451 . 1 and 451 . 2 are excluded because they were subjected to repetitive loading.

Conclusions.—Considering the errors involved in the determination of load, deformation, and modulus of elasticity, as well as those due to assumptions with reference to the distribution of stresses across the section and over the gage length, the above seems a fair agreement between experiment and theory, and justifies the conclusion that the formulas for flexure and direct stress as they apply to concrete and reinforced concrete will give values for the stresses in the materials well within the limit of accuracy required in design.

THE VALUE OF THE SULPHURIC ACID CORROSION TEST.

By C. M. Chapman.

There is no doubt of the great demand that exists for accelerated tests of materials of construction and preservation. This demand is not limited to any one branch of industry or class of materials; it is an urgent need that is heard of on every side. Both producers and consumers are greatly interested in any method that will enable them to determine in a comparatively short time what the effect of natural wear and tear will be upon the material in question, or, in other words, what "service" may be expected of it. Many tests have been devised and are in every-day use, apparently giving satisfactory results. The older materials of construction, such as stone, brick, mortar, wood, etc., have been in general use so many generations that their qualities have become well known and opinions as to the service that may be expected of them have become settled. Not so with the recently developed and the now-being-developed products. Users are skeptical of the service the new product will render. This skepticism is often fostered by the extravagant claims of the makers of the new products. A service test is the only certain method of demonstrating the true wearing qualities of the product, but a service test may require years to give the desired information. The result is that an accelerated test is devised which seems feasible and according to some theory or other seems to subject the product to such intensified conditions that in a short time it receives as much damaging action as it would receive during a long period in service. There can be no doubt as to the value of such tests, provided they are not misleading. It is highly desirable to have such tests for a great many products, but it is much to be regretted that so many of the accelerated tests now in use give results which bear little or no relation to the results obtained in service.

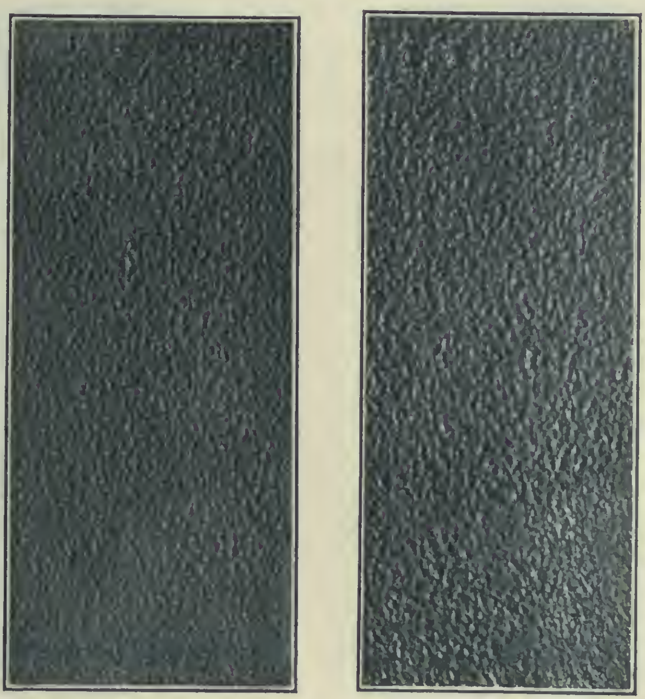
In some cases materials are tested for a quality which in service they are not called upon to develop. The list of false or misleading tests which might be cited in this connection is a long one.

Probably the most notable case of testing a material for a quality which is not required in service is that of subjecting cement to a tensile test.

Of the many examples of misleading tests there is one which, more than others, has come to the notice of Westinghouse, Church, Kerr and Company. Users of sheet iron and steel have been persuaded to believe that a metal which will best stand the sulphuric acid test as outlined in the Proceedings of the Society, Vol. VII, 1907, p. 231, is the one which will last longest under the usual conditions of weather exposure. With this belief in their minds they have purchased at greatly advanced prices metal which, while it would stand the sulphuric acid test admirably, was possibly no better suited to withstand the corrosive action of the particular exposure for which it was purchased than other metals purchasable at much lower prices.

This sulphuric acid test may be briefly described as follows: Samples of the metals to be tested are prepared by polishing their surfaces with fine emery, rubbing in a direction at right angles to the direction in which the metal was rolled. The size of the test piece recommended is $\frac{1}{16}$ by $\frac{1}{2}$ by 2 ins. After careful weighing, these small plates are immersed in a 20-per cent. solution of chemically pure sulphuric acid for one hour at a temperature of 15° C. They are then removed from the acid, washed, dried, and again weighed. The loss in weight is taken as an indication of the ability of the metal to withstand corrosive influences.

To demonstrate the differences between two methods of testing, namely, the weather exposure test and the sulphuric acid test, there are shown in Fig. 1 photographs of two plates, one of improved iron and one of common steel, showing results of weather exposure for eighteen months. The improved iron contained only minute quantities of impurities; the steel analyzed about 0.06 per cent. carbon, 0.095 per cent. phosphorus, and 0.38 per cent. manganese. These plates were clean and new when exposed side by side on the roof of the Maritime Building at the southern extremity of Manhattan. They seemed to be rather free from mill scale, but any mill scale present was left on, as it usually would be in practice. They were exposed at an angle of 45 degrees, facing south. This exposure is a rather severe one because of the fogs, smoke, and the salt air from the bay, all of which are present in this locality. At the end of eighteen months they were removed and their surfaces briskly brushed with a wire brush until the rust was removed and the surface of the metal brought to a polish. A careful examination of these plates fails to show any considerable difference in the corrosion and pitting which has taken place. As far as the eye can distinguish, each plate has rusted to the same extent. The depth and size of the pits formed by rust on the surfaces of these two plates are very nearly the same, but careful comparison shows slightly larger and deeper marks on the surface of the improved iron than on the surface of the common steel.

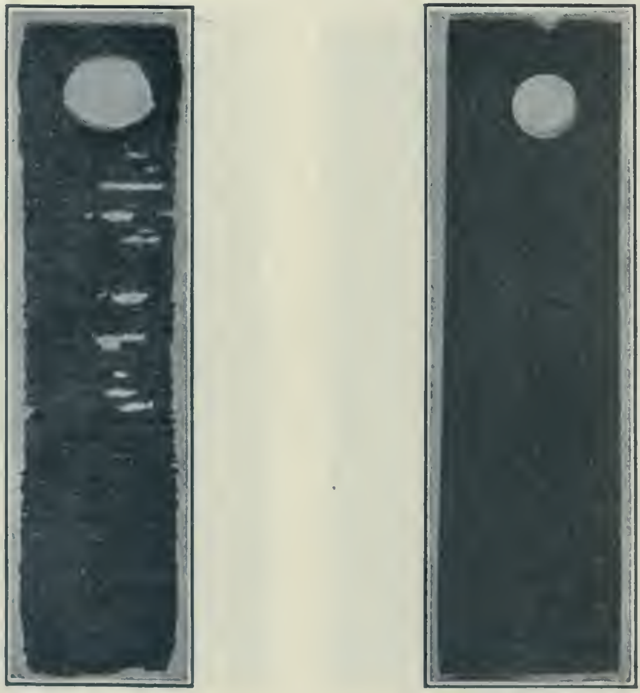


(*a*) Common Steel. (*b*) Improved Iron.

FIG. 1.—Showing Results of Weather Exposure for Eighteen Months.

Having thus compared the ability of the two metals to withstand the action of the weather, a sulphuric acid test was made on parts of the same plates which had been exposed to the weather.

Strips cut from the ends of these plates were prepared as above described, brought to about the same weight, and immersed in dilute sulphuric acid until one of them was nearly destroyed, while the other had lost only 7 per cent. in weight. At the end of the one-hour period prescribed by the test, the improved iron had lost 0.83 per cent. while the common steel had lost 6.7 per cent.



(*a*) Common Steel. (*b*) Improved Iron.

FIG. 2.—Showing Results of Acid Test for Twelve Hours.

Fig. 2 shows the two specimens as they appeared after being subjected to the acid test for twelve hours.

It is quite unnecessary to point out that although weather exposure caused approximately the same amount of rusting and pitting on the two plates, yet under the acid test one was attacked several times as violently as the other.

The makers of this improved iron claim that it is much more resistant to the corroding influences usually met with in service than are any of the ordinary grades of iron or steel, and they back up their claims by citing the sulphuric acid test as an indicator of the relative resistance of their metal to corrosion under service conditions.

We do not wish to be understood as claiming that the sulphuric acid test is useless or that when properly used it may not give useful information, but we do wish to call attention to its utter inability to give a reliable indication of the relative ability of metals to withstand the corrosive action of exposure to the weather.

DISCUSSION.

Mr. Cushman. Mr. A. S. Cushman (*by letter*).—I do not care to discuss Mr. Chapman's conclusions in regard to the acid test except to state that I do not believe that the acid test should ever be used as the sole basis for specification. Taken in connection with other data, however, I think the acid test capable of furnishing information which may be of value under certain circumstances. With regard to Mr. Chapman's roof rusting tests, I do not believe that conclusions drawn upon them are justified by the facts. A very comprehensive series of tests on much larger test pieces which have been under my supervision and which are not yet completed, are giving indications at variance with Mr. Chapman's observations on his two small test pieces. I believe the subject to be an important one and that judgment and conclusions should be reserved until more data are in hand.

Mr. Speller. Mr. F. N. Speller (*by letter*).—The accelerated acid test was first discussed by the Committee on Corrosion, American Society for Testing Materials, in 1907. Since then numerous comparative tests have been made, the majority of which show that the acid test points to the opposite conclusion to that arrived at under natural corrosion conditions, that is, where air and water are the controlling factors. It is evident to me after studying a few hundred of such comparisons that the general application of the acid test is impossible. If the comparisons be limited to iron or steel made by the same process, the test may yet prove to have value; but to compare Bessemer steel or wrought iron with soft open-hearth steel or ingot iron on the basis of the acid test, is certainly unfair and misleading.

THE MARKED INFLUENCE OF COPPER IN IRON AND STEEL ON THE ACID CORROSION TEST.

By W. H. Walker.

The unwarranted use which has in the past few years been made of the name of the American Society for Testing Materials in urging the validity of the so-called acid corrosion test, concerning which definite action has been taken at the present meeting in the report of Committee A-5 on the Corrosion of Iron and Steel* of this Society, has made the Society unwittingly more or less responsible for two rather grave errors. These are, first, that the rapidity with which a given sample of iron or steel dissolves in acid measures its tendency toward natural corrosion, and second, that a low acid test is an index of great purity of the specimen, i. e., of an extraordinarily high content of iron. Enough data are now available to show the unreliability of the first assumption, and it is the purpose of this paper to show the fallacy of the second.

In investigating samples of iron and steel which had withstood corrosion for years, and which notwithstanding dissolved in acid very readily, together with samples which withstood solution in acid in a remarkable manner, and yet were rusting at the ordinary rate, it was found that the acid-resisting specimens contained in every case a substantial amount of copper; i. e., the presence of copper seemed to be the controlling factor in the resistance to solution in acid.

Upon examining the literature on the subject, sufficient data to justify this conclusion were found. A number of investigators have studied the matter, and the results of all are fairly well presented in the Carnegie Research Report of Pierre Breuil on Copper Steels, in the Journal of the Iron and Steel Institute of Great Britain for 1907. It is here found that small amounts of copper in iron or steel greatly reduce the tendency to pass into solution in acid.

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* See p. 100.—Ed.

In order to determine whether there is any foundation for the alleged fact, so widely disseminated, that a highly acid-resisting iron or steel is necessarily a very pure product, the writer succeeded in having a number of heats made in a basic open-hearth furnace under ordinary conditions from ordinary materials into which metallic copper was added a little before or during tapping. The treatment of the product in the bar and sheet mills, and in the annealing operation, was in every case as nearly identical as could be maintained. Although as the work developed many heats were made, the results were so uniform that only a few will be presented in Table I.

TABLE I.—LOSS IN WEIGHT IN 20-PER CENT. SULPHURIC ACID IN THREE HOURS.

	C	Mn	S	P	Cu	Loss, grams.
A............	0.08	0.50	0.018	0.017	....	0.2235
A_2............	0.08	0.50	0.018	0.017	0.21	0.0075
B............	0.10	0.41	0.027	0.026	0.19	0.0082
C............	0.09	0.31	0.031	0.063	0.19	0.0095

The influence of small amounts of copper in these heats is truly remarkable and shows conclusively that this wonderfully slight tendency to dissolve in acid is not indicative of great purity.

Inasmuch as the presence of metallic copper in contact with iron greatly *increases* the rapidity with which such iron will dissolve in acid, it at first seems surprising that in this case copper should *retard* the action. But we have a direct analogy in the case of zinc and copper. Zinc free from iron dissolves in acid very slowly; mix with it some metallic copper, however, and solution is greatly accelerated. But make an alloy of the two, namely brass, and the solvent action of the acid is negligible. So long as copper is not added to iron or steel in excess of the amount which can remain homogeneously alloyed therewith, the writer can see no reason why there may not be an advantage in its use. But such advantage cannot be ascribed to purity, but rather to what we may call a ferro-brass of small copper content. Reasoning from analogy with zinc, pure iron should dissolve in acid very slowly; but the converse is here shown not to be true, namely, that a slow dissolving iron or steel is on this account to be considered an exceptionally pure product.

DISCUSSION.

The Chairman.

MR. A. S. CUSHMAN (*in the Chair*).—The Chairman would again like to caution the members of the Society against drawing conclusions based upon the results of one analysis or one test. The oxygen content of these samples is not shown. Very pure irons have very high resistance to solution in acid. If, however, there should happen to be an off heat made which was not as well deoxidized as a normal heat would be, the solubility in acid would be surprisingly higher than that of one which had been more thoroughly deoxidized. As we approach chemical purity in iron, as in zinc, the higher the purity the greater the resistance to attack by acid. In order that the metal may dissolve quickly in acid, hydrogen must depolarize rapidly from the surface. If for any reason hydrogen does not disengage and leave the surface, the solution in acid will slow down or perhaps cease entirely. Very pure irons are highly resistant to attack by acid. We do not have to depend on entering the alloy field and the addition of copper in order to make iron highly resistant to acid corrosion. I am not now entering the discussion as to relative atmospheric corrosion as between iron and steel; but I am discussing the effect of copper on the solubility in dilute acids. It is well known that the addition of a quarter of one per cent. of copper will wonderfully reduce the solution pressure of steel in acid. This is probably due to the fact that we have entered the alloy field and are dealing with a substance of low solution pressure. The fact remains that it has been possible to make very pure iron practically free from copper in the open-hearth furnace which possesses a high resistance to acid solution. The effort has been made to have it appear that this resistance is due to the copper content, but I can assure you, as the result of investigations the exact figures of which I am not armed with to-night, that very pure iron practically free from copper has been produced which has as high an acid resistance as any steels that have been alloyed purposely with copper.

Mr. Aupperle.

MR. J. A. AUPPERLE (*by letter*).—The analysis of the various grades of iron and steel, submitted by the author, does not include the oxygen content, which is unfortunate, as oxygen exerts many times the effect of copper. Oxygen in iron accelerates solution in sulphuric acid, while copper retards solution. Ingot iron containing a trace of oxygen and no copper will resist the action of sulphuric acid much better than ingot iron containing 0.05 per cent. oxygen and copper as high as 1.35 per cent.

I will submit data showing pure iron obtained by electrolysis and free from copper, which is less soluble in sulphuric acid than iron containing up to 1.35 per cent. copper. I will also show that Swedish iron containing 0.43 per cent. copper and fairly high oxygen dissolves 6.6 times more rapidly in sulphuric acid than iron low in oxygen and free from copper. This Swedish iron is extremely pure, except with respect to copper. The oxygen content is, however, much higher than in ingot iron, which accounts for the rate of solution in sulphuric acid.

In Table I the losses in sulphuric acid can be graded according to oxygen contents, but not according to copper contents. I am not claiming that the addition of copper to ingot iron or steel will not make a metal less soluble in sulphuric acid. I claim that pure iron, free from copper, is less soluble than high-copper, impure iron or steel. Copper can be added to an impure metal and make it less soluble in sulphuric acid.

TABLE I.—SHOWING ANALYSIS AND SOLUBILITY OF 16-GAGE, 1- BY 2-IN. SAMPLE IN 25 PER CENT. BY VOLUME OF SULPHURIC ACID AT 60° C. TIME OF IMMERSION, FOUR HOURS.

Analysis and Sulphuric Acid Test.

MATERIAL	Si	S	P	C	Mn	O	Cu	N	Loss. per c't.
American ingot iron..........	0.003	0.021	0.002	0.010	trace	0.027	0.05	0.0050	2.40
Electrolytic Iron.	0.000	0.001	0.005	0.006	0.000	0.032	0.00	0.0030	7.01
Swedish iron.....	0.010	0.007	0.032	0.050	0.000	0.058	0.43	0.0047	46.00
Puddled iron.....	0.075	0.009	0.072	0.010	0.010	0.145	0.03	......	100.00
Copper ingot iron	0.005	0.023	0.003	0.015	0.030	0.042	0.386	......	9.11
Copper ingot iron	0.004	0.021	0.004	0.015	0.040	0.050	1.35	......	7.40
Copper ingot iron	0.005	0.032	0.002	0.010	0.020	0.060	0.26	......	9.04
Copper steel.....	trace	0.010	0.023	0.125	0.51	0.030	0.25	......	7.61

In order to prove that the rate of solution of iron depends largely upon the oxygen content, five samples of ingot iron containing various percentages of oxygen were subjected to an

Mr. Aupperle

acid test. It was found that the iron containing the highest percentage of oxygen dissolves ten times more rapidly than the lower oxygen material. This high oxygen iron "E" also contained the lowest copper.

It was a simple matter to prove that the high solubility of "E" was due to the high oxygen and not to the absence of copper. By heating "E" for one hour at 850° C. in an atmosphere of pure hydrogen, deoxidizing it, and then subjecting to the acid test, it was found to be more acid resisting than any of the other samples, notwithstanding the fact that it contained the lowest copper content. The rate of solubility was one-fortieth as much as the same material not deoxidized.

Table II shows the influence of oxygen in increasing the solubility of iron in sulphuric acid:

TABLE II.—SHOWING PERCENTAGE OF LOSSES ON 16-GAGE, 1- BY 2-IN. INGOT IRON, WHEN IMMERSED IN 25 PER CENT. SULPHURIC ACID, AT 60° C.

TIME.	A	B	C	D	E
1 hour	0.30	0.30	0.33	0.53	0.87
2 hours	0.72	0.71	0.89	2.08	9.20
5 hours	2.40	3.12	2.21	6.74	27.77
5 hours after deoxidizing	....	....	....	....	0.70

Analysis.

	A	B	C	D	E
Sulphur	0.015	0.023	0.028	0.025	0.020
Phosphorus	0.002	0.003	0.005	0.003	0.004
Carbon	0.01	0.015	0.010	0.010	0.010
Manganese	trace	0.015	0.030	0.030	0.025
Oxygen	0.023	0.028	0.040	0.047	0.069
Copper	0.14	0.065	0.100	0.100	0.021

Mr. Cushman.

MR. CUSHMAN (*by letter*).—Mr. Walker has called attention in this paper to the well-known fact that a copper-iron alloy containing about a quarter of a per cent. of copper is singularly resistant to attack by dilute sulphuric acid. This subject has been referred to in my own paper which is printed in this volume.* Mr. Walker states that enough data are now available to show the fallacy of any claim that a low acid test is an index of great purity of the specimen, i. e., of an extraordinarily high content of iron.

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* See p. 387 .—ED.

Mr. Cushman. It is my opinion that Mr. Walker is giving a wrong impression by this unqualified statement. There is certainly insufficient evidence set forth in his paper to support any such sweeping conclusion. It may be freely admitted that a coppered steel containing about a quarter of one per cent. copper is far more resistant to the attack of acid than an ordinary steel. It is probable that nearly all the steel made to-day will carry small fractions of a per cent. of copper. This is owing to the fact that much of the pig iron and almost all of the steel scrap used contains more or less copper. It is almost certain, however, that when a steel is found to contain 0.20 per cent. or more of copper, it has either been deliberately added by the manufacturer or some carelessly mixed scrap has been used in the furnace. In either case, the product is exceptional, and is not to be compared with the general run of commercial steels or irons. As Mr. Walker himself points out, we have in such a case to do with a "ferro-brass" of small copper content, which may or may not be of commercial value. Irons of extraordinary purity have a very high resistance to acid attack, and do not depend in this respect upon the formation of a special alloy.

No one would be so foolish as to deliberately add expensive copper to steel, except for experimental purposes, until some desired end could be attained by so doing. Steel manufacturers generally, and Mr. Walker himself, do not believe that the acid test bears a relation to service corrosion resistance. Therefore, the production of coppered steels can only be considered an attempt to duplicate the extraordinary acid resistance of the very pure commercial irons. The writer does not go as far as some authorities in condemning the acid test. He is on record as stating that it is a helpful test when properly used and interpreted. While it should not be used as a sole criterion for specification, *other things being equal*, it is capable of furnishing information of some value. Other things are not equal, however, when comparing a "ferro-brass" with the ordinary run of commercial steels and irons.

It is to be hoped that Mr. Walker's statements will be accepted, only as qualified by these considerations.

Mr. Walker. MR. W. H. WALKER (*by letter*).—The foregoing discussion serves but to obscure the point of my paper, which is simply

Mr. Walker.

this,—that a high resistance to solution in acid, when shown by a sample of iron or steel, is not in itself a proof, or even an indication, of great purity. It is well known that the country has been flooded with advertising literature the burden of which is that, if a sample of iron or steel shows a marked resistance to solution in acid, it is for this reason proven to be very pure; and this notwithstanding the fact that the material advertised as exceptionally pure carried, as a rule, from 0.10 to 0.20 per cent. copper. My paper was intended to point out the fact that purity is not assured by such an acid test, unless the word "pure" is taken to mean absence of impurities other than copper.

It is not obvious wherein these statements are changed when "qualified by the considerations" contributed by Mr. Cushman.

SOME TESTS ON THE RATE OF CORROSION OF METALS EXPOSED TO LOCOMOTIVE GASES.

By A. W. Carpenter.

The problem of finding a suitable metal for structural purposes, not prohibitive in cost, which will resist corrosion when exposed to locomotive gases, is important to railroads on account of the necessity for the use of metal in constructions where it is so exposed. In engine-house construction especially is there need of such a metal, both for the fastening of timber and other non-corrosive materials and for smoke-jacks. Such a metal is also in demand for bridge construction over steam-operated tracks and in other similar structures.

In order to get first-hand information as to the resistance of metals to corrosion in such an exposure, tests have been made in the Engineering Department of the New York Central and Hudson River Railroad, Mr. George W. Kittredge, Chief Engineer, which, while perhaps crude and limited, still are thought to give some valuable information both as to the rate of corrosion of the metals tested and the comparative rate of corrosion of iron and steel.

The tests were made in the following manner: Pieces of the metals to be tested, generally in the form of thin sheets, were measured, drilled for chemical analysis, weighed and exposed in the corrosive atmosphere. At certain times the sheets were removed, cleaned, weighed and the loss in weight recorded. The test pieces were exposed in two places: in Weehawken tunnel, where the atmosphere is very highly charged with locomotive gases and steam, and in the interior of a smoke-jack in the Rensselaer engine-house. In the tunnel exposure the specimens were attached to a wooden frame fastened to one side of the tunnel, in which location they received no blast action from locomotives and it was thought that all would be exposed to the same conditions. In the second case the plates were hung inside the jack so that the conditions would probably be alike for all plates.

Three series of these tests have been made so far. The first consisted of three sheets of special iron and two of soft steel, exposed in the tunnel. This being the first series, inaugurated mainly to determine in a general way the comparative resistance of the two materials to corrosion, the sheets were not weighed before the first exposure. They were removed at the end of 64 days; corrosion had commenced on all the plates, without any very noticeable difference.

It was then seen that in order to properly measure the corrosion, the test specimens must be weighed and the rate of corrosion determined by the loss of weight per unit of exposed area. The specimens were weighed and exposed again, this time one set in the tunnel and one set in the smoke-jack. When it was attempted to remove them again, only one plate could be located, a special iron plate exposed in the smoke-jack. This plate was re-exposed several times with results as shown in Table I.

TABLE I.—FIRST SERIES; SPECIAL IRON PLATE.

Period Exposed, days.	Total Loss, grams.	Loss per Sq. In., grams.	Average Loss in Thickness of Plate, ins.
61	196	0.76	0.012
126	338	1.31	0.020
189	560	2.18	0.034

This plate corroded, therefore, at the rate of about $\frac{1}{32}$ in. per year for each surface, or $\frac{1}{16}$ in. per year for both sides exposed. The analysis of this plate showed: carbon, 0.069 per cent.; manganese, none; sulphur, 0.025 per cent.; phosphorus, 0.008 per cent.

The second series of tests included three kinds of metal: first, two plain black and two galvanized sheets of special iron, accepted as the same as that of the first series; second, one sheet of soft steel; and third, two sheets of special-coated metal.

The data and results of this series of tests are given in Table II. In the tunnel test the metals showed resistance to corrosion in the following order, beginning with the most resistant:

	Loss per Sq. In., grams, 289 days' exposure.
Special-coated metal	0.51
Soft steel, uncoated	0.55
Special galvanized iron	0.67
Special iron, uncoated	1.72

TABLE II.—SECOND SERIES.

EXPOSED IN WEEHAWKEN TUNNEL.

Plate No.	Kind of Metal.	Dimensions, ins.*	Area of Surface, sq. ins.	Weight Before Exposure, grams.	Chemical Analysis, per cent.†					First Examination.			Second Examination.			Third Examination.		
					Si	S	P	Mn	C	Days Exposed.	Total Loss, grams.	Loss per Sq. In. of Surface, grams.	Total Days Exposed.	Total Loss, grams.	Total Loss per Sq. In. of Surface, grams.	Total Days Exposed.	Total Loss, grams.	Total Loss per Sq. In. of Surface, grams.
1	Special coated metal. . .	$10\frac{1}{4}$ x 12 x $\frac{1}{32}$ $9\frac{3}{8}$ x 12 x $\frac{1}{32}$	246.4 227	458 317	0.02 0.05	0.056 0.105	0.116 0.122	1.02 0.37	0.090 0.091	231	106	0.48	289	130	0.51	. . .	. . .	. . .
3	Soft steel, plain, box annealed (open hearth)	$10\frac{1}{4}$ x 12 x $\frac{1}{16}$ $9\frac{1}{4}$ x 12 x $\frac{1}{16}$	249 225	1,006 783	0.05 0.04	0.026 0.035	0.019 0.013	0.39 0.37	0.110 0.096	231	128	0.51	289	144	0.55	. . .	. . .	. . .
4	Special iron, plain.	11 x 12 x $\frac{1}{16}$ 10 x 12 x $\frac{1}{16}$	267 243	1,084 585	0.02 0.01	0.021 0.029	0.015 0.006	None None	Trace Trace	231	442	1.66	289	460	1.72	. . .	. . .	. . .
6	Special iron, galvanized	$10\frac{7}{8}$ x 12 x $\frac{1}{16}$ $9\frac{3}{4}$ x 12 x $\frac{1}{16}$	263.3 237	1,074 825	0.01 0.06	0.027 0.026	0.001 0.006	None None	Trace Trace	231	160	0.61	289	179	0.67	. . .	. . .	. . .

EXPOSED IN RENSSELAER ENGINE-HOUSE SMOKE-JACK.

Plate No.	Kind of Metal.	Dimensions, ins.*	Area of Surface, sq. ins.	Weight Before Exposure, grams.	Si	S	P	Mn	C	Days Exposed.	Total Loss, grams.	Loss per Sq. In. of Surface, grams.	Total Days Exposed.	Total Loss, grams.	Total Loss per Sq. In. of Surface, grams.	Total Days Exposed.	Total Loss, grams.	Total Loss per Sq. In. of Surface, grams.
2	Special coated metal. . .	$10\frac{5}{8}$ x 12 x $\frac{1}{32}$	256.5	468	0.02	0.056	0.116	1.02	0.09	65	34	0.13	Plate lost.					
5	Special iron, plain.	$10\frac{3}{4}$ x 12 x $\frac{1}{16}$	260.8	1,054	0.02	0.021	0.015	None	Trace	65	138	0.61	128	326	1.22	193	598	2.24
7	Special iron, galvanized	$10\frac{7}{8}$ x 12 x $\frac{1}{16}$	263.3	1,082	0.01	0.027	0.001	None	Trace	65	118	0.45	128	298	1.11	193	561	2.09

NOTES.—Plates 1 and 2, 4 and 5; 6 and 7, were accepted as duplicates of the same composition in respective pairs.

*After first examination Plates 1, 3, 4, and 6 had a 1-in. strip cut from the bottom to be sent to the chemist for check analysis. The lower figures for dimensions, areas and weights apply to the reduced plates.

† The analyses given by the upper row of figures for Plates 1, 4, and 6 are from mixed drillings from Plates 1 and 2, 4 and 5, 6 and 7, respectively. The analyses given by the lower row of figures for Plates 1, 3, 4, and 6 are check analyses made after first examination. The analyses given for Plates 2, 5 and 7 are the same as in the upper rows of figures for Plates 1, 4, and 6. The upper row of figures for Plate 3 apply to the original analysis; the lower ones to the check analyses.

TABLE III.—THIRD SERIES.

EXPOSED IN WEEHAWKEN TUNNEL.

Plate No.	Kind of Metal.	Dimensions, ins.	Area of Surface, sq. ins.	Weight Before Exposure, grams.	Chemical Analysis, per cent.							First Examination.		
					Si	S	P	Mn	C	Pb	Zn	Days Exposed.	Total Loss, grams.	Loss per Sq. In. of Surface, grams.
01	Special iron, "B" plain	11 x 12 x 1/16	267	979	0.026	0.029	0.0055	0.030	Trace	....	....	58	20	0.075
03	Special iron, "B" plain	11 x 12 x 1/16	267	959	0.024	0.034	0.0045	0.033	Trace	....	....	58	21	0.078
019	Special iron, "B" galvanized	11¼ x 12 x 1/16	269.8	1,037	0.0047	0.041	0.006	0.030	Trace	None	3.79	58	36	0.137
05	Wrought iron, plain (rolled from piled puddle iron)	11 x 12 x ¼	270	2,200	0.110	0.039	0.17	0.013	0.062	....	....	58	25	0.092
015	Wrought iron, plain (rolled from piled puddle iron)	4 x 11 x ¼	91.8	725	0.140	0.041	0.16	0.010	0.062	....	....	58	8	0.087
07	Soft open-hearth steel, plain	11 1/16 x 12 x ¼	271.3	2,204	0.018	0.039	0.017	0.41	0.19	....	....	58	28	0.103
017	Soft open-hearth steel, plain	4 x 11 x ¼	91.8	722	0.023	0.039	0.020	0.44	0.18	....	....	58	8	0.087
09	Special non-ferrous metal, plain	11 x 12 x 1/16	267	1,197	Assumed same as Plate 010.†						....	58	60	0.225
011	Lead-coated galvanized steel	10⅞ x 12 x 1/16	263.9	849	0.012	0.080	0.086	0.39	0.075	*1.90*	*1.58*	58	13	0.049
013	Lead-coated galvanized steel	11 1/16 x 12 x 1/16	268.4	920	0.014	0.059	0.083	0.34	0.075	4.03	1.42	58	23	0.085
037	Special iron, "A" plain	10⅞ x 12 x 1/16	263.9	953	0.014	0.035	0.003	None	Trace	....	....	58	13	0.049
039	Special iron, "A" plain	11 x 12 x 1/16	267	957	0.014	0.035	0.003	None	Trace	....	....	58	12	0.045
041	Special iron, "A" galvanized	10⅞ x 12 x 1/16	263.9	1,009	0.0065	0.026	0.001	None	Trace	None	*0.87*	58	32	0.121
021	Bessemer steel bar	3 x 12½ x 5/16	83	1,461	0.0120	0.064	0.12	0.37	0.077	....	....	42	9	0.109
023	Bessemer steel bar	3 x 12 x 5/16	81.4	1,457	0.01	0.066	0.12	0.37	0.077	....	....	42	15	0.185
025	Bessemer steel angle	2 x 2 x ¼ x 12*	76	1,158	0.01	0.105	0.10	0.45	0.079	....	....	42	7	0.092
027	Bessemer steel angle	2 x 2 x ¼ x 12*	76	1,177	0.02	0.089	0.09	0.40	0.079	....	....	42	8	0.105
029	Open-hearth steel bar	3 x 11½ x 5/16	78.1	1,369	0.021	0.052	0.007	0.27	0.085	....	....	42	6	0.077
031	Open-hearth steel bar	3 x 11½ x 5/16	80.6	1,414	0.014	0.046	0.008	0.27	0.087	....	....	42	7	0.087
033	Open-hearth steel angle	2 x 2 x ¼ x 12*	76	1,143	0.11	0.044	0.009	0.39	0.21	....	....	42	5	0.066
035	Open-hearth steel angle	2 x 2 x ¼ x 12*	76	1,155	0.11	0.062	0.007	0.42	0.22	....	....	42	4	0.053

NOTE.—Analyses of Plates 013 and 019 were made on sheared samples; of Plates 011 and 041, on drilled samples. The lead and zinc contents of the latter are not considered as reliable as the former and are indicated by *italics*.

* 6 ins. were cut off of one leg of each angle for analysis.

† Analysis of Plate 010 (Series III, exposed in smoke-jack): Si. 0.27, Fe. 2.37, Cu. 27.57, Ni. 69.68.

The excessive corrosion of the special uncoated iron over that of the others will be noted. The amount of this corrosion, compared with that of the same material in the smoke-jack tests, shows the tunnel conditions to be much less severe than those of the smoke-jack.

One of the special-coated plates, one galvanized special iron plate, and one plain special iron plate were exposed in the smoke-jack. At the first examination, after 65 days' exposure, the special-coated metal showed very much less corrosion than the special iron plates, but was lost before the second examination took place, leaving only the special iron plates, one plain and one galvanized, in this test. The galvanizing was pretty well removed during the first period of exposure (65 days) and thereafter did not assist much, although it did a little. The last examination, after a total exposure of 193 days, showed a loss per square inch of surface of 2.24 grams for the plain and 2.09 grams for the galvanized plate, which results compare very favorably with the loss of 2.18 grams per square inch after 189 days' exposure as noted for the plate of similar material in the first series.

A third and more extensive series of tests has been inaugurated this year and one examination of one exposure has been made. This series includes special irons, ordinary wrought iron, various steels, and some special non-ferrous metal. Some of the irons and steels have galvanized coatings, and some have lead coatings over the galvanized coatings.

Table III gives the data and results of the first examination of the specimens exposed in the tunnel. Specimens of the same materials differing only slightly in chemical composition have been exposed in the smoke-jacks, and the results will be reported later.

Some of the results of the examination recorded for this series were unexpected. For the 58-day exposure, the galvanized plates showed a much greater loss than the corresponding plain sheets. The loss of one lead-coated plate was some 75 per cent. greater than that of the other, which might be accounted for by the difference in thickness of the respective lead coatings as indicated by the chemical analyses of the two plates; but as one of these analyses was made upon sheared samples and the other upon drillings, the writer does not wish to lay too much emphasis upon this point. The average loss of the two lead-coated plates was greater than that of one of the special irons. The special "A' iron was accepted as the same material as the special iron of previous tests; in this examination it reversed its previous record, being the most resistant for the short period exposed.

The relative order of resistance of the specimens is as follows, beginning with the most resistant:

58-Day Exposure.	Average Loss per Sq. In., grams.
Special iron sheets, "A," plain	0.047
Lead-coated galvanized steel sheets	0.067
Special iron sheets, "B," plain	0.077
Wrought-iron sheets, plain	0.090
Open-hearth soft steel sheets, plain	0.095
Special iron sheet, "A," galvanized	0.121
Special iron sheet, "B," galvanized	0.137
Special non-ferrous metal sheet, plain	0.225
42-Day Exposure.	
Open-hearth steel angles, plain	0.060
Open-hearth steel bars, plain	0.082
Bessemer steel angles, plain	0.098
Bessemer steel bars, plain	0.147

The thickness of metal corroded per year at the above rates, assuming a uniform rate of corrosion, would be 0.002 in. to 0.011 in. on each surface exposed.

It should be borne in mind that the time of exposure for this series was too short to lead to proper conclusions from the results. The specimens in the smoke-jack will be removed and examined after they have been exposed about the same number of days as those which were in the tunnel, and both sets will be returned to their respective exposures and the tests continued, it being the intention to make examinations after successive exposures of about 60 days' duration.

If these tests are thought of sufficient interest to the Society, an endeavor will be made to report the further results. It is hoped that other disinterested parties, perhaps the Society itself, will take up this investigation so that the facts may be well established.

Acknowledgments are due to Mr. R. W. Mahon, Chemist and Engineer of Tests of the New York Central and Hudson River Railroad Company and a member of the Society, for reporting the chemical analyses.

FURTHER RESULTS OF THE WESTINGHOUSE, CHURCH, KERR AND COMPANY PAINT TESTS.

By C. M. Chapman.

In the paper presented to this Society last year we gave the results of an extended series of exposure tests of structural steel paints conducted by Westinghouse, Church, Kerr and Company.* The coatings tested were divided into classes depending upon their principal ingredients and a table was presented which showed the comparative degree of protection afforded by fourteen classes or groups of paints. These tests have been continued, and other paints added, until now over one thousand coatings have been exposed to the weather, spread on approximately three thousand steel plates. The paints reported on last year consisted chiefly of ready-mixed products, sold ready for application; but a considerable number of those which are included in this report, and which had not been exposed long enough to be included last year, were made up in the laboratory, from standard pigments and vehicles, while others were supplied by the makers in the paste form and thinned for use with raw linseed oil and drier.

As last year's report gave a full description of the method of conducting the test, the details will not be repeated in this paper. We wish to emphasize, however, two points in regard to details of the method of testing. The first is the desirability of pickling the plates in dilute sulphuric acid to remove the mill scale before painting, as recommended and described in the Report of Committee E, Proceedings, Vol. V, 1905, p. 79. Uniform results cannot be obtained with plates having varying quantities of mill scale left on them. The other point is the desirability of giving the plate an opportunity to rust at some well-defined point, so as to be able to note the effectiveness of the paint film in preventing the spread of rust from that point. This was accomplished in these tests by deeply scratching the corners of each plate through the paint film with a sharp-pointed tool on the day that the plates were exposed. The widely varying degrees of protection against the spread of rust offered by the different paints under test is very clearly and conclusively shown by this method.

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* *Proceedings*, Vol. X, 1910, pp. 401–408.

As described in the former paper, all paints tested were divided into groups according to the principal ingredients which entered into them. Thus there were the carbon group, the graphite group, the iron oxide, red lead, white lead groups, etc., to the number of fourteen. When five samples of paint of the same general character were received, they were placed in a group by themselves, so that owing to the large increase in the number of samples there are more groups in the present report than in the previous one. The additional groups are sub-divisions of former ones, and as stated before comprise at least five paints of a similar nature. Thus, when the paints made from elaterite reached five in number they were separated from the Mineral Hydrocarbon group, under which all such paints had previously been classified, and they now constitute a group by themselves. The same is true of the gilsonite paints. The paints which consist of a mixture of graphite and iron oxide were, until five of that composition had been tested, classed in the miscellaneous group. The groups as at present sub-divided number twenty-one.

Table I shows the average percentage ratings of these groups of paints, after one, two, and three years' exposure to the weather. These ratings are based solely upon the amount of visible rust which had formed on the steel plates. The questions of chalking, checking, fading, gloss, appearance, etc., were not considered in rating the plates; the one quality which we desired to know was the protection against rust.

An examination of this table brings prominently forward several points to which we would call attention:

First, for short time protection, in such cases as the protection of steel for a few months by the application of a single coat, a red lead, white lead, or one of the chromate group, would be satisfactory.

Second, the early and complete failure of the gilsonite and elaterite paints, indicating the unsuitability of these materials for this particular kind of exposure.

Third, the close agreement between the groups comprising

TABLE I.—COMPARATIVE AVERAGE PERCENTAGE RATINGS OF STRUCTURAL-STEEL COATINGS.

Class.	One Year Exposure.				Two Years' Exposure.				Three Years' Exposure.			
	One Coat.		Two Coats.		One Coat.		Two Coats.		One Coat.		Two Coats.	
	No. of Tests.	Average Rating, per cent.	No. of Tests.	Average Rating, per cent.	No. of Tests.	Average Rating, per cent.	No. of Tests.	Average Rating, per cent.	No. of Tests.	Average Rating, per cent.	No. of Tests.	Average Rating, per cent.
Carbon	82	33	67	72	65	3	53	49	61	0	35	25
Carbon and Graphite	5	13	5	38	5	0	5	24	5	0	5	18
Graphite	78	22	67	71	67	4	56	48	30	2½	43	23
Graphite and Iron Oxide	5	49	5	81	5	15	5	65	4	0	3	35
Iron Oxide	101	48	98	77	74	8½	71	48	34	3	52	24
Iron Oxide and Red Lead	5	34	5	60	5	11	5	37	5	0	5	3
Red Lead	61	65	50	81	50	24	39	54	19	4	20	37
Red Lead Primers	..	..	45	76	..	..	43	54	..	..	14	37
White Lead	40	62	28	88	24	17	12	49	16	0	3	0
Lead Compounds (except Red and White)	11	45	12	84	8	10	9	64	8	7	8	36
Zinc Oxide	9	31	9	69	9	0	5	50	9	0	5	23
Mineral Hydrocarbon (no pigment)	62	32	68	58	48	8	55	24	20	4	24	28
Elaterite	12	8	12	24	12	0	12	13	12	0	8	0
Gilsonite	6	0	6	18	6	0	6	8	6	0	6	0
Mineral Hydrocarbons (with pigment)	10	16	11	53	10	0	11	27	10	0	7	9
Vegetable Hydrocarbons (no pigment)	4	4	4	52	4	0	4	0	4	0	4	0
Vegetable Hydrocarbons (with pigment)	9	26	9	60	9	0	9	20	9	0	6	0
Cement	8	31	8	40	8	13	8	27	0	..	0	..
Chromate	20	50	30	76	14	8	24	33	12	0	9	30
Vehicle	30	6	15	35	30	0	7	0	30	0	7	0
Miscellaneous	103	41	121	67	68	7	86	38	63	0	63	19

the two forms of carbon, namely, the carbon and the graphite groups. At the same time it should be noted that combinations of these two pigments have been much less effective.

Fourth, the close agreement between the two carbon groups just mentioned and the iron oxide group is noteworthy. The only distinct advantage possessed by any of them appears to be in favor of the iron oxide for short-time single-coat work, while carbon appears to be somewhat better than graphite for the same purpose. It seems that a mixture of graphite and iron oxide has some advantage over either used alone. Were it not for the small number of tests of this graphite and iron oxide group, the showing made would command attention.

It should also be noted that zinc oxide compares favorably with carbon, graphite, and iron oxide.

White lead appears somewhat superior to any other pigment except red lead for one-coat work, after either one year's or two years' exposure. For two-coat work, while it holds the honors for the first year, it has dropped down to the level of many others at the end of two years. The group marked "Lead Compounds, except Red Lead and White Lead," which includes such pigments as basic sulphate, sublimed blue lead, zinc lead, etc., is making a remarkably good showing throughout the test.

There is one other point which stands out prominently in the table, and that is the durability of the group of red lead primers. Very inferior paints were used for the second coat on many of the plates in this group and yet the average is well up with the best.

These are by no means all of the things shown by this table which are instructive to one interested in paints, but they are the features to which it seems desirable to call particular attention.

Along with and as a part of this series of paint tests some two dozen vehicles and combinations of vehicles were tested with a view of determining their relative value as protectors against corrosion. These vehicle tests were made as follows: The samples of vehicles were obtained from the makers in a condition said by them to be suitable for use in a paint without further treatment, that is, they were prepared paint vehicles, not unrefined raw materials. Each vehicle was tested with three pigments, namely, carbon, artificial graphite, and dry white lead. In the case of some of the heavier oils, a thinner was used to produce a working consistency. The paints were carefully brushed out on 2 by 6-in. clean mild-steel plates similar to those used in the tests described above. Particular care was taken to give a thorough, even, complete coat. The same operator applied all paints. Some of the vehicles were tested on one-coat work only, but the majority of them were tested twice, that is, one-coat and two-coat tests were made.

Table II gives the ratings of such vehicles as have been tested so far, after one year's exposure. These may be looked upon only as a preliminary series, and the results may be studied for what they are worth. Since only one plate containing each vehicle was exposed, we do not recommend that any conclusions be drawn from these results. A much more extended series of vehicle tests are now under way, from which it is hoped that we may be able to draw more reliable deductions. The one point that stands out

TABLE II.—COMPARATIVE PERCENTAGE RATINGS OF VEHICLE TESTS AFTER ONE YEAR EXPOSURE.

Vehicle.	Carbon.		Graphite.		White Lead	
	One Coat	Two Coats	One Coat	Two Coats	One Coat	Two Coats
Raw linseed oil and drier	50	100	33	85	76	90
Bleached varnish, raw linseed oil and drier	0	...	0	...	45	...
Raw linseed oil, 11% boiled oil containing 0.5 % MnO_2	0	...	0	...	60	...
Raw linseed oil, 25% boiled oil containing 1% PbO_2	0	...	0	...	55	...
Heavy body raw oil thinned with turps and drier	60	...	0	...	70	...
Heavy body raw oil thinned with benzine and drier.	0	...	30	...	70	...
Cold pressed raw linseed oil and drier	60	...	0	...	80	...
Boiled linseed oil "G"	0	...	0	...	50	...
Boiled linseed oil "A"	0	...	0	...	0	...
¾ boiled linseed oil, ¼ China wood oil	85	100	0	65	70	100
¾ boiled linseed oil, ¼ China wood oil (20% drier).	0	...	65	...	70	...
½ boiled linseed oil, ½ China wood oil	85	100	30	70	75	85
China wood oil	65	100	30	70	90	100
¾ raw linseed oil drier, ¼ raw fish oil	90	100	65	100	85	90
½ raw linseed oil drier, ½ raw fish oil	90	100	70	90	85	100
¼ raw linseed oil drier, ¾ raw fish oil	85	100	75	100	85	100
¾ raw linseed oil drier, ¼ boiled fish oil	75	90	65	90	90	100
½ raw linseed oil drier, ½ boiled fish oil	75	100	15	80	80	90
¼ raw linseed oil drier, ¾ boiled fish oil	70	100	30	85	75	60
½ raw linseed oil, ½ proprietary Japan oil "A"	0	...	0	...	60	...
Proprietary Japan oil "B"	0	...	0	...	0	...
½ raw linseed oil, ½ proprietary compound	0	...	0	...	0	...
Linseed oil substitute	30	...	60	...	75	...
Average	38.3	99	23.6	83.5	60.2	91.5

prominently in this table and which calls for further experiment and investigation, is the result obtained with China wood oil and fish oil used in conjunction with linseed oil.

While these tests are primarily vehicle tests, they present at the same time an opportunity of comparing the average results obtained with the three particular pigments used when mixed with a variety of vehicles. To facilitate such a comparison the average rating of each pigment is given at the foot of each column. These averages show again the superiority of white lead over carbon or artificial graphite for one-coat work for short periods, as noted in Table I, while for two-coat work it apparently loses its advantage.

As many of the paints included in this series of tests were prepared in the laboratory with pigments and vehicles of known purity and quality, it is interesting to note how such paints compare with the average of those obtained ready mixed from the manufacturers. Take for instance the chromate group. There were ten samples of paints tested which the makers claimed were rendered inhibitive by the use of chromates or chromic acid in some form. With these were tested a number of pigments and pastes containing chromium which were received from or through the laboratory of the Paint Manufacturers' Association. The averages of the results obtained with the chromates from the laboratory of the Paint Manufacturers' Association were much higher than those shown by the ready-mixed paints from the makers, especially for one-coat work. A comparison of the results obtained with ready-mixed and laboratory-mixed paints is shown in Table III. A similar comparison is made in this table of the results obtained with various carbon pigments mixed with pure linseed oil in the laboratory, with the ready-mixed carbon paints as submitted by the manufacturers. The wide difference in favor of the laboratory-mixed samples would seem to indicate the use of inferior materials in the factory-made paints.

Table III.—Comparative Results Obtained with Paints Received Ready-Mixed and Paints Mixed in the Laboratory; One Year Exposure.

Class.	One Coat.		Two Coats.	
	No. of Tests.	Average Rating, per cent.	No. of Tests.	Average Rating, per cent.
Chromate paints received ready mixed....	10	28	10	61.5
Chromate paints mixed in the laboratory..	10	72.5	16	86
Carbon paints received ready mixed......	70	26	56	69
Carbon paints mixed in the laboratory....	12	74	11	90
Red lead paints received ready mixed.....	48	62	41	80
Red lead paints mixed in the laboratory...	13	77	9	86
Iron oxide paints received ready mixed..	72	42	60	74
Iron oxide paints mixed in the laboratory .	19	88	19	93

In the case of red lead paints the difference does not appear to be so great, especially for two years' exposure. That the difference between laboratory-mixed and ready-mixed red lead paints is not greater, may seem rather surprising in view of the fact that such large proportions of light inert fillers or suspenders are usually introduced into ready-mixed red leads, whereas the laboratory paints were more nearly pure lead oxide. One interpretation of these results might be that, just as a small proportion of the proper chromate will render a paint inhibitive, so something less than 100-per cent. red lead will make a good protector for steel. Again, in the case of iron oxides the nineteen samples of ground pigments mixed in the laboratory with pure linseed oil gave results averaging much above those obtained with about seventy samples of ready-mixed products in the same group.

This table and the lesson it points is worthy of a separate paper, which might well be of the nature of a sermon to paint manufacturers on the proposition, "it requires good pigment and good oil to make good paint."

DISCUSSION.

MR. A. S. CUSHMAN (*in the Chair*).—The Chairman desires to congratulate Mr. Chapman on his novel and interesting method for determining the relative protective power of the various pigments. It is probable that the opinion will prevail that if the test pieces had been somewhat larger the results would have been more valuable; but nevertheless the results are suggestive and interesting. The paper is now open for discussion. **The Chairman**

MR. G. D. WHITE.—I should like to ask Mr. Chapman whether, if he had taken the same number of mixed paints as of laboratory samples, the results would not have agreed more nearly, or whether there would have been the same variation of percentage ratings, for the same number of test paints? **Mr. White.**

MR. C. M. CHAPMAN.—I do not know what the results would have been if something else had been done; but the figures as set forth in the table are correct. I do not mean to intimate that the paint manufacturers cannot, or that some of them do not, produce as good paint in the factory as can be produced in the laboratory; but the facts are as pointed out, that the average of the iron oxides submitted by the manufacturers fell far below the average of the iron oxides mixed in the laboratory, and those laboratory-mixed iron oxides were samples of pigments received from the pigment manufacturers. They were submitted as samples of commercial pigments. The oil was pure linseed oil. **Mr. Chapman.**

MR. WHITE.—Is it not true that a number of the ready-mixed paints averaged very much higher than the general average? **Mr. White.**

MR. CHAPMAN.—It would be necessary to refer to the records to answer that question. Some of the ready-mixed paints are very much better than others. There is no uniformity either **Mr. Chapman**

Mr. Chapman. in the laboratory-mixed paints or in the ready-mixed paints. They both vary greatly. Probably by eliminating the poorer ones I could have changed the results very much. I did not eliminate any of either class.

Mr. White. MR. WHITE.—The point I want to make is that the general average of the laboratory-mixed paints is probably better than that of the ready-mixed paints, for on the other hand the average of the good grades of ready-mixed paints would run much more closely, I judge, to the average of the laboratory-mixed paints.

Mr. Chapman. MR. CHAPMAN.—All the paints tested were submitted voluntarily by the makers. Those makers who are knowingly making a cheap substitute for paint have not been very anxious to submit samples. These paints were presented by the makers as being products that they were anxious to have tested, and that they thought would show up well against any others. They are, therefore, probably representative of the better grades of ready-mixed paints for structural steel on the market.

Mr. Heckel. MR. G. B. HECKEL.—Suppose you had two samples from the same manufacturer of which one ran high and the other very low, so that the average of both would be 50 per cent.; that of course would be unfair to the better sample of the two. If a number of good paints were averaged with a number of bad ones, would not that materially lower the average? I think that is what Mr. White meant to ask.

Mr. Chapman. MR. CHAPMAN.—That is undoubtedly true, and it also applies with equal force to the pigments from which the laboratory samples were made.

Mr. Gardner. MR. H. A. GARDNER.—I should like to ask Mr. Chapman whether the spreading rate was determined on the painted samples or whether any definite spreading rate was adopted?

Mr. Chapman. MR. CHAPMAN.—No uniform spreading rate was adopted, and no records were kept of the spreading rate. Each paint was spread at the rate that the paint seemed to be suited for. A good flowing coat was applied in all cases and brushed out sufficiently to prevent any running or sagging of the paint when suspended vertically.

Mr. Gardner. MR. GARDNER.—I cannot help but think that on small plates with a surface of less than one-twelfth of a square foot it would be very difficult to apply in every case about the same amount of paint. Even with some of the large wooden test panels upon which no spreading test was made, it was rather difficult to get the same amount of paint on in every case, and it would seem even more difficult with very small test pieces of the sort used by Mr. Chapman. I think the results would have been much better if Mr. Chapman had used larger plates, say 12 by 18 ins. More comparable results would then have been obtained. **Mr. Gardner.**

THE CHAIRMAN.—The Chair would like to point out that conclusions may be drawn from tests of this kind that may have a wide influence not only in this but in other countries. The Chair would like to interject a caution in respect to drawing conclusions from tests of this kind, however cleverly they have been carried out. It is certainly unsafe to draw sweeping conclusions from tests of protective coatings applied to very small test samples exposed in but one locality. It should not be forgotten that the unpainted edges of very small samples produce an important effect, as rust starting from the rough cut edges may soon spread under the protecting paint coatings. **The Chairman.**

MR. R. S. PERRY.—May I say a word following up what has just been said? I think the author of this paper has put before us research involving, in the number of units that he has used, greater scope than any similar work that I have seen brought before any scientific body along the lines of investigating the prevention of corrosion of iron and steel by proper protective coatings; but I cannot avoid raising my voice here to deprecate the size of these pieces of material upon which he made these tests, which are only 8½ per cent. of a square foot. The pieces of metal in the field tests under the control of this Society are 192 times the size of his test pieces, and I feel that the master painters and the public who wish to see practical work come from this Society may draw the conclusion that our work is theoretical rather than practical. I regret, therefore, that these tests were on so small a scale. **Mr. Perry.**

MR. A. W. CARPENTER.—I think the tests of the laboratory-mixed paints are particularly interesting, inasmuch as they showed better results in every case than the paints furnished by manufacturers. I should therefore like to ask for more information as to how these paints were made in the laboratory, that is, with **Mr. Carpenter.**

Mr. Carpenter. what kind of vehicle, what kind of drier, and whether the pigments were ground, or mixed by hand?

Mr. Chapman. MR. CHAPMAN.—In the case of the laboratory-mixed paints the pigments were submitted dry. They were ground to a thick paste by means of a hand-glass muller on a ground-glass plate with raw linseed oil. They were then thinned to a working consistency with raw linseed oil to which was added about 6 per cent. of a turpentine japan drier.

The tabulation here given was made within a few weeks. The experiments have been going on for several years. There was no idea at the time of making the experiments that such a comparison would ever be made. There was no predetermination to make such a comparison, and therefore no selection of the pigments or paints used in either case.

There has been some criticism of the small size of the steel plates used in these tests, and in reply it may be stated that we have duplicated on these small plates tests that have been made by other experimenters, and that the results compared favorably with the results on large plates; that is, they do not contradict one another materially, but were as nearly in conformity with each other as if they had been made on plates of the same size.

Mr. Fireman. MR. PETER FIREMAN.—I feel that Mr. Chapman has been criticized a little too severely. He has not undertaken to revolutionize things generally, but has simply been conducting a series of tests for his personal satisfaction, and has submitted the results. We should take them for what they are worth, and not blame him because he has not made the tests on a larger scale. He cannot be expected to carry on the work on as large a scale as the tests that are being conducted by the joint representatives of the manufacturers and consumers. I think that he deserves a great deal of credit and that his results are excellent as far as they go. I for myself really do not see that a small plate ought to be of necessity worse than a large plate for paint trials. Of course there may be some little defects here and there in Mr. Chapman's method, but he has used a fair variety of materials, and he now presents the results for comparison. I think his work is perfectly legitimate and that the results are indicative of what any one could do himself without having the whole world to help him. It appears too that they are entirely conclusive, as far as they go. **Mr. Fireman.**

Mr. Pickard. Mr. G. H. Pickard.—May I ask Mr. Chapman if he duplicated the plates on which he spread each preparation; and if so, how those duplicates agreed? For instance, take Paint 1; did he put it on three panels, or on only one at each spreading rate used?

Mr. Chapman. Mr. Chapman.—With the great majority of samples we applied to one plate one coat and to another plate two coats; but in some cases duplicate and even triplicate tests were made. In no case did the second or third result fail to check fairly well with the first result.

Mr. Rinald. Mr. C. D. Rinald.—The criticisms of the size of plates are entirely unwarranted. I have made tests over and over again with small brushes on little pieces of iron, somewhat like those used by Mr. Chapman, and with a regular painter's brush on large sheets. The results have generally checked up closely. I think it all depends on the care used by the painter.

Mr. Chapman. Mr. Chapman (*by letter*).—In further reply to Mr. White's question as to what the results might have been had the poorer ready-mixed paints been eliminated and only an equal number of the best been compared with the laboratory-mixed samples, the following comparisons have been made from the records kept of the tests:

There were 19 iron-oxide paints mixed in the laboratory. These averaged for the one-coat samples at the end of one year, 88 per cent. There were 72 ready-mixed iron-oxide paints tested in the same way and for the same length of time. They averaged 42 per cent., as shown in the table. If we were to consider only the best 19, or about one-fourth of the 72, the average would be 81 per cent., or 7 per cent. less than the average of all of the iron oxides mixed from the dry pigment and linseed oil in the laboratory. If only the best one-fourth of the laboratory-mixed iron oxides were selected for comparison their average would be 98 per cent. Furthermore, 24 out of the 72, or exactly one-third of the one-coat specimens of ready-mixed iron oxides, were entirely covered with rust and were rated

Mr. Chapman. zero at the end of one year, while the lowest rating of any laboratory-mixed sample in this group was 75 per cent.

In a similar manner the other groups might be compared, but lack of space forbids. The result, however, is always the same; the laboratory-mixed samples, in making which only pure linseed oil and a little drier was used, show better averages than the ready-mixed paints.

THE PRACTICAL TESTING OF DRYING AND SEMI-DRYING PAINT OILS.

By H. A. Gardner.

The high price attained by linseed oil during the past two years of over a dollar a gallon, together with the unusual scarcity of this valuable oil, has led many investigators into the field of research, with a view of discovering some mixture of other oils to partly replace linseed oil. Many valuable contributions to oil technology have resulted, but the makers and users of paints have wisely demanded specific and authoritative information as to the practical value of proposed mixtures before adopting them. The Institute of Industrial Research, at the request of the Paint Manufacturers' Association of the United States, has recently started a series of practical paint vehicle tests designed to decide the question at issue.

Forty-eight white-pine panels have been placed upon a test frame on the grounds of the new laboratory building of the Institute, at Washington, D. C. They are painted with a standard white pigment formula reduced with a different oil formula for every panel. White-pine panels were selected for the test on account of the good painting surface which this type of lumber presents; the grade selected was free from knots or pitch pockets—defects which often ruin a paint test. Each panel was constructed of four tongued-and-grooved planed boards, 22 ins. long, 1 in. thick, and 9 ins. wide. The boards were leaded together and capped at the sides with weather strips, making the finished panels about 2 ft. wide and 3 ft. high. The fence upon which the panels were placed was constructed of 4-in. squared yellow pine with open framework, allowing the panels a resting place upon which they were finally secured with sherardized screws.

Before erecting the panels, they were carefully painted in a paint laboratory especially fitted out for the tests. The work was done during the months of April and May, the temperature averaging from 60° to 90° F. This precaution was taken in order that the paint in each case might become thoroughly dry and hard before exposure, so that there would be no accumulation of dust or effect from exposure during the drying period. The actual painting of each panel was done personally by Mr. Charles Macnichol, master painter, of Washington, D. C., who has had a wide experience in the practical application and testing of paints.

The viscous nature of several of the oils tested precluded the possibility of grinding each oil formula with the white pigment base selected; great heating of the paint mills and a paste of insufficient fineness was the result of an early attempt at this method. It was decided, therefore, to grind the standard pigment formula to a thick paste in the minimum amount of raw linseed oil. Subsequently a weighed amount of the white pigment base was

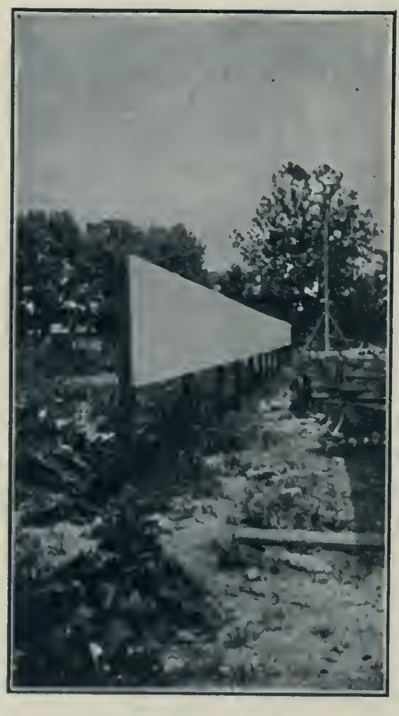

FIG. 1.—View of Vehicle Test Fence.
Institute of Industrial Research.

thinned with the oil formula to be tested, to a standard viscosity, judged by the experienced master painter in charge of the practical application of the formulas as sufficiently heavy for third-coat work. When making the reductions with oil mixtures, an allowance was made for the amount of linseed oil already contained in the ground white pigment base.

During the application of the first coat an equal amount of turpentine was added to each formula, in the proportion of one half pint to a gallon of paint; in the application of the second coat there was added to each formula a like amount of an equal mixture of turpentine and the oil formula under test. The third coat was applied without the addition of thinners of any kind.

It is well known that the time of drying and the condition of the dried film of any oil or mixture of drying or semi-drying oils will vary widely. It is for the purpose of causing oils to set up to a hard film in a short time that metallic driers in the form of salts of manganese and lead, soluble in oil, are added to a paint. Some oils require a large amount of drier, while others require only a very small amount. Those which require a large amount are apt, upon exposure, to be burned up by the drier, resulting in the formation of a powdered and disintegrated film. To add various types of drier or even differing amounts of a drier to the oils under test, seemed very unfair from every standpoint, and it was therefore

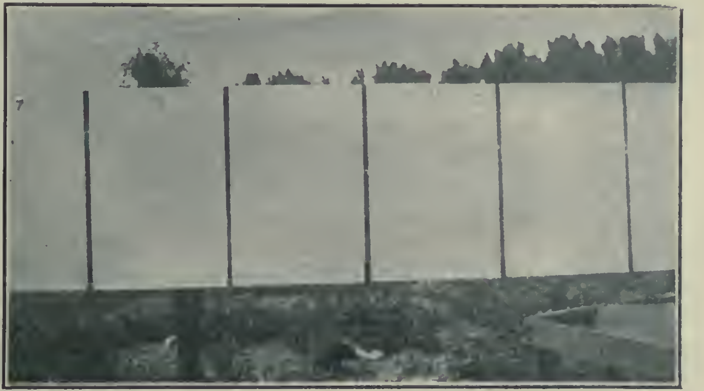

FIG. 2.—Front View of Vehicle Test Fence.

decided to eliminate the drier question entirely, so as not to vitiate the results by bringing in a factor of this nature. The plan of omitting driers proved successful in the Atlantic City steel-panel paint tests, erected three years ago by the writer under the supervision of Committee A-5 of this Society.

The systematic methods which are necessary when making paint tests were carefully followed. A standard weighed amount of white pigment paste was placed in a clean paint cup and thinned to the proper consistency with a weighed amount of the oil under test. Proper reductions were made, as before stated. Weighings of the paint, cup, and brush were made before and after application to the panel, in order to determine the quantity of paint used and the spreading power. A period of fifteen days was allowed between the application of successive coats, in order to give each formula sufficient time to dry thoroughly. Although several of the formulas remained tacky for over a week, all dried thoroughly in the time allotted. (Oils which when used alone have slow drying properties have been found to yield good firm films when used with drying pigments such as lead and zinc.) The backs and edges of each panel were painted with two coats of the paint used on the face of the panel, so as to prevent the admission of moisture. After erection, the panels were numbered with aluminum figures pressed into the surface. Frequent inspections will be made, and at the proper time reports will be issued giving the results of the tests.

During the painting of the panels considerable interesting data were collected, of which the following is a brief résumé:

The hiding power of a paint is one of its most important requisites. It was found in the tests that some oils had the effect of lessening, while others had the effect of increasing the hiding power of the standard pigment formula. This may be due in part to the varying refractive indices of the oils used, as well as to the difference in the quantity of oil required in each test. Some oils were very viscous, while others were very light.

The stiff working of heavy-bodied, blown or heat-oxidized oils, produced films which in some cases gave a very glossy surface, even on the priming coat. Some of these resembled varnished work when finished. It will be of importance to watch these tests carefully for any signs of early breakdown, which might come from too thick a film. The treated Chinese wood oil paints worked rather stiff but produced very smooth films. The rosin oil paints became slightly lumpy on standing, but worked out to a smooth finish somewhat yellowish in color. The marine animal oils, especially the menhaden oil mixtures, dried to a film slightly flatter than straight linseed oil. Any odor which was present in the paints made from the animal oils seemed to disappear a few hours after application. The cotton seed and corn oil mixtures made the slowest drying paints, but at the end of the second week of the drying period they set up rapidly to firm films. Soya bean and perilla oils behaved like straight linseed oil, the former being a little slower and the latter slightly more rapid in drying properties. The perilla oil was made from one of the first importations into this country, and was dark in appearance. It made, however, a very easy-working and hard-drying paint.

The oils used in the tests were obtained from reliable sources. After they were received, they were carefully analyzed. The results of the analyses appear in Table I.

TABLE I. ANALYSES OF OILS USED IN THE VEHICLE TESTS.

	Specific Gravity.	Saponification Number.	Iodine Number.	Acid Number.
Raw Linseed Oil	0.931	188	186	2.0
Boiled Linseed Oil (Linoleate Type)	0.941	187	172	2.7
Boiled Linseed Oil (Resinate Type)	0.930	186	176	2.2
Blown Linseed Oil	0.968	189	133	2.8
Lithographic Linseed Oil	0.970	199	102	2.7
Soya Bean Oil	0.924	189	129	2.3
Menhaden Oil	0.932	187	158	3.9
Perilla Oil	0.94	188	180	2.0
Chinese Wood Oil (Raw)	0.944	183	166	3.8
Chinese Wood Oil (Treated)*	0.898*	128*	104*	6.8*
Corn Oil	0.925	191	118	9.5
Cottonseed Oil	0.921	193	105	3.6
Rosin Oil	0.966	27	41	16.7
Whale Oil	0.924	191	148	....
Neutral Petroleum Oil†	0.916	6	12	....

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* Low constants due to presence of over 40 per cent. of volatile matter, largely petroleum spirits.

† This oil contained over 20 per cent. of petroleum spirits.

The pigment formula selected for the tests had the following composition:

Basic Carbonate White Lead	20	per cent.
Sublimed White Lead	30	" "
Zinc Oxide	35	" "
Magnesium Silicate	10	" "
Barytes	5	" "

One hundred pounds of pigment base were ground to a stiff paste in 16 lbs. of linseed oil. While this pigment formula was not selected as being superior to certain other formulas, it is of a type that has given very fair service in paint tests throughout the country, and will no doubt serve admirably for the purpose designed in these tests.

The vehicle formulas in the finished paints are as follows:

	Per cent.
No. 1.	
Raw Linseed Oil	100
No. 2.*	
Soya Bean Oil	100
No. 3.†	
Menhaden Oil	100
No. 4.	
Raw Linseed Oil	25
Boiled Linseed Oil (Resinate)	75
No. 5.	
Raw Linseed Oil	25
Boiled Linseed Oil (Linoleate)	75
No. 6.	
Raw Linseed Oil	50
Boiled Linseed Oil (Resinate)	50
No. 7.	
Raw Linseed Oil	50
Boiled Linseed Oil (Linoleate)	50
No. 8.	
Raw Linseed Oil	50
Blown Linseed Oil,	50
No. 9.	
Raw Linseed Oil	50
Litho. Linseed Oil,	50
No. 10.	
Raw Linseed Oil	50
Soya Bean Oil	50
No. 11.	
Raw Linseed Oil	50
Menhaden Oil	50
No. 12.	
Raw Linseed Oil	50
Perilla Oil	50
No. 13.	
Raw Linseed Oil	50
Treated Wood Oil	50

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* Dry pigment formula in soya bean oil.

† Dry pigment formula in menhaden oil.

	Per cent.
No. 14.	
Raw Linseed Oil	50
Corn Oil	50
No. 15.	
Raw Linseed Oil	50
Cottonseed Oil	50
No. 16.	
Raw Linseed Oil	50
Rosin Oil	50
No. 17.	
Raw Linseed Oil	50
Whale Oil	50
No. 18.	
Raw Linseed Oil	75
Soya Bean Oil	25
No. 19.	
Raw Linseed Oil	75
Menhaden Oil	25
No. 20.	
Raw Linseed Oil	75
Perilla Oil	25
No. 21.	
Raw Linseed Oil	75
Treated Wood Oil	25
No. 22.	
Raw Linseed Oil	75
Corn Oil	25
No. 23.	
Raw Linseed Oil	75
Cottonseed Oil	25
No. 24.	
Raw Linseed Oil	75
Rosin Oil	25
No. 25.	
Raw Linseed Oil	50
Soya Bean Oil	25
Menhaden Oil	25

	Per cent.
No. 26.	
Raw Linseed Oil	50
Soya Bean Oil	25
Treated Wood Oil	25
No. 27.	
Blown Linseed Oil	50
Soya Bean Oil	50
No. 28.	
Raw Linseed Oil	25
Soya Bean Oil	25
Menhaden Oil	25
Treated Wood Oil	25
No. 29.	
Raw Linseed Oil	25
Soya Bean Oil	25
Menhaden Oil	25
Corn Oil	25
No. 30.	
Raw Linseed Oil	25
Soya Bean Oil	25
Menhaden Oil	25
Cottonseed Oil	25
No. 31.	
Raw Linseed Oil	25
Soya Bean Oil	25
Menhaden Oil	25
Rosin Oil	25
No. 32.	
Raw Linseed Oil	25
Soya Bean Oil	25
Treated Wood Oil	25
Rosin Oil	25
No. 33.	
Raw Linseed Oil	20
Soya Bean Oil	20
Treated Wood Oil	20
Menhaden Oil	20
Cottonseed Oil	20

No. 34.

	Per cent.
Raw Linseed Oil	20
Soya Bean Oil	20
Treated Wood Oil	20
Menhaden Oil	20
Rosin Oil	20

No. 35.

Raw Linseed Oil	40
Soya Bean Oil	20
Corn Oil	20
Cottonseed Oil	20

No. 36.

Whale Oil	33
Treated Wood Oil	33
Raw Linseed Oil	33

No. 37.

Raw Linseed Oil	25
L. O.*	75

No. 38.

Raw Linseed Oil	50
Raw Chinese Wood Oil	50

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* Mixture of boiled tung and soya bean oil, thinned with petroleum and turpentine.

No. 39.

	Per cent
Raw Linseed Oil	75
Reducing Oil*	25

No. 40.

Raw Linseed Oil	50
Soya Bean Oil	35
Neutral Petroleum Oil	15

No. 41.

Raw Linseed Oil	50
Soya Bean Oil	25
Neutral Petroleum Oil	15
Tungate Drier	10

No. 42.

Linseed Oil	25
Soya Bean Oil	37
Neutral Petroleum Oil	23
Tungate Drier	15

No. 43.

Raw Linseed Oil	25
Soya Bean Oil	37
Whale Oil	19
Tungate Drier	19

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* 25 per cent. Raw Linseed Oil.
72 per cent. Petroleum Oil.
3 per cent. Drier—lead and manganese linoleate.

No. 44.

Special test on white base of the following composition, in pure linseed oil:

Asbestine	10 per cent.
Corroded White Lead	20 " "
Sublimed White Lead	30 " "
Zinc Oxide	40 " "

Upper board of panel reduced with straight turpentine on priming coat
Second board of panel reduced with wood turpentine on priming coat.
Third board of panel reduced with pine oil on priming coat.
Bottom board of panel reduced with petroleum spirits on priming coat.

No. 45.

Same pigment formula as No. 44, reduced with:

Pine Oil	50 per cent.
Linseed Oil	50 " "

No. 46.

Special test of white base of the following composition, in pure linseed oil:

Corroded White Lead	20	per	cent.
Sublimed White Lead	30	"	"
Zinc Oxide	35	"	"
Asbestine	15	"	"

No. 47.

Cypress panel unpainted.

No. 48.

Cypress panel painted with Formula No. 1, thinned with benzol on the priming coat.

A NOVEL METHOD OF DETECTING MINERAL OIL AND RESIN OIL IN OTHER OILS.

BY A. E. OUTERBRIDGE, JR.

The importance of having a rapid, simple and reliable method of detecting the presence of cheap mineral (hydrocarbon) oil, or resin oil, as an adulterant in expensive vegetable or animal oils, used in large quantities in industrial works, was forcibly impressed upon my mind some time ago by the following experience: The purchasing agent of the large manufacturing establishment with which I am connected handed me a sample of oil, of a rather dark color, which had been offered as commercially pure linseed oil, slightly off color, suitable for making "oil sand cores," at a concession from the market price of linseed oil.

Preliminary examination indicated that the sample was just what it was represented to be. A "skin test" was made by evaporating a drop or two to dryness in a shallow platinum capsule, and as it gave a good tough skin a trial lot of five barrels was ordered and used for making oil sand cores. This proved very satisfactory and in a short time another lot was bought. After thirty barrels had been purchased, complaints came from the foundry of cores broken in handling, and a larger proportion of oil to sand was used to overcome the trouble. Another shipment making forty-five barrels in all was received and tested in various ways before being used in the foundry. I reported that this oil was, in my judgment, adulterated and unsuitable for making oil sand cores. The purchasing agent notified the shippers; they denied the charge and asked for payment. We then had samples tested by a well-known firm of analytical chemists, who reported that the linseed oil was "largely adulterated with mineral oil and resin oil." On being asked to give the proportions they replied that it was a difficult thing to do and would cost much more than the amount of their bill. Still the shippers refused to remove the barrels, and wrote as follows: "We must insist upon immediate settlement and your check sent promptly will be appreciated." After some months' delay a copy of the State law making it a crime to sell adulterated linseed oil in Pennsylvania, unless so branded, under severe penalties, was mailed to the oil company. This brought a prompt request to send the oil back at shippers' cost, which was done; and the incident was then closed.

It started, however, a new line of investigation which has resulted in a method of instantly detecting such adulterations, the nature and apparent novelty of which method may be clearly and briefly indicated by the following correspondence. I may say that the process had been discovered and put into use, experimentally at least, some time before the first letter was written. The main object of this letter was to ascertain whether any method of detecting such adulteration, not requiring chemical analysis or expert knowledge, was known to the chief chemist of the Bureau of Chemistry of the Department of Agriculture at Washington, D. C., whose life-long investigations of adulterations in such products have given him wide renown.

WILLIAM SELLERS AND COMPANY, INCORPORATED.

PHILADELPHIA, January 6, 1911.

DR. HARVEY W. WILEY, *Chief Chemist*,
Department of Agriculture,
Washington, D. C.

Dear Sir:

Knowing that the subject of adulteration of linseed oil and other oils has occupied the close attention of your department, I write to ask whether you know of any rapid, simple and reliable method, capable of being used in industrial works by any one not skilled in oil analyses, of detecting the presence in linseed oil, lard oil, or other vegetable or animal oil, of mineral oil, either crude or refined? I have, before writing to you, consulted several standard books on oil analyses and testing, also books on lubrication and friction.

I have, of course, found certain chemical tests, but these are too complicated for the purpose, requiring expert chemical knowledge, and even then the results are apparently unreliable,—even an approximate quantitative determination of the proportion of mineral oil adulterant is impracticable by any method that I have found.

Linseed oil is being used more and more largely in foundries in a comparatively new process of making "cores" for molds, and various "core oils" are being sold for that purpose, usually having linseed oil as a basis, with various adulterants, such as cotton-seed oil, mineral oil, etc. These core oils are also often loaded with soluble gums and are made to resemble linseed oil more or less closely in appearance, specific gravity, drying qualities, etc., so that mere inspection is no guide whatever as to their composition or the cost of compounding them.

Any information you may be able to give on the subject of such adulterations of such oils, especially respecting a rapid and simple method of detecting the presence and determining approximately the proportion of mineral oil adulterant in vegetable or animal oils, will be duly appreciated.

Yours very truly,
A. E. OUTERBRIDGE, Jr.,
Metallurgist.

UNITED STATES DEPARTMENT OF AGRICULTURE.
BUREAU OF CHEMISTRY.
CONTRACTS LABORATORY. WASHINGTON, D. C., January 7, 1911.

WILLIAM SELLERS & CO., INC.,
1600 Hamilton St., Philadelphia, Pa.

Sirs:

In reply to your letter of January 6, 1911, in regard to the adulteration of linseed oil, I know of no rapid, simple and reliable method for detecting such adulteration, which can be performed by one not skilled in chemistry. While some tests, such as boiling with an alcoholic solution of caustic potash and afterwards diluting with water and judging from the milky appearance of the precipitate that mineral oil is present, will work in certain cases, in other cases the detection of such adulteration in linseed oil is a problem of extreme difficulty and can be solved only by a man who is skilled in chemistry as applied to oils.

Respectfully,
(Signed) H. W. WILEY,
Chief.

WILLIAM SELLERS AND COMPANY, INCORPORATED.
PHILADELPHIA, April 6, 1911.

DR. HARVEY W. WILEY, *Chief Chemist,*
Department of Agriculture,
Washington, D. C.

Dear Sir:

I have been asked to present at the next annual meeting of the American Society for Testing Materials a novel method by which the presence of mineral oil or resin oil in minute or considerable quantity in vegetable or animal oils may be instantly detected by any one not skilled in chemistry. This method has been tried out and put into practical use in these works for some time past.

It depends upon utilizing certain ultra-violet rays from an electrical source, which I found to possess a remarkable effect in developing "fluorescence" in mineral and resin oils to a degree hitherto unsuspected, whereby a mere trace of either of these oils in a non-fluorescent oil such as linseed oil, Soya bean oil, etc., say one-tenth of one per cent. is instantly revealed. By comparing an adulterated sample of linseed oil or other non-fluorescent oil with prepared samples of adulterated oils as standards, the actual quantity of such adulterant may often be stated accurately by an apprentice boy, who knows nothing of analysis, in a moment.

I propose to give this as a scientific discovery freely, without applying for any patents or other reservations.

On January 6, 1911, I wrote to ask you if any rapid, simple and reliable method for detecting such adulteration in oils which could be performed by one not skilled in chemistry, was known to your department. I duly received your reply of January 7, stating that no such method was known, and this was corroborated by the chemist of the Pennsylvania Railroad, and by others in letters of similar portent.

If entirely agreeable to you I will refer to the correspondence with you in my address, which I expect to illustrate by visual tests, for the purpose of showing the novelty of my method. While there is nothing confidential in the letter I would prefer to have your assent before alluding to it.

Yours respectfully,
A. E. OUTERBRIDGE, JR.,
Metallurgist.

UNITED STATES DEPARTMENT OF AGRICULTURE.
BUREAU OF CHEMISTRY.
CONTRACTS LABORATORY. WASHINGTON, D. C., April 12, 1911.

MR. ALEXANDER E. OUTERBRIDGE, JR.,
WM. SELLERS & CO., INC.,
1600 Hamilton St., Philadelphia, Pa.

Sir.

I beg to acknowledge the receipt of your letter of April 11. I see no objection to your using my letter of January 7, 1911, addressed to William Sellers and Company, in connection with your paper on a novel method of detecting mineral oil in vegetable or animal oils. This is an interesting subject, and I shall take pleasure in reading your article when it appears in print.

Respectfully,
(Signed) H. W. WILEY,
Chief.

Copies of these letters were sent to the chemist of the Pennsylvania Railroad Company with a request for an expression of his opinion for use in this address. He courteously replied as follows:

Dear Sir:

We are thoroughly in accord with Doctor Wiley's reply to you in regard to this matter, and are not in position to suggest any quick test which could be generally used to detect the presence of hydrocarbon oils in linseed oil.

Very truly yours,
(Signed) F. N. PEASE,
Chemist.

Mineral and resin oils differ from all other oils in many ways, but especially in one respect, and resemble each other in many ways, notably in one, which will now engage our attention.

When examined by reflected light, hydrocarbon oils (improperly named "mineral" oils), whether crude or partially refined, show a peculiar greenish tinge commonly called "bloom." When examined by transmitted light the bloom disappears and the true color of the oil is seen. This color ranges from dark red or mahogany tint through various shades of orange and yellow up to "water white," according to the degree of refinement. Resin oil possesses the same peculiar charactistics, except that the color of the bloom is pure blue. Its chemical composition is so nearly like that of a hydrocarbon oil that these resemblances appear to me to be more than accidental coincidences and suggest the possibility of a common origin between so-called mineral oil and resin oil. This speculation, however, is not germane to our topic, which has to do strictly with a new practical application of that property commonly called bloom to the instantaneous detection of adulteration of vegetable or animal oils with hydro-carbon oils.

Doubtless everyone has noticed the bloom in mineral oils and wondered perhaps as to the cause of this singular greenish appearance, which is especially noticeable in crude oil and in heavy lubricating oils. Bloom is merely a popular name for a remarkable property possessed by a number of substances, the scientific name for which is "fluorescence," and the question may now be properly asked, "What is fluorescence?" The New Century Dictionary defines fluorescence thus:

> "The property possessed by some transparent substances of becoming self-luminous while they are exposed to the direct action of light rays. It is especially excited by the violet and ultra-violet rays of the spectrum, and is explained by the change in refrangibility (that is, wave-length) of the incident rays by the substance under experiment. Thus, if a beam of sunlight fall upon a solution of esculin or sulphate of quinine, its path through the liquid is marked by a bluish opalescent light. Again, if a

paper moistened with the solution is exposed to the ultra-violet rays of the spectrum, it becomes blue, since these rays are diminished in refrangibility so as to become visible; by this means the ultra-violet spectrum (given by prisms of quartz) can be studied. The delicate blue surface-color of some fluorspar and the yellowish-green surface color of glass colored with uranium oxide are phenomena of the same nature."

In simple non-technical words, therefore, fluorescence is a property inherent in some substances of becoming self-luminous while exposed to certain rays of light known as "ultra-violet" or "actinic" rays. These rays are always found in sunlight and in some forms of electric light. If a beam of white light from the sun or from an arc-light be passed through a prism (preferably for some purposes made of quartz) it is separated into a broad band of prismatic colors. Such a beam may be likened for illustration to a closed fan; after passing through the prism the fan is opened, and it is seen that each fan-stick has a different color, namely, red, orange, yellow, green, blue, indigo, and violet. Beyond the red, on one side of the prismatic band, and beyond the violet on the other, are many rays which are invisible to the eye; but some substances when placed in the path of these invisible rays become luminous or fluorescent, the color of the fluorescence being characteristic of the substance, some showing green, some blue, some yellow some purple, some white, and others various intermediate tints.

In the course of my investigations I found that the greenish bloom or fluorescence of mineral oil and the blue bloom of resin oil noticeable in daylight can be enormously intensified or magnified, perhaps a thousand fold, under certain conditions to be described herein, so that, if a single drop of mineral oil be placed in a vessel containing a hundred or even a thousand drops of pure linseed oil, or any other non-fluorescent oil, its presence may be instantly detected by the greenish fluorescence which it imparts to the whole of the oil. The same is true of resin oil which gives blue fluorescence. The utilization of this observation for practical purposes of detecting adulterations is the gist of this paper.

By increasing the proportion of either adulterant the intensity of the fluorescence imparted to the naturally non-fluorescent oil is correspondingly increased; and, by preparing standard samples of any non-fluorescent oil containing one-tenth, one, two, three per cent., and upwards, of mineral or resin oil, in clear glass test-tubes placed in a suitable frame against a dark background, each showing readily and unmistakably the increasing proportions of the adulterant under a light giving ultra-violet rays, a "fluorescent scale" has been established, somewhat similar to the well-known carbon color scale used in steel foundry laboratories for quickly determining, by color comparison, the proportion of carbon in an acid solution of steel.

By comparing a sample of non-fluorescent oil which has been adulterated with mineral oil or resin oil with these standards, the proportion of such adulteration may often be accurately stated in a moment by any one. I use the word "often" in connection with the quantitative determination advisedly, in order to allow for some modifications, to be explained further on; but I am prepared to unhesitatingly make the broad assertion without fear of contradiction that there is no method known by which the presence of either mineral or resin oil in any non-fluorescent oil in small or large amount can be so disguised as to be undetectable instantly by this method, and this certainly is an interesting and important fact of considerable practical value to consumers of costly oils.

It is stated in text books on oil analyses, and also in elaborate works on oil refining, that methods of chemical treatment of mineral oil have been discovered to "de-bloom" mineral oil so that it can be used with impunity, so far as the bloom is concerned, as an adulterant for expensive vegetable and animal oils, and I learned that there is a very large trade in de-bloomed oils for this purpose.* It was of course at once evident to me that my new process would be totally incapable of detecting adulteration of non-fluorescent oils with mineral oil when deprived of all bloom or fluorescence. Accordingly, I obtained samples of de-bloomed oils of different grades and colors. These samples are free from bloom in bright sunlight or ordinary diffused daylight, or in the light from an ordinary electric arc, but, when subjected to the kind of light which I shall presently describe *they all became highly fluorescent.* When I stated to a certain refining company that their samples of de-bloomed oil were far from bloomless, they expressed the greatest astonishment; they said there must have been some inexplicable error in selecting the samples and that they would forward new samples guaranteed to be "absolutely bloomless." The new bottles not only had the printed "de-bloomed" labels, but they had other labels with the word "bloomless" written on them. These samples were precisely the same as the others, all showing the same fluorescence under the ultra-violet light illumination.

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* "There are a large number of so-called *de-bloomed* (mineral) oils on the market, and it is of the utmost importance that engineers should guard against their use. These oils are made for the sole purpose of enabling unprincipled lubricant manufacturers (especially middlemen) to sophisticate fatty oils with a cheaper mineral oil.

"As the de-blooming of a mineral oil can only be accomplished by treating the oil with nitric acid or a nitro-compound such as nitro-naphthaline, nitro-toluole, etc., it stands to reason that the lubrication of machinery by a de-bloomed oil means ruination to the machinery."—I. I. Redwood, *Lubricants, Oils and Greases*, p. 5.

Makers of de-bloomed mineral oils claim that prolonged exposure to sunlight will remove bloom without chemical treatment. Several specimens of this sun-bleached de-bloomed oil were tested and all showed marked fluorescence in the ultra-violet light, but not in daylight.

I then prepared a new series of standards for comparison, made with this so-called bloomless oil, so that it is possible to readily state the fact and even the proportion of adulteration with de-bloomed mineral oil in any specimen of non-fluorescent oil mixed with such de-bloomed mineral oil. I anticipate that this positive statement now made for the first time, will cause some consternation among makers of de-bloomed mineral oil.

If both mineral oil and resin oil be used in combination as adulterants, it becomes more difficult to make quantitative determinations instantly by the fluorescent method; hence the qualification implied in the word "often." But "practice makes perfect" in many operations and this is no exception to the rule.

It might naturally be supposed, in a method of comparison of this nature, that it would be necessary for all of the test tubes or phials used to contain the standards and the samples to be tested to be of absolutely uniform size; also that precisely the same quantities of oils should be used in all of the tests; such, however, is not the case. I soon noticed that the color of the fluorescence of any sample of mineral or resin oil was not changed by differences in the quantity of oil examined or in the dimensions of the bottles containing the specimens, differing in this respect, from the marked change in color of the same samples viewed by transmitted light in glass containers of different sizes. To illustrate this interesting observation I have prepared exceedingly thin films of mineral and resin oils (scarcely thicker than soap bubble films) by pressing a few drops of each between plates of clear glass and mounting them like lantern slides. Crude mineral oil and resin oil are very dark in color and are opaque in four ounce bottles, but the films are so transparent as to be almost invisible when viewed by transmitted light of any kind. When placed against a black background, however, and examined by reflected light from the source I employ, they fluoresce brilliantly in the same green and blue colors that large quantities of the same oils show under the same light.

The apparatus generally used by scientists for studying fluorescence is quite elaborate and costly, consisting of quartz prisms and lenses mounted in a spectroscope and requiring highly trained observers; but, fortunately, for the practical use and value of this new method in industrial works, I have found, ready at hand (in every establishment probably) a source of light which is peculiarly adapted to the purpose, so that no special appliances and no highly skilled operators are needed.

In the works with which I am connected we have in daily use many kinds of electric lights, such as incandescent filaments of carbon, tungsten, etc., Cooper-Hewitt mercury vapor tubes, ordinary electric-arcs, flaming-arcs and enclosed arc-lights. It is one of these forms that I have found not only best adapted to and most convenient for the purpose, but actually far more effective than any of the costly outfits for studying fluorescence that I have examined in one of the best appointed physical laboratories in the country.

Incidentally I may say that I have asked a number of scientists to guess which one of the forms of electric light named is the one most likely to be found suitable for this purpose. All but one said the "Cooper-Hewitt mercury-vapor light," the other said "the flaming-arc." They were wrong; it is the *ordinary enclosed arc*, so commonly used in industrial works by reason of its relative economy, that happens to give out rays of the exact wave lengths needed to enormously increase the fluorescence of these oils. If the plain glass cover of this light fits properly, so that air does not enter as rapidly as it is consumed, the arc burns in a partial vacuum or, at least, the air is rarified and, under these normal conditions, this light shows continuously, after burning a minute, a faint rosy light in addition to the powerful white light. If now a vessel containing any mineral oil, crude or refined, or any resin oil, be placed in the path of these rays the most intense fluorescence appears, even in daylight, greenish in the case of mineral oil, blue in the case of resin oil, the thin films already mentioned glowing in the same manner. So strong is this fluorescence that if one cubic centimeter of either mineral oil or resin oil be diffused in a bottle containing ninety-nine cubic centimeters of linseed oil, or any non-fluorescent oil, its presence is plainly seen; and I have even detected one cubic centimeter of crude mineral oil in nine hundred and ninety-nine cubic centimeters of non-fluorescent oil.

I have examined a large number of vegetable oils, such as cotton-seed oil, corn oil, China bean oil, China wood oil, etc.; and have not found a trace of fluorescence in any of them. It is stated in some text-books that "oleic acid," which is found in lard oil, is fluorescent. On examination I find that pure white strained lard oil is entirely free from fluorescence under the ultra-violet ray, but all of the samples of so-called No. 1 or No. 2 lard oil (sold for use in machine shops) examined possess some fluorescence, and this may prove to be a novel means of rapidly determining the proportion of oleic acid in lard oil, though I only suggest it tentatively, not having studied the matter carefully from this point of view. The slight fluorescence of ordinary lard oil is different in appearance from that of mineral oil or resin oil, and does not materially interfere with the application of the fluorescent test for its adulteration with mineral or resin oil.

There is a metal-cutting compound sold called "mineral lard oil"—there is no deception here, for its name proclaims its composition. The proportion of mineral oil in this compound is sufficient to cause intense fluorescence when examined under the rays of the enclosed arc.*

In order to make a quantitative fluorescent oil analysis—if I may be permitted to coin such a term—in cases where the amount of mineral or resin oil in vegetable or animal oil is over ten per cent., causing too great intensity of fluorescence for accurate quantitative determinations, it is simply necessary to dilute the sample to any desired degree for the test by adding sufficient pure vegetable or animal oil, as the case may be, to bring the proportion of adulterant within that of the prepared standards. Thus, if the sample of adulterated oil showing more intense fluorescence than the ten-per cent standard is diluted with an equal quantity of pure non-fluorescent oil and then shows a degree of fluorescence corresponding with the ten-per cent. standard, it is safe to conclude that it contains twenty per cent. of fluorescent adulterant.

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* *Mineral Lubricants.*—There are a number of compounds on the market sold under various names which are more or less transparent and of a jelly-like or gelatinous consistency. These are nothing more nor less than stearates, palmitates, or oleates of alumina mixed with a certain proportion of mineral oil. Such lubricants are bad for the reasons that: First, They have a strong tendency to separate into thick jelly and free mineral oil, the former of which will choke the oil-holes and prevent lubrication; second, owing to the uncertainty of the feed, the bearings are liable to become overheated; and, third, heated bearings will liquefy the jelly and in that condition it has little or no lubricating quality.—"*Lubricants, Oils and Greases,*" I. I. Redwood.

In daily practice I have found it convenient to put the standards in narrow tubular oil test bottles holding about fifty cubic centimeters each; these are corked, labeled, and placed side by side in small wooden racks (like test-tube holders) on a shelf in proximity to an enclosed arc-light, beginning with pure oil at the left-hand side, then a similar sample containing one-tenth per cent. of mineral or resin oil, as the case may be, then one per cent., and so on, increasing by single percentages up to ten per cent. It is advisable to prepare several different series of standards with fluorescent oils of different grades. Crude mineral or resin oils are much darker in color than refined oils, and the color by transmitted light is a guide to the kind of oil that has been used for adulteration and is consequently an indication of the proper standard series to be used for comparison in making a quantitive fluorescent analysis.

It is not necessary to prepare standards for each kind of vegetable or animal oil; thus, the standard series prepared with linseed oil serves for examination of cotton-seed oil, corn oil, China wood oil, China bean oil, or any other non-fluorescent vegetable oil. It is necessary, however, to prepare special standards with lard oil for testing adulterated lard oils.

The ingenious inventor of oil sand cores (now so largely superseding cores made by the old methods) conferred a valuable boon to foundries, but his name unfortunately is unknown. Probably the process was, at first, guarded as a trade secret, but such secrets cannot be kept inviolate for any length of time; very recently I have experienced this. Two novel processes were devised in the works with which I am connected, and before they had been in practical daily use for one month knowledge regarding them was conveyed by workmen to another establishment and doubtless the same will be repeated elsewhere, so that in a brief time both methods will be common property. Process patents are rarely of monetary value, as they are easily evaded. The making of oil sand cores has grown enormously in recent years and many thousands of dollars are spent annually by large concerns for oils for this purpose. The compounding of core oils has become a large business and nearly all samples that have come to my notice contain mineral or resin oil or both. Neither of these oils impart any valuable properties to core oils, but are used simply to dilute more costly oils; and, in point of fact, they are positively deleterious, being of a non-drying nature, impairing the good oil binder and requiring more fuel and a longer time for baking the cores in the ovens. When we realize that linseed oil (which is the best binder) costs, at the present time, in the neighborhood of one dollar per gallon and crude mineral oil about three cents per gallon, its use as an adulterant is readily explained. Resin oil costs a good deal more and is therefore used more sparingly.*

I have found that "Soya" oil expressed from beans grown in enormous quantities in China and elsewhere, is an excellent substitute for linseed oil for making cores if used in its natural state, without having been compounded or adulterated by core-oil makers. It costs about sixty cents per gallon for fine grades. The very best substitute for linseed oil as a binder for oil cores that I have discovered is crude whale oil, costing about the same as Soya oil, the only objection to its use being an unpleasant fishy smell which escapes from the core ovens during the baking of cores. It makes a splendid binder. I am told it is now being de-odorized, but I doubt the effectiveness of any such treatment. Cotton-seed oil is used for the same purpose, but so much larger proportion of oil to sand is required that there is little economy in its use as compared with the other vegetable oils.

A simple and practical test of the value of core oils is to make a dozen companion test cores 1 by 1 by 15 ins. from batches of pure linseed oil and sharp sand, and also from the same proportions of any other oil and sharp sand. These are placed side by side on an iron plate and baked under precisely the same heat

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*The price of linseed oil has been doubled in the past few years. Recent statistics show that in the United States alone over twenty-five million bushels of flaxseed are consumed annually in the manufacture of linseed oil, the bulk of which is used for paint. To produce so much seed requires from two million to two and one-half million acres under cultivation.

LEGEND TO PLATE V.

Description of colored plate showing twenty-four samples of vegetable, mineral and animal oils by reflected light from rays of an enclosed arc lamp using direct current of 220 volts. Number 1 to 18, inclusive, show only orange yellow color in daylight, similar to samples numbered 1 and 7 which are not fluorescent oils.

SERIES A. LINSEED OIL.

1. Linseed oil (pure), shows no fluorescence. Unchanged by ultra-violet rays.
2. " " containing 0.1% mineral oil. Greenish-blue fluorescence.
3. " " " 1.0 " " " " "
4. " " " 3.0 " " " " "
5. " " " 5.0 " " " " "
6. " " " 10.0 " " " " "

SERIES B. SOYA BEAN OIL.

7. Soya bean oil (pure), shows no fluorescence. Unchanged by ultra-violet rays.
8. " " " containing 0.1% resin oil. Pure blue fluorescence.
9. " " " " 1.0 " " " " "
10. " " " " 3.0 " " " " "
11. " " " " 5.0 " " " " "
12. " " " " 10.0 " " " " "

SERIES C. LINSEED OIL.

13. Linseed oil, containing 1.0% de-bloomed mineral oil. Violet-blue fluorescence.
14. " " " 3.0 " " " " " "
15. " " " 5.0 " " " " " "
16. " " " 10.0 " " " " " "
17. " " " 15.0 " " " " " "
18. " " " 20.00 " " " " " "

SERIES D. MISCELLANEOUS OILS.

19. Cylinder oil.	Ruby-red color by transmitted light.
20. De-bloomed mineral oil.	Orange-yellow color by transmitted light.
21. Kerosene.*	Water-white color by transmitted light.
22. Gasoline.	Shows no fluorescence.
23. Resin oil.	Ruby-red color by transmitted light.
24. Ordinary No. 1 lard oil.	Pale yellow color by transmitted light.

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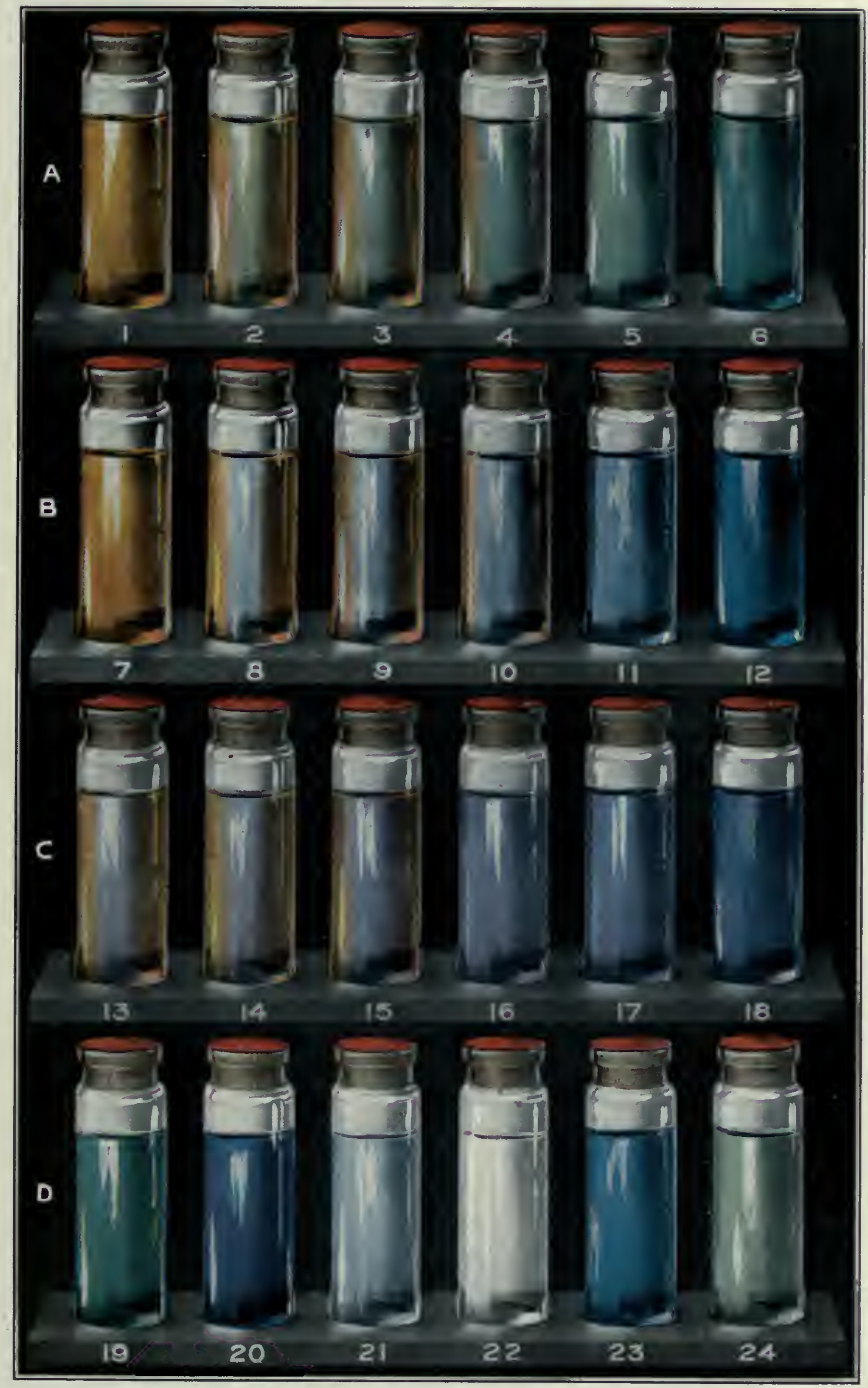
* Owing to the comparatively faint degree of fluorescence of kerosene under the rays of the enclosed arc lamp, this light will not readily detect kerosene when present as an adulterant in small amount in any non-fluorescent oil. The more highly volatile products of petroleum, such as petroleum-ether, are not fluorescent at all.

PLATE V.
PROC. AM. SOC. TEST. MATS.
VOL. XI.
OUTERBRIDGE ON FLUORESCENT TESTS.



conditions. When cold they are broken on a transverse testing machine with supports 12 ins, apart. The relation between the average strength of the two sets of test cores is a measure of the binding qualities of the oils.

The prime motive of this paper has been to present to the American Society for Testing Materials a new and practical method, suitable for daily use in industrial works of instantly detecting, without chemical analysis or highly skilled operatives, the presence, and, in many instances, the actual proportion of cheap mineral or resin oil adulterants in costly vegetable and animal oils now used in great quantities in such establishments. In addition, I venture to think that perhaps this investigation in fluorescence of certain oils may open up a new field of scientific study of the long-disputed question of the true origin of mineral oil. Alexander Winchell, LL.D., said in one of his books on Geology: "Mineral oil is chiefly the product of the distillation of shales charged with vegetable matter, probably ancient sea-weeds." If such be the case should we not expect to find in mineral oil a trace, at least, of bromine and iodine, since these are invariably components of sea-weed? As mineral oil and resin oil fluoresce with equal intensity (unlike any other oils) under excitation of rays of light having the same wave lengths, and as they resemble each other also in their chemical constitution and molecular structure, is it not at least permissible to speculate, in advance of investigation along these lines, on the probability of a uniform origin? Mineral oils and resin oils are both hydrocarbon oils, the main difference between them being that resin oils are partially oxidized hydrocarbons, and are to a certain degree saponifiable, while mineral oils are non-oxidizable hydrocarbons and are not saponifiable.

Chemists will, I believe, eventually solve the problem that has hitherto baffled all their endeavors, of oxidizing hydrocarbon or mineral oils and of rendering them saponifiable. The monetary value of such a discovery is incalculable and the solver of the riddle would at once rank among great discoverers.*

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* *Bulletin No. 2*, Vol. 7, 1910, of the Bureau of Standards of the Department of Commerce and Labor, Washington, D. C., entitled "The Action of Sunlight and Air upon some Lubricating Oils," states as follows: "In 1885 Schaal patented a process for the manufacture of soaps by the action of air upon mineral oils in the presence of alkaline substances. In 1891 Zaloziecki studied the oxidation of petroleum by means of air. By blowing cold air through the oil the oxidation was slight, but was greatly increased in the presence of sodium hydroxide. Hirsh passed air through mineral oils in the presence of soda lye and even after

After careful search through literature on the subject of light, and after extended correspondence with scientific investigators of fluorescence and cognate studies, I have been unable to find any precedent for guidance in proposing such a scheme for practical use in the instantaneous detection, without employing chemical means or expert knowledge of hydrocarbon oils in other oils and of making quantitative determinations by means of fluorescence under illumination with ultra-violet rays.

In opening up a new path in any hitherto unexplored region new vistas usually present themselves at unexpected moments, and several alluring by-paths have already been partially revealed in the course of this investigation which may lead to furthur interesting developments in the future.

In conclusion, I venture to express the hope and belief that the process here described will prove to be all that is claimed for it, and applicable to all manufacturing industries using vegetable and animal oils for sundry purposes; that it will even prove helpful in analytical laboratories; that it may serve to discourage the making of de-bloomed oils for purposes of adulterating other oils; and that it may be a worthy contribution to applied science by one whose somewhat lengthy professional life, extending over forty years, has been mainly occupied in experimental work in large industrial establishments.

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twelve hours at 70° could detect no change in the alkali, concluding that there was no oxidation."

Tests are recorded in this Bulletin showing the action of sunlight and air on mineral oils, extending over periods of many months, which were made in the Bureau of Standards. It appears that the maximum amount of oxygen taken up by mineral oil under these conditions was less than 2 per cent.

There is no doubt that oxidation to a limited extent does occur under some conditions, although statements that appear from time to time in print regarding the discovery of new methods of saponifying mineral oil may be classed in the same category as perennial announcements of the discovery of perpetual motion; but, whereas the theory of perpetual motion is diametrically opposed to known physical laws and is, therefore, not logically conceivable, the problem of oxidation and saponification of mineral oil encounters no such theoretical objections. The obstacles in the way are due to the limitation of our knowledge of the molecular construction of mineral oil. We know the elements of which it is composed, but we do not know how they were originally united, or how they may be divorced and made to form new unions with other elements, such as oxygen, etc.

The extremely high temperatures now obtainable in electric furnaces and the modern methods of producing oxygen and ozone on commercial scale at comparatively low cost suggest new possibilities for research in the direction indicated, by subjecting mineral oil to highly oxidizing influences at very high temperatures and under varying pressures.

DISCUSSION

VICE-PRESIDENT R. W. LESLEY (*in the Chair*).—This has been a very interesting demonstration. I am sure that the Society ought to be very thankful to Mr. Outerbridge for having made it. It will doubtless prove useful to a great many of us. Is there any discussion on this presentation? **The Chairman.**

MR. S. S. VOORHEES.—I should like to ask Mr. Outerbridge if he finds much variation in the fluorescence in different types of mineral oils? **Mr. Voorhees.**

MR. A. E. OUTERBRIDGE, JR.—Only in connection with the degree of refining, as I have shown. The more highly oil is refined, the more transparent it becomes and the less fluorescent. Thus, kerosene is of a beautiful sky-blue color under this light while gasoline is not fluorescent at all. As a rule highly volatile products are not fluorescent. **Mr. Outerbridge.**

MR. P. H. WALKER.—Have you made any experiments by mixing turpentine and resin oil? **Mr. Walker.**

MR. OUTERBRIDGE.—Yes, it shows exactly the same thing that I have shown you. There is no fluorescence in turpentine. I have a sample here, containing 50 per cent. of resin oil and 50 per cent. of turpentine. I am quite sure I showed it. It is full of fluorescence owing to the resin oil. One per cent. of resin oil in turpentine is sufficient to impart fluorescence that is readily observable under the ultra-violet light rays. **Mr. Outerbridge.**

ORGANIC RESIDUES FROM SOLUBLE BITUMEN DETERMINATIONS: SULPHUR IN TAR RESIDUES.

By Prévost Hubbard and C. S. Reeve.

In a recent paper presented before this Society,* the authors discussed various methods for the determination of soluble bitumen with reference to comparative data obtained upon six types of bituminous materials. As a result of this work, one of the methods was tentatively selected for routine work in the laboratories of the United States Office of Public Roads. In this method, which will be later described in detail, the bitumen is first digested for 15 minutes in carbon disulphide at air temperature and the solution then filtered through a Gooch crucible fitted with an asbestos filter. Another method which also looked promising was quite similar, the only difference being that the bitumen was first digested for a short time in boiling carbon disulphide before filtering.

The object of the work presented in this paper is, first, to study the composition of insoluble organic residues in bitumens, and, second, from such study to secure data which might throw further light on the relative value of the two methods previously mentioned. In this work, which is merely preliminary to future investigations, it was decided to study the insoluble organic residues from coal tars, in preference to other bitumens, for the following reasons:

First: This class of bitumens shows the greatest variation in the percentage of organic residue obtained by different methods.

Second: These residues are practically free from inorganic material.

Third: A large amount of residue for investigation can be obtained from a single determination.

The insoluble organic matter in tars is commonly termed "free carbon," although it has long been known that other constituents are also present in relatively small amounts. As the percentage of so-called free carbon obtained from a given tar is found to vary with the solvent used and the method of extraction, it seemed reasonable to suppose that such residues would naturally vary somewhat in composition; also that the composition of residues obtained from different tars might be expected to differ even when obtained according to the same method.

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* "The Determination of Soluble Bitumen," *Proceedings*, Vol. X, 1910, pp. 420–431.

Supporting the first assumption Lunge* states that "Behrens,† by digesting hard pitch, freed from all oils up to specific gravity 1.120, successively with cold benzene, carbon disulphide, boiling benzene, and boiling alcohol, obtained 23.54 per cent. of a black powder, containing 90.836 per cent. carbon, 3.058 per cent. hydrogen, and 0.398 per cent. ash; when treated somewhat differently, the residue yielded 91.921 carbon, 3.157 hydrogen and 0.872 ash." Donath and Asriel‡ found that the residue obtained by extracting hard pitch with petroleum spirit, benzol, and carbon disulphide, contained 89.2 carbon, 2.3 hydrogen, 0.7 nitrogen, 7.13 oxygen and 0.67 ash.

Residues obtained by the authors from a refined coal tar according to the flask methods hereafter described, in which benzol, and carbon disulphide both cold and boiling, were employed as solvents for varying periods of digestion, gave the following results upon analysis:

Carbon	varying	from	90.17	to	94.26	per	cent.
Hydrogen	"	"	2.59	"	3.31	"	"
Oxygen	"	"	1.81	"	5.91	"	"
Sulphur	"	"	0.50	"	1.78	"	"
Nitrogen	no trace upon a qualitative test.						

As the estimation of sulphur in organic tar residues offered an apparently new field of investigation, it was decided to first concentrate work upon this element. The results so obtained will be discussed after the following descriptions of the methods of securing the organic residues which were examined and the method of determining the sulphur content of these residues.

Cold Method for Determining Insoluble Organic Residues.—A 2-gram sample of the bitumen was weighed in a 100-cc. Erlenmeyer flask which had been previously tared. It was dissolved by the slow addition of 100 cc. of solvent, with constant twirling until the material was entirely broken up, after which the flask was set aside for 15 minutes. At the end of that time the solution was filtered through a felt of long-fiber amphibole asbestos, contained in a large platinum Gooch crucible which had been previously ignited and weighed. All the insoluble matter was washed on to the felt by the solvent and the flask scrubbed with a chicken feather. A slight suction was applied when necessary, and the contents of the crucible washed until the washings came through practically colorless. The crucible and contents were dried in a hot-air oven at about 100° C., cooled, and weighed.

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* "Coal Tar and Ammonia," Fourth Edition, Part 1, p. 431. (D. Van Nostrand Company.)

† *Dingler's Polytechnisches Journal*, Vol. CCVIII, p. 369.

‡ *Chemisches Central-Blatt*, 1903, i., 1099.

Hot Method for Determining Insoluble Organic Residues.—A 2-gram sample of the bitumen was weighed in a 100-cc. Erlenmeyer flask as before, and broken up by the addition of 50 cc. of solvent. The flask was then connected to a short spiral reflux condenser and placed over a steam bath, where the solution was maintained at its boiling point for 15 minutes. The flask was then disconnected and the solution filtered, the procedure being exactly the same as in the cold method.

Determination of Sulphur.—The sulphur determinations were made in the apparatus designed by Dr. Graefe. The method is based upon the combustion of the material in an atmosphere of oxygen, the absorption and oxidation of the sulphur products to the sulphate form in an aqueous solution of sodium peroxide, and the determination of the sulphur as barium sulphate. The combustion is made in a glass bottle of about 8 liters capacity, fitted with a rubber stopper carrying a 100-cc. short-stem separatory funnel. Two copper wires about $\frac{1}{16}$ in. in diameter pierce the stopper, one on each side of the funnel. One of these wires extends into the bottle two-thirds of the distance to the bottom, the other about one-half. The end of the longer wire is bent at right angles into a circle to hold a small perforated platinum cone. A piece of platinum foil is soldered around the end of the shorter copper wire, and another at a point opposite on the longer, to make good contact with a thin platinum wire 0.1 to 0.2-mm. in diameter which connects the two. The rubber stopper is protected at the bottom by a disk of asbestos board having perforations for the funnel and copper wires.

In preparing for the determination, the bottle was first filled with tap water which was then displaced by oxygen from an oxygen tank. Any impurities remaining from the tap water were rinsed out by a couple of washings with a small amount of distilled water.

The sample of organic residue for combustion was taken from the Gooch crucible in small pieces as free as possible from asbestos and placed in the weighed platinum cone. From 0.2 to 0.3 gram was used for a determination. The cone with contents was placed in the ring of copper wire and a strand of yarn was run from the platinum wire into the sample. The sample was then lowered into the bottle of oxygen and a brass clamp placed over the rubber stopper to hold it firmly in place.

On applying the poles of a dry battery to the copper wires the yarn ignited, producing ignition of the sample, and complete combustion of the organic material followed. The bottle was cooled by pouring ice water over its sides, after which 50 cc. of a 10-per cent. solution of sodium peroxide in ice-cold distilled water was run in through the separatory funnel. The bottle was allowed to stand for about an hour, or until all vapors had settled, after which the contents were washed out into a beaker, acidified with hydrochloric acid, and the determination of sulphur made by precipitation with barium chloride. The asbestos remaining in the cone was dried at 105° C., cooled and weighed, and its weight deducted from that of the original amount taken, thus giving the exact weight of the organic matter actually consumed.

TABLE I.—THE SULPHUR CONTENT OF ORGANIC RESIDUES OBTAINED FROM TAR SAMPLE 3898 WITH CARBON DISULPHIDE.

Digestion.	Insoluble Organic Matter, per cent.	Sulphur in Insoluble Organic Matter, per cent.			Sulphur in Residue, per cent. of original tar.
		1	2	Average.	
15 minutes, cold. . . .	22.22	0.54	0.59	0.57	0.13
30 days, "	26.63	1.78	. . . .	1.78	0.47
15 minutes, hot.	21.42	0.79	. . . .	0.79	0.17
3 hours, "	21.24	1.10	1.03	1.07	0.23
24 " "	. . . . .	1.28	. . . .	1.28	. . . .
32 " "	23.17	1.48	. . . .	1.48	0.34

Before beginning the work, blank tests were run with the reagents and cotton yarn, and no trace of sulphur was found.

The first tar to be examined was a very viscous refined product which was found to contain 0.51 per cent. of sulphur. A 15-minute cold extraction with carbon disulphide made in the manner above described yielded an organic residue of 22.22 per cent. This residue was found to contain 0.57 per cent. of sulphur, which, calculated on the basis of the original tar as 100, represents 0.13 per cent. Upon different periods of digestion with cold and hot carbon disulphide, the percentage of organic residue was found to vary considerably, as was to be expected from the work described in our former paper. Moreover, the composition of these residues was found to vary in so far as their sulphur content is concerned. These results are given in Table I and indicate that where carbon disulphide is used as a solvent, the percentage of sulphur in the residues increases with the length of digestion and the temperature of the solvent. That this increase in the percentage of sulphur in the residue has no direct relation to the percentage of residue obtained, is evidenced by the figures in the last column. It will be noted that in the 30-day cold extraction nearly the same amount of sulphur found in the original tar appeared in the insoluble residue, the percentage based upon the tar as 100 being 0.47 as against 0.51 per cent. originally found. This led us to suspect that upon long digestion or heating, either the total sulphur in the tar was precipitated as organic sulphur compounds, or that a reaction between the solvent and the tar might have taken place with the formation of insoluble compounds containing sulphur.

TABLE II.—THE SULPHUR CONTENT OF ORGANIC RESIDUES OBTAINED FROM TAR SAMPLE 3898 WITH BENZOL.

Digestion.	Insoluble Organic Matter, per cent.	Sulphur in Insoluble Organic Matter, per cent.			Sulphur in Residue, per cent. of original tar.
		1	2	Average.	
15 minutes, cold....	25.60	0.51	0.48	0.50	0.13
30 days, "	28.65	0.64	....	0.64	0.18
3 hours, hot......	23.95	0.51	0.54	0.53	0.13

To shed some light on this theory it was decided to make use of a solvent containing no sulphur in its composition. Chemically pure benzol seemed well adapted for the work and a number of extractions were therefore made with this solvent. The results obtained are given in Table II. It will be noted that the residue from the 15-minute cold digestion with benzol shows a slightly lower percentage of sulphur than that from the 15-minute cold digestion with carbon disulphide. The percentage of residue from the former was, however, about 3.4 per cent. higher, so that the sulphur in the residue calculated on the basis of the original tar amounts to 0.13 per cent., which is exactly the same as that obtained in like manner from the 15-minute cold digestion with carbon disulphide. This indicates that a short cold digestion with carbon disulphide produces no reaction with the formation of sulphur compounds. The 30-day cold digestion with benzol showed only a very slight increase in sulphur and the 3-hour hot digestion none at all, which is quite the reverse of the results obtained with carbon disulphide for a long cold digestion or a short hot one. The superiority of carbon disulphide as a solvent for each comparative run is, however, very evident.

TABLE III.—THE SULPHUR CONTENT OF ORGANIC RESIDUES OBTAINED FROM TARS WITH CARBON DISULPHIDE.

Tar Sample No.	Digestion.	Insoluble Organic Matter, per cent.	Sulphur in Insoluble Organic Matter, per cent.			Sulphur in Residue, per cent. of original tar.
			1	2	Average.	
3898....	15 minutes, cold..	22.22 22.39	0.54	0.59	0.57	0.13
	15 " hot...	21.24	0.79	....	0.79	0.17
4332....	15 minutes, cold..	28.76 28.96	0.75	0.78	0.77	0.22
	15 " hot...	28.62 28.48	1.06	1.02	1.04	0.30
4897....	15 minutes, cold..	14.79	0.95 0.89 0.82	0.91 0.89 0.96	0.90	0.13
	15 " hot...	14.05	1.07 1.00	0.91 1.03	1.00	0.14

The fact that a 15-minute hot digestion with carbon disulphide gave a residue with a slightly higher percentage of sulphur, on the basis of the original tar, than the 15-minute cold digestion, made it desirable to ascertain if the same relation would hold true for other tars. Two other refined road tars were therefore run according to both methods, with the results shown in Table III. From the last column in this table it will be seen that the same tendency occurs, although the difference in Sample 4897 is so small as to be negligible.

Without commenting further on the work so far accomplished, the authors wish to state that these investigations will be continued on other types of bitumen, not only with regard to the sulphur content of organic residues, but also along the line of determining their ultimate composition. Certain weathering tests on different types of bitumen show that the percentage of insoluble organic matter increases with the age of the material, and a study of the composition of such products offers an interesting field which is now being investigated. The results of these investigations will be published in a later paper.

IMPROVED INSTRUMENTS FOR THE PHYSICAL TESTING OF BITUMINOUS MATERIALS.*

BY HERBERT ABRAHAM.

In the preceding papers instruments were described for determining the fusing point (fusibility), hardness (consistency), tensile strength (tenacity), and ductility of bituminous substances. The instrument for testing the fusing point has been termed the "Fuse Meter," that for determining the hardness the "Consistometer," and the instrument for determining the tensile strength and ductility the "Tensometer."

These three instruments have been developed further in the past year, and tests have been made on a large number of commercial bituminous substances in order to study the above physical characteristics. In this paper the modified types of instruments will be first briefly described. A number of typical tests will follow, together with a discussion of the results.

Fuse Meter.—The improved instrument for determining the fusing point is illustrated in Fig. 1. The apparatus has been modified in one or two minor respects since the original article was published. The several parts are now enclosed in a wooden case equipped with a counterweighted sliding glass door, not shown in the illustration.

The dial in the center of the figure is graduated in degrees Fahrenheit, and is fastened to an electric clock which is connected with a metronome interrupter shown in the lower left compartment. The metronome is adjusted to beat seconds, so that one revolution of the minute hand on the dial will take place in 60 seconds, recording a rise of 4° F. This corresponds to the rate of 1° F. every 15 seconds. The hour hand records the exact temperature on the inner portion of the dial, which is graduated from 0° to 240° F. The electric clock is operated by two sets of dry batteries contained in the second compartment, which is shown closed. The batteries may be thrown in by means of the two-pole switch located directly underneath the dial. The third and lower right-hand compartment is used for storing the fusing-point tubes and for collecting the mercury at the termination of the test, which is poured through the small chute shown directly beneath the measuring tube at the right-hand side of the instrument.

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* This is the third paper on this subject by the author. See papers of the same title, *Proceedings*, Vol. IX, 1909, pp. 568–579; Vol. X, 1910, pp. 444–451.

The construction of the heating coil is exactly the same as described in the original paper.

For substances fusing at temperatures higher than 212° F., a different arrangement must be used, as illustrated in Fig. 2. The water in the previous test is replaced with castor oil. This has been found to be much more satisfactory than paraffin oil, which was originally recommended for this purpose, for it does not exert a marked solvent action on the harder forms of bituminous materials at high temperatures, and neither vaporizes nor darkens appreciably, as was the case with paraffin oil. Castor oil may be heated safely up to about 600° F. This temperature may seem high, but the author has examined a number of so-called "asphaltites" which fused at as high as 580° F.

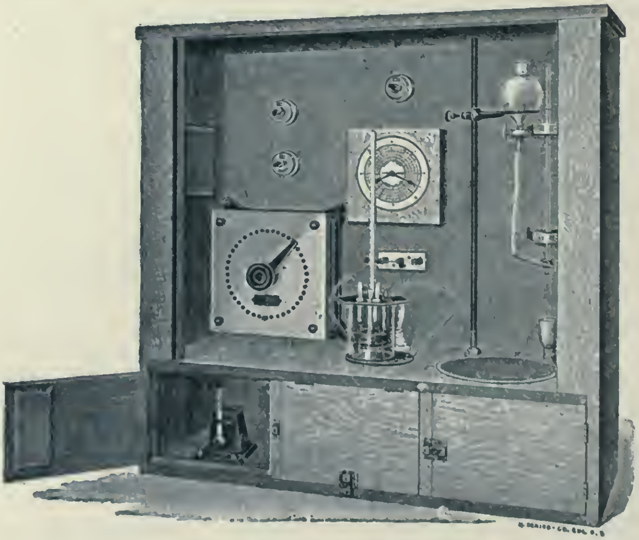

FIG. 1.—Fuse Meter.

The method of procedure consists in powdering a small quantity of the high-fusing-point bitumen, and compressing some of the powder in the lower end of the glass fusing-point tube. The tube is then heated carefully above the flame of a burner until the plug of bitumen softens, which is evidenced by the color changing from a dull to a glossy black. The tube is then stood upright and held firmly against a block of wood, and a snug-fitting glass rod inserted in the upper end of the tube and pressed against the plug of softened bitumen, so as to compact it into a solid mass, which should then measure about 7 to 9 mm. in length. On cooling, the bitumen is carefully scraped from the lower portion of the tube, leaving a plug about 5 mm. long and 2 to 4 mm. above the lower opening. Five grams of mercury are then introduced on top of the plug of bituminous material in the usual manner. On suspending the fusing-point tubes in the heating bath of castor oil, the free space below the plug of pitch will remain filled with air. This will remove any likelihood of the oil coming in contact and softening the bituminous material.

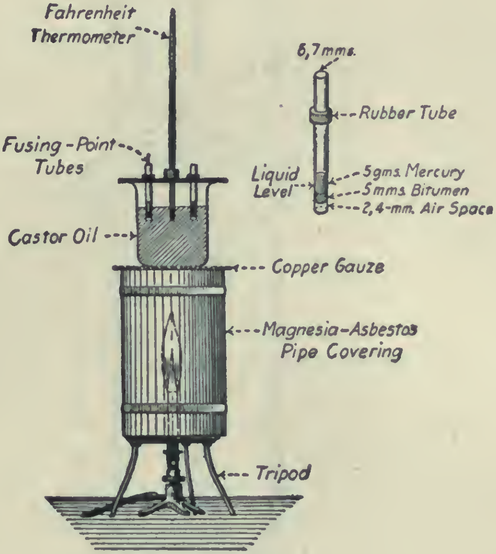

FIG. 2.

The beaker is stood on a piece of copper gauze placed on a section of magnesia asbestos pipe covering about 20 cm. in length and 10 cm. internal diameter. The heating is done with a Bunsen burner placed 15 cm. away from the bottom of the beaker; the flame is carefully controlled so that the temperature will rise exactly 1° F. every 15 seconds, as determined by the rings of a metronome.

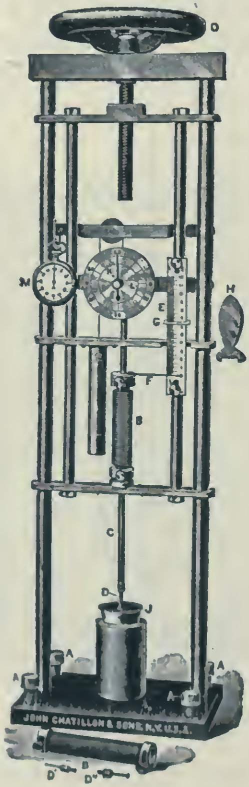

FIG. 3.—Consistometer.

The heating is thus effected by means of a current of hot air without allowing the flame to come in contact with the beaker. This insures safety, and will permit the temperature to be controlled quite readily.

Consistometer.—This instrument has been improved and simplified, and is shown in Fig. 3. It was found that the internal friction of the earlier form of consistometer entered as a larger disturbing factor than was realized at the time. In the new model, the internal friction has been almost entirely overcome.

The plungers have been constructed more carefully, so that the perimeters of the penetrating surfaces are virtually "knife-edges." This *entirely* eliminates the frictional adhesion of the bituminous material to the sides of the plungers, which was not found to be the case with the first set of plungers manufactured. With this new instrument, the relation between the three plungers is *exactly* 1 : 10 : 100, and this ratio remains constant irrespective of the material examined. In other words, for any individual bituminous substance, the number of grams required to force the plungers into the material at the uniform speed of 1 cm. per minute is directly proportional to the *volume* displaced. The volumes displaced by the plungers per minute are 0.01 cc., 0.10 cc., and 1.00 cc., respectively. Hence the relation 1 : 10 : 100.

For testing exceedingly soft bituminous materials, such as road oil and the like, a fourth plunger having an area of 1,000 sq. mm. (35.6 mm. in diameter), has been used to still further extend the range and sensitiveness of the instrument.

Two interchangeable springs are used with the consistometer, one for reading in grams on a scale ranging from 0 to 1,000 grams in 10-gram divisions, the other for reading in kilograms on a scale ranging from 0 to 10 kilograms in 0.1-kilogram divisions.

A new method has been devised for expressing numerically the hardness or consistency of the bituminous material. It is designated in every case as the *cube root* of the number of grams which must be applied to the 100-sq. mm. plunger to cause it to displace the substance at the speed of 1 cm. per minute. It will be recalled that in the first paper presented by the author, the hardness was designated as the *square root* of the number of grams applied to the 1-sq. mm. plunger. The new method, however, has an advantage over the old, in expressing the hardness figures on a more evenly balanced scale, which is uniformly carried to but one decimal place, and which can, therefore, be readily plotted on coordinate paper. It also magnifies to a greater extent the differences between the softer bituminous materials included in the lower portion of the numerical scale, as will appear from Table I.

TABLE I.

Plunger.	Spring.	Actual Consistometer Readings.	Readings converted to Grams per 1-sq. mm. Plunger.	Readings converted to Grams per 100-sq. mm. Plunger.*	Square Root of Grams per 1-sq. mm. Plunger.	Cube Root of Grams per 100-sq. mm. Plunger.
100 sq. mm.	Gram.	From 10 grams	0.1	10	0.32	2.1
		To 1000 "	10.0	1,000	3.16	10.0
	Kilogram.	From 1.0 kgs.	10	1,000	3.16	10.0
		To 10.0 "	100	10,000	10.00	21.5
10 sq. mm.	Kilogram.	From 1.0 "	100	10,000	10.0	21.5
		To 10.0 "	1,000	100,000	31.6	46.4
1 sq. mm.	Kilogram.	From 1.0 "	1,000	100,000	31.6	46.4
		To 10.0 "	10,000	1,000,000	100.0	100.0

A number of inquiries have been received regarding the details of the electrical thermostat used for maintaining the bath at a constant temperature. This arrangement is illustrated in Fig. 4, in which *A* represents a vessel filled with water containing the Fahrenheit thermometer *B*, the heating coil *C*, and the thermostat *D*. This thermostat is a Reichert mercury type in circuit with a dry battery *E* and a telegraphic relay *F*. One wire is attached to the adjusting screw *G*, and the other to a small platinum contact *H*, inserted through the upper opening of the thermostat. The heating coil is connected with a rheostat *I* and the relay *F*. An incandescent lamp *J*, shunted across the main circuit as shown, will indicate when the current is passing through the heating coil *C*.

When the mercury in the thermostat rises, closing the battery circuit, it operates the relay *F*, which shuts off the current from the heating coil *C*. This allows the bath to cool, causing the mercury in the thermostat to contract. When the contact at *H* is broken, the battery current is shut off the relay, and the main circuit is then closed by the action of the spring *K*. By adjusting the rheostat *I* and the set screw *G*, the temperature of the bath may be maintained at any desired degree.

Tensometer.—In the discussion of the author's paper at the last meeting of the Society, Mr. A. W. Dow suggested† that a mold having a minimum cross-section would probably give more uniform results than the one proposed originally by the author. Further experiments were conducted along these lines, and Mr. Dow's opinion was substantiated.

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*This is obtained by multiplying the number of grams per 10-sq. mm. plunger by 10, and the grams per 1-sq. mm. plunger by 100.

† *Proceedings*, Vol. X, 1910, p. 452.

The form of mold shown in Fig. 5 is accordingly submitted as an improvement over the former type. The dimensions over all are the same as before, and the specimen has a minimum cross-section of 1 sq. cm. (diameter, 1.128 cm.). As it is not necessary to amalgamate the inside of this mold with mercury, it is constructed of hardened steel instead of bronze, to avoid the possibility of the circular knife-edges being injured during use.

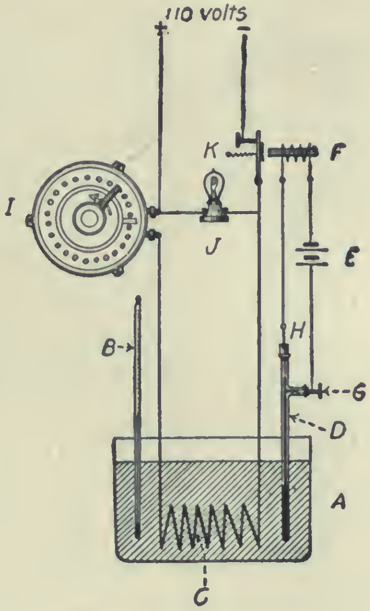

FIG. 4.

This new form of mold gives more uniform readings, for it tends to reduce the distance to which the bituminous material stretches. In the former type the stretching was due to the contraction of the cylindrical section of bituminous material, and it very often happened that the cylinder had a tendency to stick to or hug one side of the mold, and did not, therefore, draw out in the center. In the improved type the stretching is due to a cupping of the bituminous material around the circular knife-edges, and does not seem to be as largely influenced by the homogeneity of the material.

In making a test, the tension applied to the specimen is increased at the uniform rate of 5 kilograms per minute. At the moment the molds commence to separate, the cross-section of the specimen is reduced, and it becomes impossible to increase the tension beyond this point. When the molds begin to separate, they are drawn apart at the uniform speed of 5 cm. per minute until the thread of bituminous material parts.

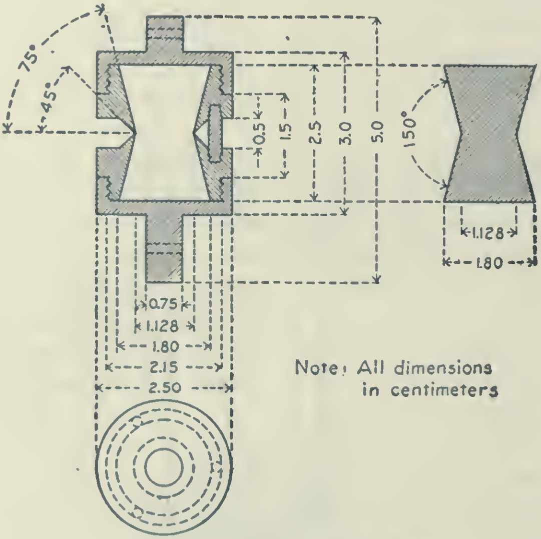

FIG. 5.—Tensometer Mold.

It is also necessary to take the precaution of filling the reservoir with a liquid of about the same specific gravity as the bituminous material tested, in order that the thread of bitumen will neither have a tendency to float nor sink while the molds are being drawn apart.

The accuracy of this ductility test is roughly as follows:

Ductility.	Accuracy.
0 to 10 cm	0.5 cm.
10 " 25 "	1 "
25 " 50 "	2.5 "
50 " 100 "	5 "

In other words, with this new form of mold the accuracy is about 5 per cent.

Tests.—The curves shown in Figs. 6, 7, and 8 will illustrate the effect of variations in temperature on the hardness, tensile strength, and ductility of several typical bitumens (fusing at about the same temperature), and will indicate how readily these properties may be represented graphically. The solid lines represent the hardness expressed as the cube root of the number of grams applied to the 100-sq. mm. plunger; the dotted lines represent the tensile strength in kilograms multiplied by ten; and the dashed lines represent the ductility in centimeters.

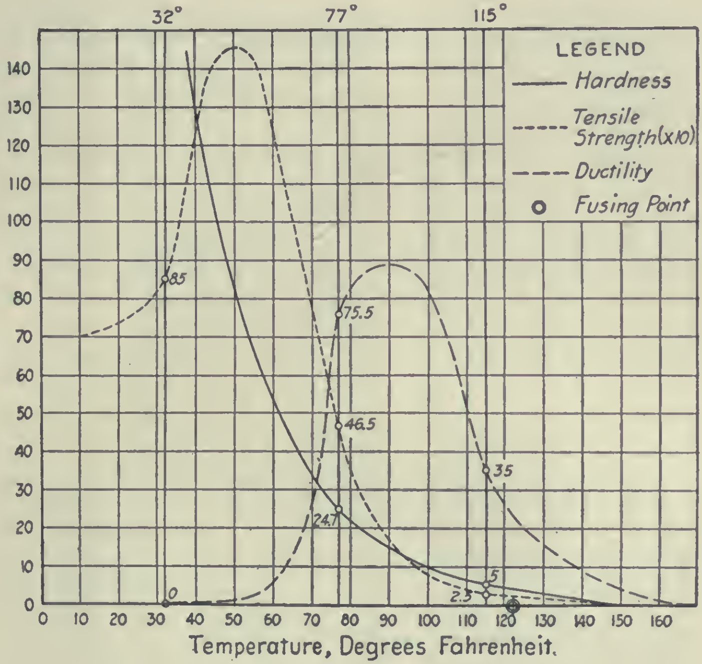

FIG. 6.—Coal-Tar Pitch. (Fusing Point, 122° F.)

Table II shows the results obtained upon testing a number of commercial bituminous substances, and a careful study of the table will be found very instructive.

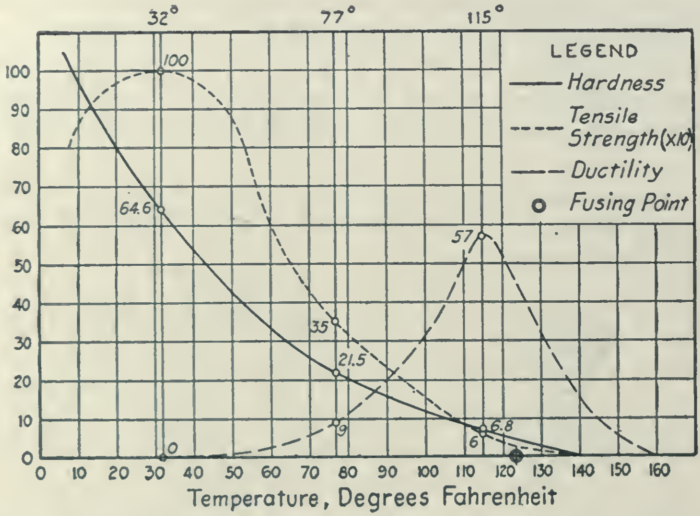

FIG. 7.—California Oil Asphalt, Grade D. (Fusing Point, 124° F.)

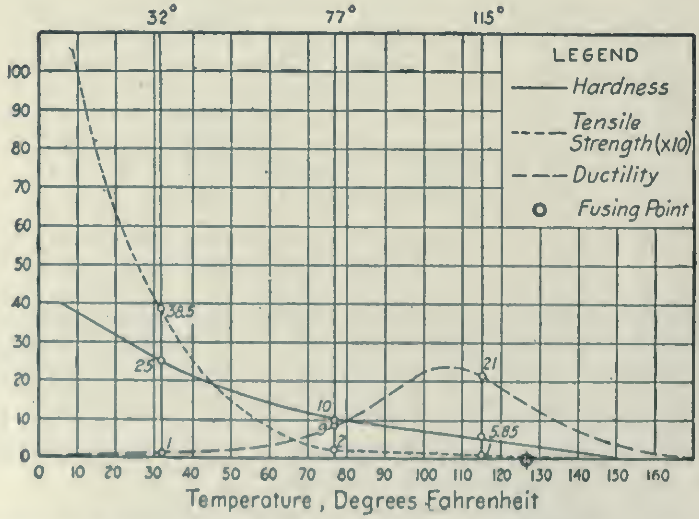

FIG. 8.—Blown Lima Asphalt, Grade 140. (Fusing Point, 127° F.)

General Observations.—1. The hardness or consistency of bituminous substances invariably tests between 2 and 5 units at their fusing point.

2. A new figure was obtained which promises to be of value in differentiating bitumens. This has been termed the "Susceptibility Factor" as listed in the table, and it expresses the susceptibility of bituminous substances to changes in temperature. We all know that some bitumens are more affected by temperature changes than others, but up to the present there has been no method of expressing this fact numerically. This, however, may be accomplished with the assistance of the consistometer, as follows:

$$\text{Susceptibility Factor} = \frac{(\text{Hd. at } 32^\circ \text{ F.}) - (\text{Hd. at } 115^\circ \text{ F.})}{\text{Fusing Point}} \times 100$$

A glance at the table will show that tar pitches have a susceptibility factor greater than 100, straight asphalts fall somewhere between 50 and 100, and blown asphalts and fatty acid pitches fall lower than 50. The susceptibility factor does not appear to be dependent upon the hardness or fusing point of the substance, but is fairly constant for all bituminous substances obtained from the same origin and produced by the same general process.

3. No relation appears to exist between the hardness and the tensile strength of bituminous materials. In the case of petroleum asphalts manfactured from the same crude, the tensile strength seems to be reduced after the distillation process is carried beyond a certain point. This applies only after the product has reached the hard, brittle stage. Excessive blowing seems to produce the same results, but to a lesser degree.

4. With bituminous substances obtained from the same source and manufactured by similar processes, there does not appear to be any relation between the ductility on one hand and the hardness or tensile strength on the other. In each bituminous substance, however, there is a certain temperature peculiar to itself, at which its ductility is at a maximum. This temperature does not appear to bear any definite relation to the fusing point of the substance. The ductility curve of any substance for different temperatures has about the same form as the probability curve of higher mathematics. A break usually occurs in the tensile strength curve at about the same temperature at which the ductility approaches zero. This is

probably due to the disappearance of plasticity and cohesive properties at low temperatures when the substance becomes transformed into a brittle solid.

It is, of course, desirable that the maximum ductility of bituminous substances should occur as close as possible to the average temperature to which the material will be subjected in actual practice. This, however, will depend largely upon the locality and the exposure in which the bituminous material is to be used.

It is entirely possible to prepare bituminous substances having a low susceptibility factor, and at the same time possessing relatively great tensile strength and high ductility. At the present time, substances of this description are not often encountered, as the trade does not usually demand them. It is predicted, however, that in the future more stress will be laid upon these three properties, and that the manufacturer will be compelled to take them into consideration in preparing his products.

[For Discussion of this paper see pages 693–698.—ED.]

A NEW CONSISTOMETER FOR USE IN TESTING BITUMINOUS ROAD MATERIALS.

By W. W. Crosby.

The writer hesitates under existing conditions to suggest a new machine for testing the consistency of bituminous road materials, but the endeavor to compare different results of the writer's work, or the latter with the work of others, has so frequently resulted in confusion that he is led to do so at this time from the belief that the suggestion may result in clarifying the present situation rather than in producing further confusion.

Unquestionably the determination of the physical character of a pitch or oil must include some definite expression of its mobility, resistance to displacement, consistency, or whatever the property may be called. The knowledge of this characteristic is important in considering such a substance when submitted or contemplated for use, and the definite expression of such a characteristic is absolutely necessary in drawing specifications. Further, the writer thinks it may be safely stated that the particulars of such a characteristic of many materials in terms of a definite instrument at one temperature, give but very little information as to what the same material will show in this regard with another instrument or at another temperature. It is perhaps even doubtful whether a direct relation always exists between the determinations of the same instrument at different temperatures.

An instrument of sufficient range to cover all materials at almost all temperatures would unquestionably be desirable, if indeed it is not necessary. The writer believes the instrument now to be described is such a one, for so far he has found it possible to determine with it the consistency of all the materials now on the market for use in the treatment of roads with pitches or oils, at temperatures from 0° to 100° C., or higher. A description of the instrument (see Fig. 1) follows.

The apparatus consists of an accurately balanced wheel, grooved at the circumference and provided with some measuring device to determine with accuracy the arc through which the wheel is turned. We have used two wheels in our experiments, one

8 ins. in diameter, weighing 148.6 grams, the other 4 ins. in diameter weighing 31.1 grams. A thread passes around the circumference of the wheel, to which it is attached at one point to prevent slipping. At one end of the thread a small hook is used to attach the plungers, and at the other end is suspended a pan for weights. Sperm oil is used to lubricate the friction parts, which consist of pivots in the one case and a steel axle in the other. An excess load of 0.3 gram is required on one side or the other in both machines to overcome friction resistance, inertia, etc.

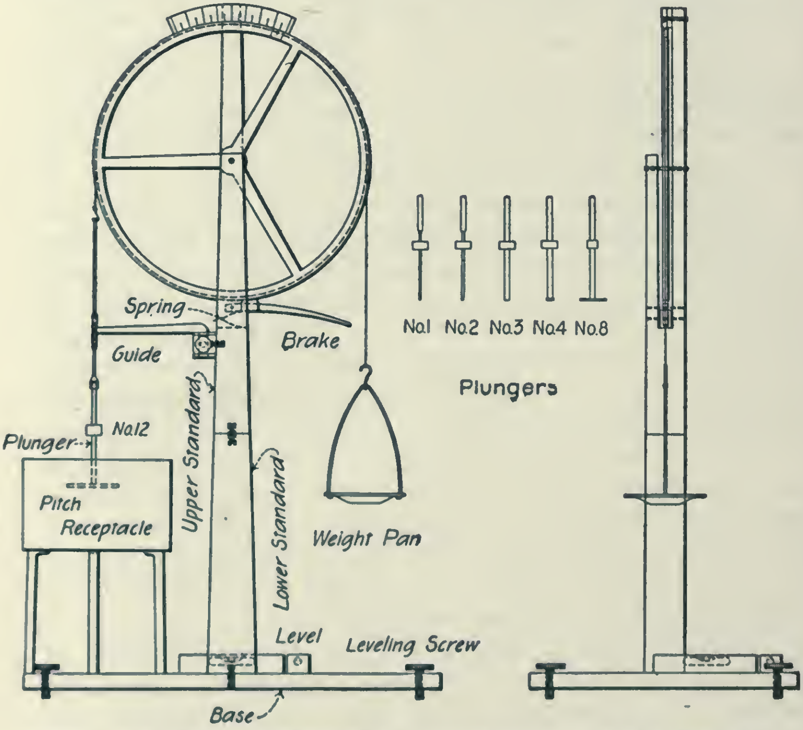

FIG. 1.—A New Consistometer for Testing Bituminous Road Materials.

The principle of the machine is to measure the distance traveled by a disk of known area and under a known load through a substance contained in a box of fixed dimensions, during a fixed interval of time, five seconds. The distance traveled is measured in tenths of a millimeter, and is the number sought. The plungers are all round disks of brass with spindles inserted in the center. All of these spindles are stiff and strong, $\frac{3}{32}$ in. diameter wire, and about 2 ins. long, except in the plunger marked "No. 12 Special," in which case it is more slender and is 5 ins. long. The box in which the fluid is placed is the ordinary box used in the evaporation tests of tar and is $2\frac{1}{2}$ ins. in diameter, 1 in. high; in the case of very fluid substances, a box of the same diameter but 5 ins. deep is used.

In the testing of a substance, the plungers are immersed directly beneath the surface of the liquid, a correction being applied for the sustaining force of the liquid (which is evidently readily obtained from the specific gravity of the tar or oil and the determined relationship between the specific gravity of the plunger and its weight); and the weights are so adjusted on the balanced pan that a known weight is applied to the plunger. The brake is then released for five seconds, the plunger descends into the material, and the depth of its descent is read on the measuring device first mentioned.

It is found, from our experience, that no guide for the spindle is necessary until the load on the plunger is in excess of 10 grams for the No. 12 Special, and 5 grams for the other plungers. The plungers and loads are so selected that the penetration is between 100 and 200 (Dow scale), except in the case of fluid oils where the use of the No. 12 Special is required. The diameters of the disks of the plungers (whose thickness is uniformly $\frac{5}{64}$ in.) are as follows:

Number.	Diameter, ins.
0	No. 2 Cambric needle*
1	$\frac{1}{32}$ (wire).
2	$\frac{1}{16}$ "
3	$\frac{1}{8}$ "
4	$\frac{3}{16}$ (disk).
5	$\frac{1}{4}$ "
6	$\frac{3}{8}$ "
7	$\frac{1}{2}$ "
8	$\frac{3}{4}$ "
9	1 "
10	$1\frac{1}{8}$ "
11	$1\frac{3}{8}$ "
12 and 12 special	$1\frac{5}{8}$ "

The guide to keep the spindle vertical is used, as indicated above, when the load exceeds 5 or 10 grams. The construction of this and its attachment to the machine is sufficiently clear from the sketch. The wheel for carrying the plunger and weight was adopted for convenience, simplicity and cheapness, but any contrivance—such, for instance, as an analytical balance—by which the plunger can be counterpoised and loaded and which is provided with an index for recording the movement of the plunger, can be used with concordant results.

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* Used in Penetrometer.

The machine is easily operated and can be used with a load of 500 grams with a No. 2 needle, which might be required for a very hard pitch, or with a load of 0.5 gram and the No. 12 for road oils which may have, theoretically at least, a viscosity less than water. Its readings on all products up to these very fluid substances are entirely satisfactory; its readings on very fluid oils are probably sufficiently accurate for road oil work, though not as accurate as the well-known viscosimeters used for testing lubricating oils.

Perhaps the writer should state that the idea of such an instrument as has been described occurred to him in 1908. At that time, it was customary to state that a pitch had a "penetration of 100" (referring to Dow's machine), or that an oil had a "viscosity of 250 seconds" (Engler). Other instruments were occasionally mentioned, such as Saybolt's viscosimeter, Lunge's tar-tester, Richardson's float, etc. The lack of calibration as well as the variety of the various instruments available, seemed to emphasize the desirability of a single substitute. While at work on the idea, during such moments as could be spared from his daily duties along other lines, a somewhat similar and more complicated machine* was brought to his attention. The writer's main objections to this instrument are its complexity and that a considerable factor of unknown importance seems to be present in its work, namely, the relation between the specific gravity of the material and of the head of the plunger.

Somewhat later, the writer's attention was attracted to a test proposed by J. Walker Smith in his book on "Dustless Roads and Tar Macadam" for determining the consistency of tars. Smith's test seems to be at the opposite extreme in the matter of complexity of instrument. The writer hopes that neither Mr.

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*"Improved Instruments for the Physical Testing of Bituminous Materials," by Herbert Abraham. *Proceedings*, Vol. IX, 1909, pp. 568–579.

Abraham nor Mr. Smith will feel that he had unwarrantedly borrowed any ideas from either and begs to state that so far as his consciousness goes, he is unaware that he has done so.

The development of the consistometer above referred to, and all the work with it, has been done for the writer by Penniman and Browne, of Baltimore. The writer wishes to here record his great appreciation of their assistance and his obligation therefor, without which it would have been impossible to have obtained these results.

In order that a better understanding may be had of the machine and its work, some of the records of the results obtained with it will be given. Ten samples of material were selected varying from a fairly hard pitch to a fairly liquid oil, as shown in Table I.

TABLE I.

Sample No.	Material.	Specific Gravity at 25° C.	Viscosity, seconds.		
			Engler 50 cc., 25° C.	Engler 100° C.	Lunge, 25° C.
110	Ugite	1.137	.....	126.0	280.0
116	Headley Binder No. 11	0.978	.....	104.0	40.7
120	U. G. I. Anti-Dust	1.094	258	17.0	.....
128	Special Texaco Compound	0.930	.....	36.0	0.8
130	U. G. I. Dust Layer	1.099	813	20.0	.....
131	U. G. I. Dust Layer	1.119	1,610	19.4	.....
133	Asphaltoilene	0.942	.....	57.6	4.2
134	Standard Oil No. 5	0.930	2,233	34.2	.....
135	Texaco Joint Filler	0.991	18*	.....	.....

Taking now for instance Sample 120 and the No. 12 Special Plunger, we have:

No. 12 Special Plunger + Hook = Scale Pan + 12.1 grams.
The loss of weight of plunger in water was 2.28 grams.
$2.28 \times 1.094 = 2.4943$. $12.1 - 2.5 = 9.6$.

That is, this plunger immersed in Sample 120 would be balanced by 9.6 grams in the scale pan. The frictional resistance was found to be 0.3 gram. The consistometer was then adjusted for work, the plunger immersed in the sample, and 9.3 grams placed in the scale pan. By removing weight from the pan the plunger was considered to be loaded accordingly. The "constants" of each sample with a given plunger were determined in this way, and the sample tested.

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*Dow Penetrometer at 25° C. (100 grams load).

The results of the tests are given in Table II. An inspection of the table shows that with the first nine samples listed it was easily possible to use at least two different plungers; in the case of Sample 116, nine were used. Sample 135, Texaco Joint Filler, was tested with Plunger 1.

The values given in Table II were averaged in most cases from two or three readings, which seldom varied more than one or two units from the mean. In many instances the readings were identical.

As of incidental interest, a comparison of the results obtained with our No. 1 Plunger and a No. 2 Needle on a sample of special pitch, follows:

Net Load, grams.	No. 1 Plunger.	No. 2 Needle.
50	5, 5, 5.	4, 4.
100	12, 11, 12.	13, 13.
150	17, 17.	
200	22, 19, 20, 20.	32, 30, 31.
300	28, 25, 29, 28.	52, 48, 51.
400	31, 28, 30.	52, 50, 51.

In order to note the possible effect of the dimensions of the wheel on the determinations made, another wheel was crudely arranged on a separate standard and base. The following tests were then made on this machine: Sample 120, using special Plunger 12; Sample 116, using Plunger 9; Sample 110, using Plunger 3. The results of these tests are given, for purposes of comparison, in Table II, in the columns marked with an asterisk. Considering the crudity of the small-wheel machine, it is felt that, within reasonable limits, the size of the wheel is shown to have no practical effect on the actual movement of the plunger provided the friction in the axis is a minimum. The concordance of the results of repeated determinations by the two machines is very satisfactory.

Typical results obtained with other instruments now in use, from actual records of work, are shown in Table III.

Apparently the curve of a definite penetration when plotted with the weights as ordinates and the areas of the plunger faces as abscissas is very close to a parabola—at least sufficiently so for practical purposes. There appears to be no direct relation be-

Table III.

No. of Trial.	Engler, seconds.	Lunge, seconds.	Penetration, Dow Scale.			Float, seconds.
			A	B	C	
1	683.5	152.6	10	37	182	75.8
2	623	155.4	7	30	191	66.2
3	598	140.5	11	42	179	83.8
4	.....	.....	12	40	193	83.4
5	.....	.....	10	40	...	....

tween the penetrations resulting on different materials. even when their specific gravities are considered, which, to the writer, is not unexpected.

In order to obtain if possible an indication of any relation, or lack of relation between plungers of the same diameter, but with heads of different volumes, we prepared a No. 6 Special Plunger, whose head was a cylinder $\frac{3}{8}$ in. in diameter and nearly $\frac{9}{16}$ in. high, having a volume of exactly one cubic centimeter. The comparison of the results from the use of this special plunger and Plunger 6, is given in Table IV.

Table IV.

Sample No.	Net Weight, grams.	Plunger.	
		No. 6.	No. 6 Special.
116	3	22	4
116	4	42	20
116	5	58	40
116	10	110	62
116	15	153	80
116	20	185	..
110	5	18	10
110	10	60	22
110	15	108	..
110	20	135	48
110	30	180	68

Note Readings are given in tenths of a millimeter. (Dow Scale.)

As of further interest, determinations were made with this machine on three samples of pitch compounds prepared by the courtesy of Mr. W. H. Fulweiler. According to him, they were "samples of coal-tar, asphaltic oil, and water-gas tar," showing "with 60 cc. at 25° C. with the Engler viscosimeter 514, 518, and 521 seconds, respectively." The results of the tests are given in Table V.

TABLE V. COMPARISON OF PITCH COMPOUNDS.

No. 12 Special Plunger. (Time, 5 seconds.)

Net Load, grams.	Asphaltic Oil.	Water-Gas Tar.	Coal Tar.
0.29	38, 40, 38.	....	....
0.3	65, 65, 64.	8, 9, 9.	4, 4, 5.
0.4	90, 90, 90.	15, 17, 15.	8, 8, 8.
0.5	105, 108, 105.	25, 24, 25.	12, 13, 12.
0.6	132, 132, 132.	36, 36.	20, 20, 20.
0.7	151, 150, 105.	52, 54, 53.	25, 28, 28.
0.8	170, 169, 171.	65, 68, 68.	36, 36.
0.9	202, 200, 201.	78, 81, 80.	48, 50, 50.
1.0	....	92, 92.	58, 57, 58.
1.1	....	104, 105, 104.	70, 70.
1.2	....	121, 122; 121.	78, 80, 78.
1.7	....	190, 190.	118, 122, 122.

Engler Viscosimeter. (60 cc. at 25° C.)

Fulweiler..........	518 seconds.	521 seconds.	514 seconds.
Crosby............	551 "	547 "	553 "

It will be noted that the time factor has been used as a constant, and it may occur to many, as it did to the writer early in his work, that the time or "time load" should be the variable to be determined. However, in order that the work with this machine might be compared with that of the ordinary penetrometer in general use, as well as for the reason that uncertainties seemed to be more probable with the larger than with the smaller plungers (or "needles"), it was decided for the earlier work to use a constant time of five seconds. Those interested can of course settle this point for themselves, as the writer will endeavor to do for his future work. The writer's limited opportunity for carrying on such work as described herein has prevented him from reaching definite conclusions regarding formulas, the selection of the few plungers desirable for practical work; and various other questions. He believes, however, that in the near future he will be using this machine in his regular work for the selection of pitch compounds, and he trusts that others with more time for such matters may, from the foregoing, be able to carry the conclusions still farther for both their own benefit and that of the profession.

DISCUSSION.*

Mr. S. R. Church.—There was one point brought out by **Mr. Church.** Mr. Abraham that is not quite clear to me, and possibly it would be interesting to have him elaborate it a little. He says, under General Observation 3: "No relation appears to exist between the hardness and the tensile strength of bituminous materials." Now one reference to his table will show that refined Trinidad Lake asphalt, which has a fusing point of 188° F., has the very high tensile strength of 4.15 kilograms per sq. cm. at 115° F., 21.0 kilograms at 77° F., and 27.0 kilograms at 32° F.; whereas Trinidad asphalt cement, which has a fusing point of 113° F., has the very low tensile strength of 0.25 kilogram per sq. cm. at 115° F., and 0.75 kilogram at 77° F., and 15.0 kilograms at 32° F. I think possibly Mr. Abraham can give more information on that point.

Mr. Herbert Abraham.—In reply I would say that if **Mr. Abraham.** you will refer to the figures for wood-tar pitch and coal-tar pitch, you will note that wood-tar pitch, with a fusing point of 123° F., has a hardness of 53.1 at 77° F., and a tensile strength of 2.1 kilograms; coal-tar pitch, with a fusing point of 122° F., has a hardness of 24.7 at 77° F., or about half that of wood-tar pitch, and yet its tensile strength is twice as great as that of coal-tar pitch at the same temperature, amounting to 4.65 kilograms. If you will compare other materials, as, for example, refined Trinidad Lake asphalt (having a hardness of 74.9 and a tensile strength of 21.0 kilograms at 77° F.) and straight Texas oil asphalt, grade 185 (having a hardness of 76.1 and a tensile strength of but 4.5 kilograms at 77° F.), you will likewise find that the relationship between the tensile strength and the hardness is not at all proportionate. Some materials may be harder than others and at the same time possess less tensile strength; and vice versa.

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* Joint discussion of the two preceding papers.—Ed.

Mr. Church. MR. CHURCH.—I did not understand that you meant to compare different materials. I thought you were referring to a comparison of the same material of different hardnesses. I see now what you meant.

Mr. Abraham. MR. ABRAHAM.—I might add that this same fact holds true if the same material is compared at different temperatures. Thus if you refer to the curves shown in Fig. 6, you will notice that as the temperature decreases the tensile strength increases up to a certain point, and then suddenly drops; whereas the hardness increases continuously. This is true for all substances. It follows, therefore, that there is no relation between the hardness and the tensile strength of a single substance at different temperatures.

I should like to ask Mr. Crosby whether it is possible to plot the results with any one substance examined at different temperatures; in other words, whether a curve of the results can be constructed.

Mr. Crosby. MR. W. W. CROSBY.—A curve can be constructed from the results secured on one material with the different plungers at any one temperature. With No. 116, we used nine different plungers at 25° C. and of course a number of different plungers can be used at any other temperature, not necessarily the same number. With the same material there would be, as far as we know, no relation between two curves from different temperatures.

Mr. Abraham. MR. ABRAHAM.—Then if I understand you correctly, while it is possible to construct a curve for a substance at one temperature, it is impossible to construct one showing the behavior of that same substance at various temperatures?

Mr. Crosby. MR. CROSBY.—I should not say it was impossible. We have simply not had the time to work that out.

Mr. Abraham. MR. ABRAHAM.—Have you noticed any effect of frictional adhesion of bituminous materials to the sides of the plunger?

Mr. Crosby. MR. CROSBY.—Yes. Please refer to figures reported on use of Plungers No. 6 and No. 6 Special.

Mr. Abraham. MR. ABRAHAM.—Have you tried any plungers with flat heads and reduced shanks?

Mr. Crosby. MR. CROSBY.—All our plunger heads are uniformly $\frac{1}{16}$ in. thick, and the shank is set perpendicularly into the center of the head. The shank is simply a round wire, and there is no curve at the junction of the shank and the head. **Mr. Crosby.**

Mr. Fulweiler. MR. W. H. FULWEILER.—There is one point in Mr. Crosby's paper which appealed to me as being of very considerable interest, namely, that the three samples of materials that were prepared to give approximately the same viscosity with the Engler viscosimeter differed so widely when tested with Mr. Crosby's instrument. This would seem to indicate the possibility of differentiating between the different classes of materials by this variation in viscosity. It is, of course, well known that a number of the other viscosimeters will differ not only in actual results, but also relatively with the Engler instrument, particularly the type of viscosimeters in which the head is changing, such as the Dudley; but none of them seem to give so widely divergent results as those reported in this paper. The possibility of thus differentiating with Mr. Crosby's instrument between materials that have the same apparent Engler viscosity, may serve to explain why materials that are apparently prepared with the same viscosity act very differently when applied in practice on the road.

If an instrument of the type described by Mr. Crosby should be adopted, we shall undoubtedly have to revise many of our ideas as to the required viscosity of materials for different methods of road treatment.

Mr. Crosby. MR. CROSBY.—I might say on that point that my actual experience confirms Mr. Fulweiler's suggestion. We were using two different materials two years ago, one of which showed considerably greater viscosity with the Engler viscosimeter than the other, and yet when we attempted to apply them to the road under the penetration method, the material indicated as lighter and more mobile by the Engler test was shown to be much less mobile in the work on the road. We took samples of the two materials, and on testing them with this machine the impression received on the road was confirmed, the very opposite of the indications of the Engler instrument.

The President. THE PRESIDENT.—Of course the real value of any of these methods of testing depends upon how closely they measure the behavior of the materials in service. That is a very important point.

If I may be permitted to speak upon a subject of which I

The President. know very little if anything, it would seem to the uninitiated as if the two words consistometer and viscosimeter represent really the same thing and as if one of the two names might be suppressed and the other retained. Is not the instrument after all a viscosimeter? That is to say, it does not determine, as I understand it, the true melting point, but the point at which the substance becomes fluid enough to drop off, though no molecular change or true change of state is taking place. It is not a true melting point, but it is the temperature at which a certain specific degree of viscosity is reached. Viscosity decreases with rising temperature, and in decreasing falls to a certain minimum. The fact that it falls you accept as proof that it has fused, when that is not true, for what you recognize as the fusing point is a certain specific degree of fluidity arbitrarily agreed on. If so, is this not a special measure of viscosity after all?

Mr. Abraham. MR. ABRAHAM.—Very often a material will fuse at a certain temperature, and the trade will designate that material as having a certain fusing point, a certain melting point. Unfortunately the term "melting point" is not strictly correct. Melting point, as applied to a pure organic substance, or any other of definite chemical composition, has been defined as the point at which it passes from a solid to a liquid state. It is really the point of melting. In the case of alloys of iron and steel the melting point is shown by a break in the cooling curve; but in the case of bituminous materials where there is no break in the curve; it is merely an arbitrary point which will serve to give some indication of the consistency or hardness of material at ordinary temperatures.

Mr. Pullar. MR. H. B. PULLAR.—I should like to ask Mr. Abraham if he has ever tried two materials which have the same penetration, but one material being sticky and the other more cheesy. Would he, in that case, get a definite correction factor or would he get approximately the same results by the penetration instrument?

Mr. Abraham. MR. ABRAHAM.—I have found that two materials having the same penetration by means of the Dow instrument will have approximately the same consistency (hardness) when tested by the consistometer, irrespective of whether one is sticky and the other is cheesy.

Mr. W. B. D. Penniman.—I have been asked whether in using the Crosby machine I had noticed any adherence of the material to the side of the disk. Crosby's machine evidently measures the adherence on both the bottom and sides of the disk used. I think the point made in regard to bringing the edge of the disk to a knife edge is a good one, because then the areas of the circular disks are directly compared and the adhering surfaces are of the same kind. In each instrument the same question comes up, for if you have two materials of the same penetration, using a needle with a fine point, the penetration is a measurement of two resistances: first, that of the point; second, the clinging of the material to the sides of the needle. When testing hard material, the first resistance is the larger; with soft, sticky material the second is the larger. In Mr. Abraham's instrument, this combination of resistances is largely avoided. **Mr. Penniman.**

Mr. Abraham.—Yes, that is true. **Mr. Abraham.**

Mr. Penniman.—I cannot see, therefore, how two materials could possibly test in your machine the same way, using your plunger the same as the needle. It has not been so with my small plungers. In other words, do you find that two materials that run with the needle 150 penetration would give, say, 50 each with your small plunger? **Mr. Penniman.**

Mr. Abraham.—Perhaps I did not make myself clear. Two materials testing 50 penetration with a needle would give closely agreeing readings with the consistometer; there might in certain cases be some variation, but my experience has shown that it would be very small and probably not more than a point or two one way or the other. **Mr. Abraham.**

I found in the old type of consistometer (described in the original paper in 1909) where the edge of the plunger was slightly rounded this noticeably affected the ease of penetration, so that the force necessary to produce the same penetration with the round edge as compared with the knife edge was in the ratio of 10 to 7. Do I make myself clear?

Mr. Penniman.—Perfectly. That answers the question. **Mr. Penniman.**

Mr. L. Kirschbraun.—I should like to ask Mr. Abraham whether in making the tensile strength test of those various bitumens he made them on the pure bitumen, or upon asphalt that may have contained a proportion of foreign matter. **Mr. Kirschbraun.**

Mr. Abraham.

MR. ABRAHAM.—Both. In some cases the bitumen was impure from mineral matter. Most of the oil asphalts are almost free from mineral matter; but in no case was an attempt made to remove the mineral matter. The substance was tested in its commercial condition.

Mr. Kirschbraun.

MR. KIRSCHBRAUN.—Do you know just what effect the mineral matter in these various bitumens will have on the tensile strength?

Mr. Abraham.

MR. ABRAHAM.—There seems to be quite a field for investigation on that line. As yet I have not examined the effect of mineral matter on tensile strength as completely as I should like. It appears to me from tests we have made, however, that the presence of mineral matter will increase the tensile strength; and I have never found a case in which the mineral matter has decreased it.

Mr. Kirschbraun.

MR. KIRSCHBRAUN.—I should like to add in confirmation of Mr. Abraham's statement, that I have made similar tests and have found that the addition of mineral matter very materially increases the strength of otherwise pure bitumen.

The President.

THE PRESIDENT.—Is that not true of plastic substances in general? It would seem to me as if it were a natural consequence, drawing an analogy from certain phenomena in the metallurgy of steel which are explained on a like supposition. It was the view that Dr. Dudley held that the presence of hard substances in a plastic medium must necessarily tend to harden and strengthen that material; and we know certainly that in the case of clay crucibles which are used at a high temperature, the clay becomes soft and squashy, but by mixing it with 50 per cent. of coke by volume we get a crucible which at very high temperatures withstands admirably the crushing pressure of the tongs which are used in lifting the crucibles out of the furnace, whereas if it were not for the coke the crucible would crush right in, would not in fact sustain its own weight. The phenomenon is a very interesting one for all of us who have had to do with any form of plastic material.

APPARATUS FOR DETERMINING THE DROP POINT AND SOFTENING POINT OF COMPOUNDS.

By H. W. Fisher.

During the past few years, the writer has made many experiments with compounds, asphaltums, resins, etc., of various manufacturers and jobbers, and has found so great a difference in their consistency at the specified melting point, that an investigation was made which showed: first, that a large majority of compounds, unlike minerals, have no well-defined melting point; second, that what some companies specify as a melting point is really a softening point of the material; and third, that the melting point as used by other companies corresponds to the temperature at which the compound will drop. This investigation led the writer to believe that instead of using the misnomer "melting point," it would be more practical to speak of the softening point and drop point of compounds.

The next step was to design a method for determining the softening point and melting point. Of necessity, any plan must be arbitrary, and the important question was to devise a method and apparatus that would be simple and by means of which important deductions as to the physical quality of the compound could be made.

After various experiments, the most uniform drop point tests were obtained by filling a small hole in a piece of iron with the compound and providing an air vent at the top so that the material would be free to flow out, when soft, under the uniform action of gravity.

A method for determining the softening point of compounds presented more difficulty, but the writer happened to think that reversing the apparatus used for the drop-point test and using mercury, would force the compound to the surface at a temperature below that of the drop point. Experiments not only showed that this method gave very uniform results, but also that the softening point corresponded with what some manufacturers call the melting point, and which, as explained above, is only a softening point.

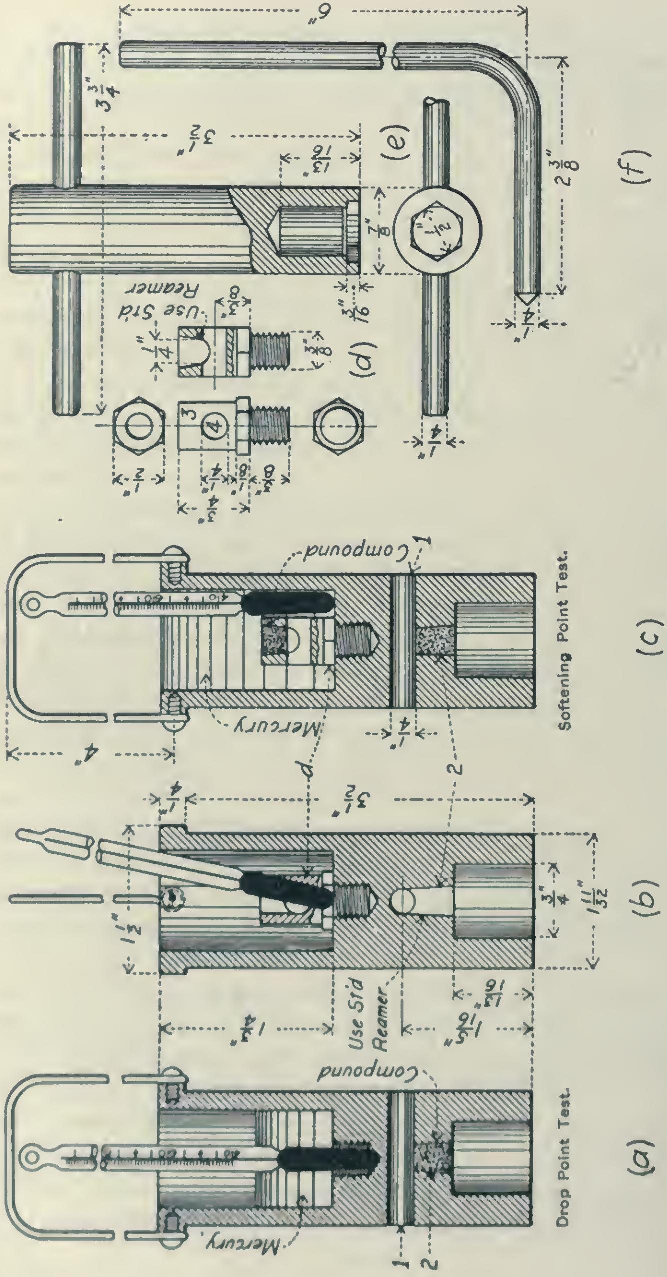

FIG. 1.—Apparatus for Determining the Drop Point and Softening Point of Compounds.

Now in all these experiments, the question of time plays a most important part, and as specifying a slow rate of heating would mean long-drawn-out tests, it was decided to use a rate of heating that would give drop-point tests somewhat comparable with those made by several large companies, where the compound is molded on the bulb of a thermometer and heated at the rate of 5° C. per minute until the compound drops off, at which instant the temperature is read. Tests of compounds by both methods showed that with the apparatus of the writer, the rate of heating should be 4° C. (7° F.) per minute.

The apparatus was then designed so that the softening point test and the drop-point test could be made at the same time. Fig. 1 gives a working drawing of the outfit. The section of the instrument when arranged for a drop test only is shown at (*a*). The compound is placed in hole 2; hole 1 is the vent hole. The nipple (*d*) is provided with a vent hole 4 and a compound hole 3. A wrench for removing and inserting the nipple is shown at (*e*). The rod (*f*) is placed into the nipple through hole 4, after which the nipple and rod are heated above the melting point of the compound. The hole 3 is then filled with the compound, and after partial solidification the rod is removed and the surplus compound at the top of the hole in the nipple cut off flush with the top of the nipple. Afterwards, the nipple can be screwed into place as shown at (*c*).

Fig. 2 shows a convenient method for filling the hole in the part of the apparatus used for the drop-point test. The No. 8 B. & S. G. copper wire, *B*, can be raised or lowered at will. The rod *D* fills the vent hole. The rod and iron are first heated to a point above the melting point of the compound and then placed as shown in Fig. 2. After the wire *B* becomes hot, a piece of compound is held against it as shown and the hole thus filled with compound, after which one or two drops more should be applied to allow for shrinkage. The iron and rod are then removed and cooled, after which the rod is removed. The surplus compound is removed by means of a piece of thin steel with a square edge or a suitable tool, so that the surface of the compound in the hole is flush with the bottom surface of the hole.

Referring again to Fig. 1 (*a*), it will be noted that hole 2 is some distance above the bottom of the circular piece of iron. The reason for this is to provide a space where the air around the melting compound will be at about the same temperature as that of the compound and the iron, thus allowing the compound to drop freely and not be subject to non-uniform atmospheric conditions.

Fig. 3 illustrates the method by which the writer applies heat uniformly over the entire apparatus. A fiber spool approximately $1\frac{9}{16}$ ins. inside diameter, 3 ins. long, and 4 ins. outside diameter, is wound with about 240 turns of No. 12 D. C. C. magnet wire. By means of alternating current of 60 cycles, the iron testing apparatus is heated to any desired degree. The voltage employed varies between 30 and 55 volts and the current from 4 to 8 amperes. The voltage can be regulated by steps of half a volt. If one desired to make the test on a 110-volt circuit, wire half the size could be employed and the voltage could be varied between 60 and 110 volts and the current from 2 to 4 amperes. By this method the

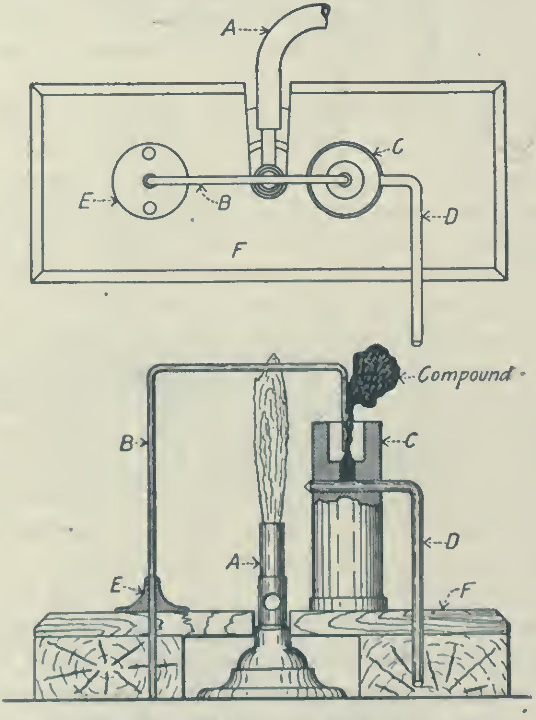

FIG. 2.—Showing Method of Filling Apparatus.

temperature can be kept almost constant at any degree or can be made to vary as desired.

When an alternating current is not available, the iron can be heated sufficiently uniformly to give good results in the following manner. On top of the glass tube place a piece of metal or fiber with a hole in the center equal in diameter to the hole in the bottom of the apparatus. Take a piece of iron pipe about 1 in. larger in inside diameter than the outside diameter of the apparatus, make a number of vent holes around the bottom of the pipe, make two diametrically opposite holes at a distance above the bottom of the iron pipe equal to the distance between the vent hole and the bottom of the apparatus. Then place the apparatus on the center of the plate and over it put the iron tube so arranged that the holes in it will be at right angles to a line through the vent holes of the appa-

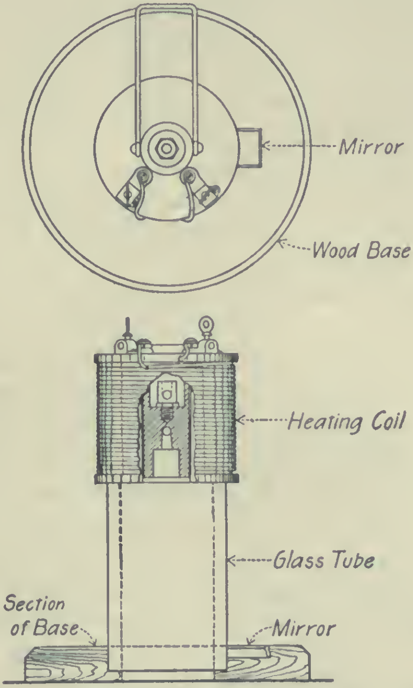

FIG. 3.—Showing Method of Applying Heat Uniformly.

ratus. A suitable burner must be arranged by means of which small flames can be made to strike the apparatus through the holes in the outside pipe. The adjustment of these flames should be made by means of an ordinary gas valve provided with a handle about a foot long. The writer has tried the above plan and has been able to obtain results which agree very well with those made by the more refined electrical method.

The testing apparatus and coil are placed on top of a large glass tube which is embedded in a wooden base. By means of a mirror at the side of the glass tube in the base, the melting of the compound, in the drop-point test, can be observed. When doing this, it is necessary to have an incandescent lamp on the opposite side of the base from the mirror.

Having filled both sides of the apparatus with the compound, and having inserted a thermometer as shown in Fig. 1 (*c*), the temperature is increased at the rate of 4° C. per minute until the compound comes up through the mercury at the top of the apparatus. The temperature at which this occurs is recorded as the softening point of the compound. Continuing the test further, the temperature at which a drop of the compound falls through the glass tube is recorded as the drop point of the compound. If the compound has a high softening point and a high melting point, a somewhat greater rise of temperature per minute is admissible to within about 30° F. of the softening point.

Table I gives the results of a number of tests made with this apparatus, the heating being done by the electrical method. From an examination of the table, one can judge as to the possible accuracy obtainable when care is taken to make the tests as accurately as possible. By carefully adjusting the rate of temperature rise, it should be possible to obtain results nearly comparable with those in the table. The table also gives the results of tests made with the temperature kept constant for 25 minutes. Of course, it was necessary to make two or three tests at different temperatures before obtaining the results given. The figures in both Columns C and E give an idea of the change of viscosity below the drop point. In the case of roxite wax, it will be noticed that the difference between the drop point and softening point is only 5°.1. Any one familiar with this apparatus who was given the result of this test would know at once that the material was a

Table I.

(Temperatures given in degrees Fahrenheit.)

Name of Compound.	Rate of Temperature Rise per Minute.	Temperature		Average Temperature Difference between Drop and Softening Point.	Time Test.	Temperature at Softening Point.	Temperature Difference between A and D.	Ratio, E/C.
		at Drop Point.	at Softening Point.		Time in Minutes.			
		A	B	C		D	E	
Roxite Wax, 185°.	7	188.5	182.2	5.1	25	177.5	10.8	2.1
	7	188.0	182.0					
	7	.....	182.3					
Asphaltum Ox., 180°.	7	213.7	180.0	33.6	25	150.0	63.7	1.89
	7	214.0	180.5					
	7	.....	179.0					
Petroleum Pitch.	7	209.2	191.5	18.2	25	162.0	47.2	2.6
	7	209.2	191.0					
	7	209.3	190.2					
	7	.....	191.5					
Commercial Insulating Compound "A."	7	146.0	129.0	18.3	25	96.0	50.5	2.75
	7	147.0	127.5					
	7	146.5	.....					

kind of wax. In the case of asphaltum, the difference between the drop and softening point is 33°.6, which indicates a slow variation in viscosity. The figures in Column E are useful in showing the effect of a time test, and the ratio E/C is a measure of the flowing properties of the compound.

If we were to draw conclusions from the figures in Column C, we should say that pitch and compound "A" have about the same flowing properties. By applying the time test, however, we find that the flowing properties of "A" are greater and undoubtedly, if hour time tests had been applied, the differences would have been still greater. The commercial insulating compound "A" has the largest ratio E/C, and by actual test this material will flow even when quite brittle at a temperature of about 60° F. On the contrary, asphaltum, which has the lowest ratio E/C, is more of the nature of rubber and does not readily flow until a high temperature is reached.

It will thus be seen that by means of this apparatus not only the drop point and softening point can readily be determined, but one can also get an idea as to the nature of the compound under investigation. Hence, a purchaser who is familiar with this apparatus and who has been given by a manufacturer or jobber the drop point and softening point of a compound, will at once get an idea of the properties of the material under consideration.

Much of the experimenting which resulted in the design of the apparatus just described was done since the formation of Committee D-9 on Standard Tests of Insulating Materials. As a member of this Committee, the writer made inquiry and found that the methods employed by different companies for testing compounds were very diverse. Before the adoption of this apparatus, the company with which I am connected had difficulty in purchasing compounds of uniform characteristics. Now, all that is necessary is that the company furnishing the material have the apparatus above described and, having once specified the desired drop point and softening point, satisfactory compounds are assured. The apparatus is simple and can be made by any one for a few dollars.

HARDNESS TESTS.

BY BRADLEY STOUGHTON AND J. S. MACGREGOR.

In the general discussion on hardness at the last annual meeting of this Society, we called attention to the need of a test to show not only the superficial hardness of steel and other metals but also the depth to which this hardness extends. For example, it is often desirable to measure this property in relation to case-hardened gears, pinions, and other materials, and to determine the sub-surface hardness of dies used for drop-forging, etc. The authors collected a number of dies with varying service records and have been using them in connection with this problem during their available spare time for the past year, to determine first, whether any of the already known hardness tests are adapted to this accomplishment, or if not, whether they can be made so; and second, whether, failing both of these, a test could be devised suitable for the purpose.

As regards the first of these problems, our conclusion is that none of the tests now known to us are adequate and, although we have considered, and, in some cases, tried modifications of these tests, we have as yet failed to discover any method of reaching the desired result.

As regards a new form of test suitable for this purpose, we are not as yet in a position to offer anything of value. It was our hope to present at this meeting a testing method that should accomplish the result desired, but we are regretfully obliged to confess our inability to do so at the present time.

While we recognize that resistance to abrasion is a different form of hardness than resistance to penetration, which is what we have been considering, nevertheless it may be of interest to refer here to the apparatus we have been using to test this quality of abrasion which is so important in many circumstances of service; for example, the wearing of the bottom of cylinders, etc. We have had in operation for some time a machine in which rotating specimens of metal are subjected to the rubbing action of fine-grain whetstones. By means of this machine it is possible to test six specimens at the same time. Each whetstone is pressed upon the specimen with a weight of 1 lb.; the speed of rotation is 400 to 500 R.P.M., which is not sufficient to overheat the metal. After each 700,000 revolutions the specimens are moved along one space in the machine, and when each specimen has been in each of the six different positions for 700,000 revolutions, the loss of weight and the reduction in diameter are determined.

[For Discussion of this paper see pages 740–743.—ED.]

A COMPARISON OF FIVE METHODS OF HARDNESS MEASUREMENT.

By R. P. Devries.

More or less theoretical definitions of hardness have been proposed at various times. The tests based upon these definitions have in some cases been impracticable. More often they have failed to give information concerning hardness that conformed to the ideas of the workshop and to the demands of actual practice. Any hardness test of metals in order to be successful must conform to the conception of hardness which is held by the men whose interest in this form of test is entirely a practical one. The increasing need for accurate information concerning hardness has resulted in the development of the hardness tests which are in use at the present time.

After looking over the field it was determined to investigate only those methods which seemed the most promising of results and which have recently been developed.

The object of this investigation is to determine whether any agreement exists between the results obtained by methods which are apparently in no way related to each other.

Methods Investigated.

1. The Brinell method is based upon determining the resistance offered to indentation by a hardened sphere when the sphere is placed on the metal under investigation and subjected to a given pressure.

2. The cone test depends upon the same principle as that of the sphere, except that a circular cone of 90° angular opening is substituted for the sphere.

3. The Shore scleroscope method depends on the height of rebound of a hardened steel hammer when it is dropped on the metal under test.

4. The Bauer method depends upon the rate at which a steel drill running at constant speed and under a constant pressure drills the metal under test.

5. The Ballantine method depends upon the amount a leaden disk is indented when it transmits the energy of a falling hammer through an anvil on to the metal under test.

1. THE BRINELL TEST.

The method of carrying out a hardness test as recommended by Brinell was to indent the metal under test with a sphere 1 cm. in diameter subjected to a pressure of 500 kilograms for the softer, and 3,000 kilograms for the harder metals. He defined the hardness as the pressure in kilograms per square millimeter of spherical indentation.

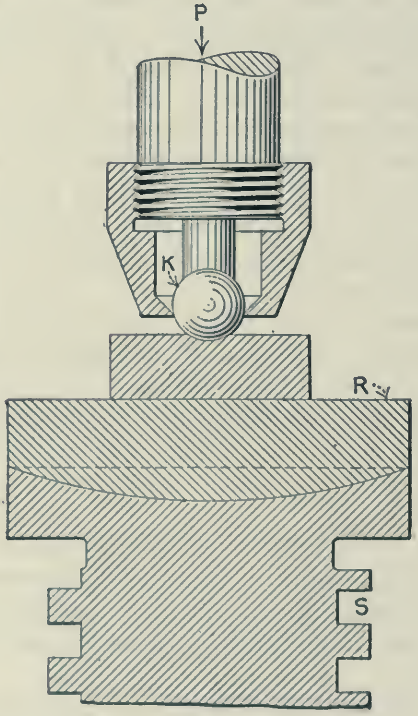

FIG. 1.—Brinell Tester.

After this method was introduced it was shown by Benedicks,*

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* *Recherches physiques et physicochimiques sur l'acieur au carbone.* Upsala, 1904.

Le Chatelier,* Leon,† Malmstrom,‡ Meyers§ and others that the hardness numeral is dependent upon the size of the sphere and the pressure to which it is subjected. Meyer calculated the hardness numeral by dividing the load by the projected area of the indentation, but shows clearly that the determination of two constants is necessary for the accurate measurement of hardness.

Martens and Heyn¶ have shown that a linear relation exists between the load and the depth of indentation for very low pressures and have suggested as the hardness numeral, the load which is required to produce an indentation 0.05 mm. deep. Since this method of determining the hardness eliminates the necessity for any arbitrary assumption, it was determined to further investigate the relation between load and depth of indentation at both low and high pressures.

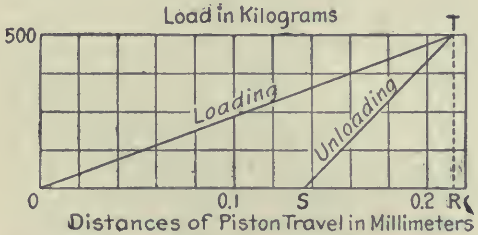

FIG. 2.—Typical Curve of Loading and Unloading in the Brinell Test.

Description of Method.—Fig. 1** shows a sketch of part of the Brinell apparatus. The test piece is placed on the head *R* which is brought into contact with the 1-cm. sphere by means of the screw *S*. A small initial pressure is applied to the piston *P*. A micrometer microscope reading to one one-thousandth of a millimeter is focussed upon a line drawn upon the piston, and a reading taken. The desired pressure is then applied and released to the point of initial pressure before the final reading is taken. The difference between the initial and final reading is equal to the depth of indentation of the sphere.

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* *Revue de Métallurgie*, 1906, No. 2.

† Die Brinellsche Härteprobe und ihre praktische Verwendung." *Proceedings, International Association for Testing Materials*, 1906.

‡ *Stahl und Eisen*, 1907, No. 50.

§ " Untersuchungen über Härteprüfung und Härte." *Zeitschrift des Vereins Deutscher Ingenieure*, 1907.

¶ " Vorrichtung zur vereinfachten Prüfung der Kugeldruckhärte und die damit erzielten Ergebnisse." *Zeitschrift des Vereins Deutscher. Ingenieure*, 1907.

** Acknowledgment is made to the *American Machinist* for the drawings from which the cuts in this paper were made.—ED.

Fig. 2 shows a typical curve of loading and unloading. In this figure *OR* represents the entire downward travel of the piston. The distance *OS* is the actual depth of indentation. It is equal to $OR - SR$. The distance *SR* represents the compression of the sphere, piston and all parts of the apparatus lying below the reference line.

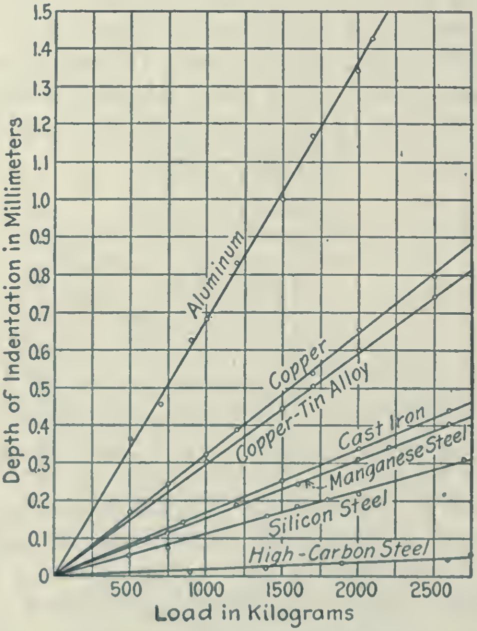

FIG. 3.—Relation between Load and Depth in the Brinell Test.

The depths of indentation were measured for a series of metals at varying pressures. The curves plotted in Fig. 3 are typical of the results obtained. They show the existence of a linear relation between load and depth of indentation for pressures ranging from 0 to 3,000 kilograms.

The total downward travel of the piston was measured for several metals. These results are shown in Fig. 4. The actual depth of indentation as measured is also plotted for easy comparison. Curve *A* shows the total downward travel of the piston.

Curve *B* is the actual depth of indentation. The difference between any ordinate of Curves *A* and *B* represents the compression of the different parts of the apparatus lying below the reference line. Any measurement of the depth of indentation must totally exclude these elastic compressions. It seems probable that Martens and Heyn may have encountered difficulty in doing this, because they state that when their curves depart from a straight line they sometimes curved upwards and sometimes downwards.

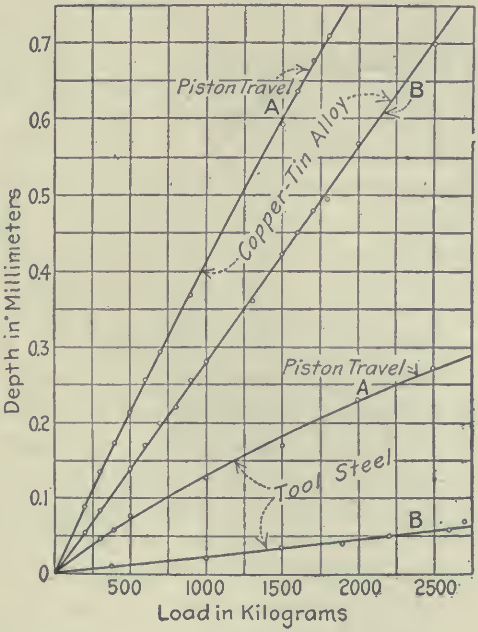

FIG. 4.—Load-Depth Relations for Two Metals.

Elastic Deformation of the Sphere.—The hardness numeral as ordinarily determined is a function of the size of the sphere. If the sphere is elastically deformed under pressure, the hardness numeral will also be a function of the temper or hardness of the sphere.

Measurements of the depth and diameter of indentation of several metals were made at various loads. From these values the radius of curvature of the indentation was calculated on the assump-

TABLE I.—RADIUS OF CURVATURE OF INDENTATION FOR DIFFERENT METALS.

Load, kilograms.	Values of R′ in cms. for				
	Bessemer Steel.	Cast Iron.	Tobin Bronze.	Copper-Vandium. Alloy.	Manganese Steel.
500	5.71	6.71	6.31	6.72	7.64
1,000	5.79	6.46	5.83	6.46	6.98
1,500	5.73	6.26	5.43	6.26	6.59
2,000	5.59	6.16	5.13	6.16	6.24
2,500	5.56	6.05	5.05	6.05	5.70
3,000	5.57	5.91	5.06	5.91	5.43

tion that the indentation is a segment of a sphere. The values given in Table I show that the radii of curvature of the different indentations made with a sphere of 5 cm. radius vary considerably with the load and the metal tested.

It may be seen from analytical considerations that the amount will vary considerably for different metals even at the same pressures. For metals in which the metal flows up above the original surface, forming a ridge about the sphere, the radius of curvature of the indentation can not be calculated in the above manner.

The Relation, $P = at$.—The calculation of the hardness numeral is important, since uniformity of results in hardness testing depends upon the method employed. Of the three methods mentioned that of Brinell is always in error because the radius of the sphere is not equal to the radius of curvature of the indentation. Meyer has shown that his method is open to the objection that some metals have reached their maximum hardness when subjected to 3,000 kilograms pressure and others have not yet attained it.

The linear relation between load and depth of indentation suggests at once as the measure of the hardness, that load which is required to produce an indentation 0.1 mm. deep. The simplicity of this method recommends it, but it is an evident departure from determining the resistance to indentation.

In order to conform to our definition of hardness the hardness numeral is also calculated by dividing the entire pressure by the area of the indentation, which gives

$$H.N. = \frac{P}{2\pi t R'} \qquad (1)$$

where P is the applied pressure, t the depth of indentation, and R' the radius of curvature of the indentation.

If we substitute in Equation (1) the value $P = at$, and let $a/2\pi = C$, the mean pressure or hardness numeral becomes

$$H.N. = \frac{C}{R'} \qquad (2)$$

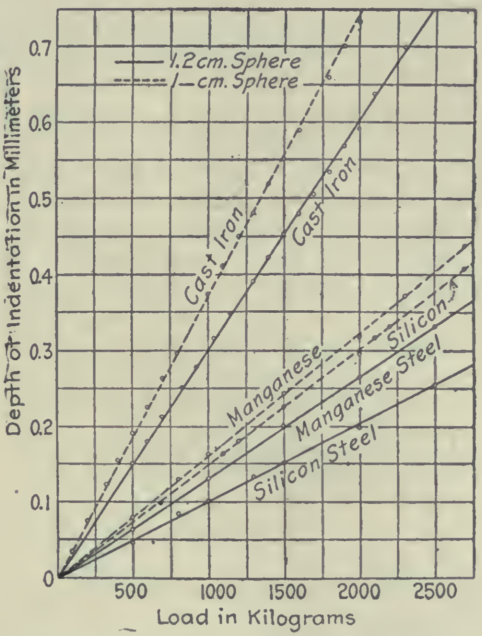

FIG. 5.—Load-Depth Relation for Spheres of Different Diameters.

Since the product of hardness numeral and radius of curvature is a constant for a sphere 1 cm. in diameter, some measurements were made with a sphere 1.2 cm. in diameter. The results, plotted in Fig. 5, show a linear relation between load and depth of indentation. The measured depths of indentation for the same metals obtained with the 1-cm. sphere are plotted in Fig. 5 for easy comparison. These curves show that the depth of indentation is smaller for the larger sphere, but that the differences in depth are dependent on the metal under test.

TABLE II.—VALUES OF HARDNESS NUMERALS OBTAINED WITH SPHERES OF DIFFERENT DIAMETERS.

Load, kilograms.	Hardness Numeral in kilograms per sq. mm. for							
	Copper.		Manganese Steel.		Silicon Steel.		Cast Iron.	
	1.2-cm. sphere.	1-cm. sphere.	1.2-cm. sphere.	1-cm. sphere.	1.2-cm. sphere.	1-cm. sphere.	1.2-cm. sphere.	1-cm. sphere.
1,000	86.5	83.4	179	177.5	215	214	136	137
2,000	....	....	183	181.5	222	219	138	139
3,000	....	....	187	185.6	225	225	140	144

The calculated values of the hardness numeral for the 1.2-cm. sphere are given in Table II, and show a good agreement with the values obtained for the 1-cm. sphere. Whether the product of hardness and diameter of indentation is a constant for all sizes of sphere, is a subject for further investigation. The hardness numeral suggested by Meyers is

$$H.N.=\frac{P}{\frac{\pi d^2}{4}} \qquad (3)$$

The other hardness numeral is, from Equation (1),

$$H.N.=\frac{P}{2\pi tR'} \qquad (4)$$

Substitute in Equation (4) the value of R' expressed in terms of t and d, and it becomes

$$H.N.=\frac{P}{\pi\left(\frac{d^2}{4}+t^2\right)} \qquad (5)$$

If we compare Equation (3) with (5) it becomes apparent that when the load and consequently the depth of indentation is small the values of hardness are practically the same for both methods.

For the higher values of the load and depth of indentation the values of hardness obtained by Equation (3) are appreciably higher than those obtained by Equation (5). The hardness numeral when calculated according to Meyers' suggestion therefore shows a greater variation with load than when it is calculated upon the actual area of the spherical indentation.

2. THE CONE TEST.

Ludwik* seems to have been the first to propose and use a cone of 90° angular opening for the measurement of hardness. Although the test is the same in principle as the sphere hardness test, he thought that the variations of the hardness numeral for different loads encountered in the sphere would be eliminated in the cone test, because the indentations made by a circular cone are geometrically similar.

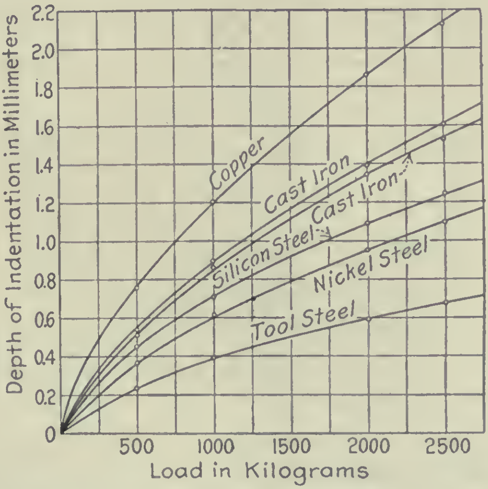

FIG. 6.—Relation between Load and Depth in the Cone Test.

In carrying out the cone test he measured the depth of indentation by means of an instrument, the action of which depends upon the downward motion of the piston to which it is attached. He calculates the hardness numeral on the basis of the entire applied pressure divided by the area of the conical indentation.

Following the same general method of procedure but using the micrometer microscope to measure the depth of indentation, radically different results were obtained, as plotted in Fig. 6.

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* "Die Kegelprobe. Ein neues Verfahren zur Härtebestimmung von Materialen."

They show that the relation between load and depth of indentation is parabolic.

To compare the results obtained in this investigation with those given by Ludwik, the hardness numerals for the same loads are given in Table III. The results tabulated are typical of a large number of results on different metals.

If the results obtained by Ludwik were correct it would mean that the hardness numeral is independent of the load. But since the relation between depth of indentation and load is parabolic, according to the curves of Fig. 6, it is apparent that the hardness numeral decreases rapidly with increasing loads and becomes approximately constant only at the higher loads. Ludwik seems to have been aware of the errors made in the measurement of the depths of indentation, for he remarks in a footnote of his paper that a more careful adjustment of his measuring instrument would give results that would show the relation between load and depth of indentation to be parabolic. The parabolic relation between load and depth of indentation makes an arbitrary selection of load necessary but not sufficient to place metals in their correct position in a relative hardness scale.

TABLE III.—COMPARISON OF THE RESULTS OF THE CONE HARDNESS TESTS.

Load, kilograms.	Copper.				Cast Iron.			
	Ludwik.		Author.		Ludwik.		Author.	
	t, mm.	$H. N.$, kgs. per sq. mm.	t, mm.	$H. N.$, kgs. per sq. mm.	t, mm.	$H. N.$, kgs. per sq. mm.	t, mm.	$H. N.$, kgs. per sq. mm.
500	1.23	74.4	0.759	195.0	0.93	130	0.549	374
1,000	1.74	74.3	1.201	154.9	1.32	129	0.864	304
2,000	2.46	74.3	1.844	132.0	1.85	132	1.343	256
3,000	3.00	75.0	2.435	114.0	2.27	131	1.683	239

The diameter of indentation might be measured if it were not for the fact that the metal, when under pressure, flows up above the original surface of the metal, forming a ridge about the cone as shown in Fig. 7, in which d' represents the diameter of indentation that could be measured, while d is the diameter that should be measured.

In his work on the cone test Meyers states that the ridge of metal which forms about the cone supports part of the applied pressure and that the hardness numeral should be calculated on the basis of entire pressure divided by the projection of the conical indentation. He therefore calculates the area using *d′* as the diameter of indentation. In determining the hardness in this way he overlooks the fact that the metal which forms the ridge has been forced from beneath the cone and has evidently lessened the resistance at that place. He also assumes that the ridge of metal offers as much resistance as if it were supported on all sides by unstressed metal.

Tests were made with cones of 60° and 90° opening on all of the metals used in the sphere test. The hardness numerals were calculated for the load of 3,000 kilograms by dividing the entire pressure by the area of the conical indentation. The results are given in Table V. For two steels no results could be obtained because the points of the cones flattened under pressure. This is one of the limitations of all forms of hardness tests which employ a pointed cone or punch.

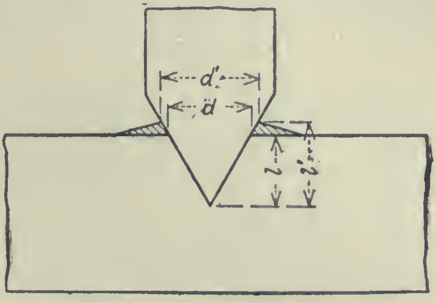

FIG. 7.—Cone Ridge Formation.

3. THE SHORE SCLEROSCOPE TEST.

The scleroscope, briefly described, is an instrument consisting of a pointed hammer, which falls from a definite height through a guiding glass tube upon the metal to be tested. The height to which the hammer rebounds is the measure of the hardness and is read directly from a scale placed back of the glass tube. This scale is approximately 10 ins. in height and is arbitrarily divided into 140 equal parts. The hardness numeral is therefore not expressed in definite physical units. This is not a disadvantage if the scleroscope is accepted simply as a hardness measuring device, but it leaves much to be desired for investigational purposes.

In this method of measuring hardness the hammer makes a permanent indentation in the metal under test. An attempt was made, therefore, to determine the hardness as the resistance to indentation. In order to do this, it is necessary to measure either the diameter or the depth of indentation. Both of these quantities are extremely small and the latter is inaccessible to all length-measuring devices. Some measurements of the diameter of indentation were made, but in general this is impracticable, because the bounding edge of the indentation is not circular. In the measurements referred to the greatest diameter of indentation was only 0.005 mm.

It would be entirely practicable to determine the hardness numeral in the manner outlined if the hammer was enlarged and the height of fall increased so that the diameter of indentation could be measured accurately.

In working with the scleroscope it was found that the hammer must be calibrated after a certain number of tests. For this purpose a piece of steel hardened to 100 scleroscope measurement must be used. The rebound for any given steel cannot be corrected by proportionate decrease or increase if the hammer gives a rebound either less or greater than 100 on the hardened steel. This shows that a slight change in the form of the hammer assigns the metal tested to a different position in the hardness scale. Although there are many things stated by the inventor concerning the theory of the instrument which cannot be accepted without experimental confirmation, they are left for discussion in the paper which deals with the relations of the physical properties of metals to hardness.* The hardness numerals of all the metals tested are given in Table V.

4. THE BAUER DRILL TEST.

The Bauer drill test differs essentially from the other hardness tests. It indicates the cutting hardness, and has for its object the determination of the workability of metals. The ease or difficulty with which a metal is worked in any machine tool is known to depend upon the toughness and the abrasive qualities as well as on the hardness. The drill test can not, therefore, be strictly considered as being only a hardness test.

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* See p. 726.—ED.

Since the drill test depends upon other qualities than hardness, it is evident that the results obtained should furnish instructive comparisons.

The machine which was used in this work is manufactured by William Keep and was designed for testing the hardness of cast iron. It has for its essential parts a fluted drill driven at constant speed; a table upon which the metal to be tested is placed and from which weights may be suspended for securing desired pressures; and an autographic attachment which traces a curve whose ordinates are some constant multiple of the depth of hole drilled and whose abscissas represent some constant multiple of the number of revolutions required to drill a given depth. The drill was sharpened on a special grinder in such a manner that the rake and clearance of the drill was the same for every test.

The metal to be tested is securely clamped or otherwise held on the table. The weights needed to give the desired pressure are hung on the supports attached to the table. The point of the drill is allowed to drill into the metal before the diagram tracing attachment is thrown into gear. If the drill dulls, it becomes apparent at once from the change of slope of the curve. The drill must then be taken out and sharpened.

Only a few of the metals tested by the other methods could be tested by the Bauer method. Some difficulty was encountered in testing these. This was chiefly due to the fact that they had to be drilled at the same pressures in order to have a basis of comparison. It is a matter of common experience that the pressure applied to the drill and the speed at which it runs must be suited to the metal. Since the method depends upon the rate at which the metal is drilled for the measure of the hardness or workability, it is obvious that the results obtained for different metals are comparable only within very narrow limits.

The line traced by the autographic apparatus shows a linear relation between multiples of the depth of hole drilled and the revolutions required to drill it. The tangent of the angle which this line makes with the Y-axis is taken as the hardness numeral. For metal as hard as the steel drill the line traced is the X-axis. The hardness numeral of such a metal is therefore infinity. The results obtained are given in Table V; each result is the mean of three separate determinations.

5. THE BALLANTINE HARDNESS TEST.

Fig. 8 illustrates the essential parts of the instrument. The hammer *H* is fitted with a hardened cylindrical piece of steel *E*. The two springs *S* and *S′* are attached to the hammer and hold a leaden cylindrical disk 0.6 cm. in depth and 1.1 cm. in diameter in the position shown. The hammer is held in the upper part of the tube *T* by a trigger, not shown in the figure. The anvil *A* in the lower part of the tube is free to move vertically. The upper part of the anvil is a duplicate of the lower part of the hammer. The anvil terminates in a point *P* which rests upon the metal to be tested.

TABLE IV.—RESULTS OBTAINED WITH DIFFERENT CONES IN THE BALLANTINE TEST.

Metal.	Depth of Indentation of Lead Disks in mms. for Circular Cone Anvils of		
	120°	90°	60°
Copper-tin Alloy 1	3.26	3.14	2.99
Copper-tin Alloy 2	3.18	3.23	3.06
Copper-tin Alloy 3	3.20	3.35	3.04
Copper 4	3.18	3.17	2.95

The instrument is supposed to operate and give results as follows: When the hammer is released it falls on the anvil, and the force of the blow drives the point *P* into the test piece. The lead disk is then indented a certain amount depending on the hardness of the metal. If the metal is hard, the disk will be indented more than if the metal is soft. The reciprocal of the depth of indentation of the lead disk is the hardness numeral.

Tests were made on ten different metals, but the indentations of the lead disk were practically the same for the hardest and softest metals.

The instrument as it comes from the manufacturers has an anvil point which is a circular cylinder slightly upset at the end.

Thinking to increase the sensitiveness of the apparatus, anvil points were provided which were circular cones of 120°, 90°, and 60° angular opening, respectively. The results obtained for three copper-tin alloys with these different points are given in Table IV. A comparison of these results shows that the modified cone points did not serve the expected purpose.

The next modification introduced was to reduce the area of the lower end of the hammer and the upper end of the anvil, in approximately the ratio of 2 to 3. That neither of these modifications served the intended purpose is shown by the results given in Table V.

For all of the metals tested the depth of indentation of the metal—not of the disk—was very small, amounting in the case of the softest metal to about 0.3 mm. The energy of fall of the hammer is therefore practically a constant. This energy is spent in indenting the test piece and the leaden disk, and in heat. If a hard metal is tested the indentation of the metal is small, but the work done may be no less than that required to produce a larger indentation in a softer metal. If this is the case there remains only a constant fraction of the energy to do the work required to indent the leaden disk. This constant fraction of the total energy available can be a varying quantity for the different metals, only when the total energy of the falling hammer is itself a variable quantity for every metal. This would be the case only for widely varying depths of indentations of the metal.

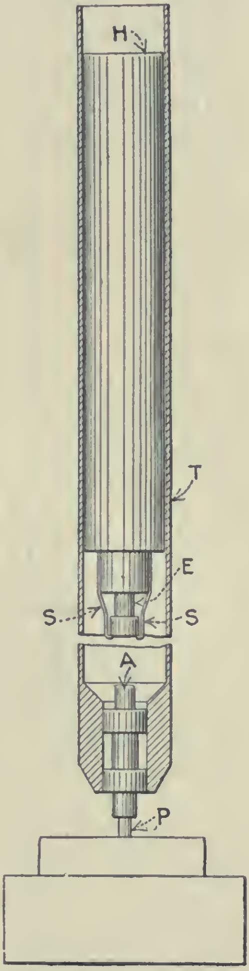

FIG. 8.—Ballantine Apparatus.

DISCUSSION OF RESULTS.

The hardness numerals for all of the metals tested by the five different methods are given in Table V. To enable the reader to see at a glance how these methods compare, the results obtained by the scleroscope, the sphere, and the cone method are represented graphically in Fig. 9. To obtain convenient numbers for graphical representation, the hardness numeral of the scleroscope is multiplied by 100, while that of the sphere, $(P/2\pi tR')$ and of the cone are unchanged. The sphere numeral P/t is unchanged.

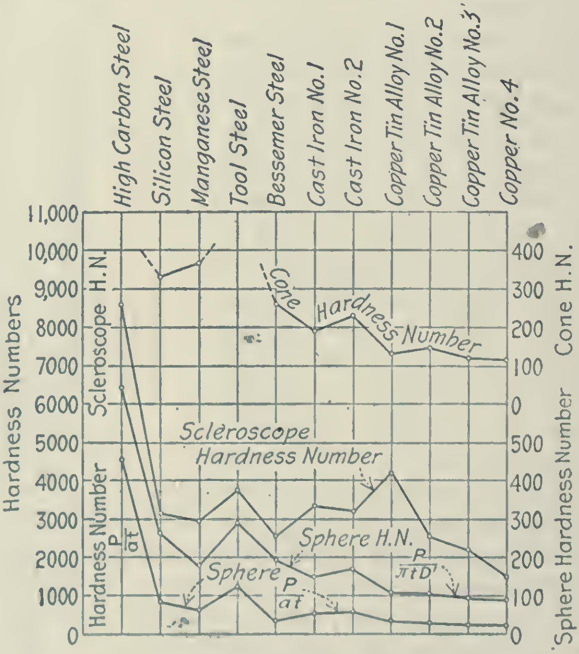

FIG. 9.—Graphical Comparison of Hardness Numerals.

Strictly speaking, no comparison of these quantities can be made; but if the different methods measure the same quantity the lines connecting the points of hardness by one method would be parallel to the lines by another method. In these curves it is interesting to notice that the widest divergence between the Brinell and the Shore method occurs for those metals which contain

TABLE V.—HARDNESS NUMERALS OF DIFFERENT METALS FOR FIVE METHODS OF TEST.

Metal.	Shore Sclero-scope.	Brinell Test.		Cone Test.		Bauer Drill Test.	Ballan-tine Test.
		$\frac{P}{2\pi tR'}$	$\frac{P}{t}$	90°	60°		
High-Carbon Steel. . . .	86.1	641.0	4,550	. . . . .	. . . . .	. . . .	3.23
Silicon Steel.	33.6	261.8	865	331.0	130.0	. . . .	3.03
Manganese Steel.	29.5	179.0	641	368.0	124.0	. . . .	3.25
Cast Iron 1.	33.3	149.0	538	191.0	68.0	. . . .	3.18
Cast Iron 2	32.9	172.3	590	231.0	79.2	2.29	3.23
Bessemer Steel.	26.6	188.0	428	260.0	76.5	. . . .	3.31
Tool Steel.	37.8	289.0	1,230	. . . . .	. . . .	. . . .	3.29
Cu.-Sn. Alloy 1	42.4	110.0	360	130.0	50.1	0.73	3.14
Cu.-Sn. Alloy 2	25.5	105.0	323	149.2	54.1	1.04	3.23
Cu -Sn. Alloy 3	22.1	94.7	289	122.0	48.8	1.24	3.35
Copper 4.	15.0	89.5	235	114.0	38.6	1.88	3.17

elements whose presence is generally supposed to contribute toughness rather than hardness. Consider, for instance, the copper-tin alloys numbered 1, 2, 3 and 4. No. 1 is an alloy of 90 per cent. copper and 10 per cent. tin. By the scleroscope it shows a hardness greater than that of tool steel. By the Brinell test it shows a hardness not much greater than that of ordinary commercially pure copper. No. 2, an alloy of 85 per cent. copper and 15 per cent. tin, is almost as hard as Bessemer steel by the scleroscope. By the Brinell test it is only slightly softer than No. 1. By both the scleroscope and the Brinell method the hardness for the copper-tin alloy series is in the same direction. Alloy 1 is the hardest, with Nos. 2, 3 and 4 following in order. Comparing this with the Bauer drill test hardness numerals we see that the order is reversed. Alloy 1 drills the easiest and 2, 3 and 4 follow in the order named.

The cone and sphere tests give results that are quite concordant throughout. For these two tests a strict comparison can be made, for the results are expressed in the same units.

[For Discussion of this paper see pages 740–743.—ED.]

HARDNESS IN ITS RELATION TO OTHER PHYSICAL PROPERTIES.

By R. P. Devries.

Of the five methods of hardness measurement compared in my first paper,[*] only two will be considered in this paper. They are the Brinell and the Shore methods. Knowledge of the physical properties of metals is necessary to obtain a clear conception of the significance of the different hardness tests. It is also desirable for the reason that correlation may help to avoid the introduction and use of certain tests whose results might as well be determined from some form of test already standardized.

From the complete investigation of thirteen alloy steels furnished by the Halcomb Steel Company and the Crucible Steel Company, only those results will be presented which stand in direct relation to the hardness tests.

The Brinell and Shore hardness tests are alike in that they respectively measure and indicate hardness in the region of permanent deformations. In the Brinell tests the indentation is made under static pressure and the hardness is the resistance offered. In the Shore test the indentation is made by an impulsive force, but it is not evident what the height of rebound indicates. To determine this, thirty different metals were tested by the scleroscope with three different hammers. These hammers were the universal scleroscope hammers, but the striking points of two of them were ground differently. For convenience of reference they were lettered A, B, and C. The striking area of Hammer A was ground so that the radius of curvature was very large. Hammer B was unchanged. Hammer C was ground to the form of a circular cone, the solid angle subtended at the apex being approximately 45°. The heights of rebound obtained for each metal are graphically represented in Fig. 1.[†] The curves are lettered A, B, and C to correspond to the hammer used in obtaining them.

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[*] "A Comparison of Five Methods of Hardness Measurement," p. 709.—Ed.

[†] Acknowledgment is made to the *American Machinist* for the drawings from which the cuts in this paper were made.—Ed.

These curves assign different hardnesses to the metals tested, but do not place the metals in the same position in the hardness scale. The question then arises, why shall the hardness scale established by Hammer B be accepted as correct?

The rebound of all three hammers is, of course, primarily due to the elasticity of the metal and the hammers, but the different forms the three curves assume show that some other variable or variables modify the height of rebound. Since all the conditions except the form of the hammer remained constant in these tests, it is clear that one variable which affects the height of rebound is the form and size of the permanent indentation made in the metal. When the hammer falls upon the metal under test it rebounds from the indentation formed. The cold-working imparted to the metal increases both the elasticity and the hardness. This can be seen by allowing the hammer to rebound twice from the same indentation. The second rebound is higher for all metals. For

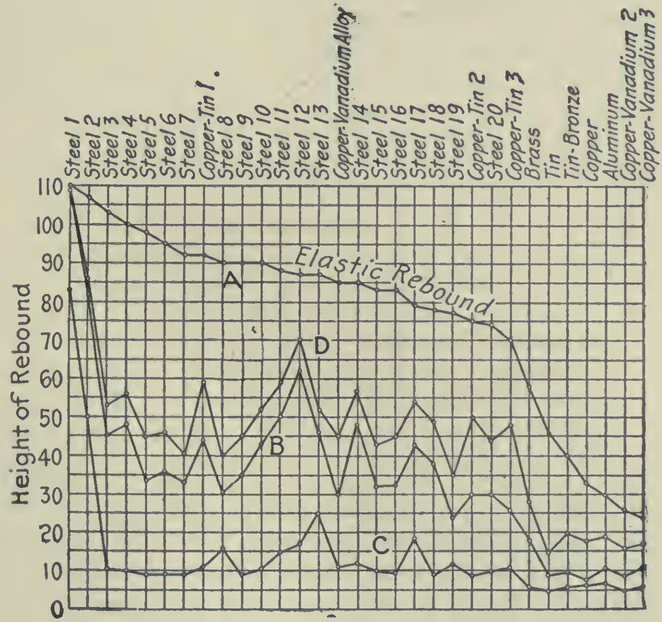

FIG. 1.—Height of Rebound for Three Hammers, Shore Scleroscope.

the steels under investigation the height of rebound is increased from 10 to 30 per cent. of the initial height of rebound.

To show this effect in a different way a bar of steel was stressed in a tensile machine and the heights of rebound were obtained for the different stresses. The results, given in Table I, show that the height of rebound did not increase for stresses below the elastic limit, but that it did increase approximately 18 per cent. of the initial rebound for the highest stress above the elastic limit.

Although these experiments are sufficient to show that the scleroscope responds to the increased elasticity and hardness imparted by cold working, they give no idea of the effect actually produced by cold working of the scleroscope hammer. To determine this, all of the thirty metals previously tested by the three different hammers were indented with a 1.2-cm. sphere subjected to 3,000 kilograms pressure. The metals were then tested in the scleroscope with the universal hammer B. The hammer was made to drop in the center of the indentation formed by the sphere. The heights of rebound are shown in Curve D of Fig. 1. The difference between any ordinate of Curves B and D is the increased height of rebound due to cold working. The 3,000 kilograms static pressure applied to the different metals cold-worked them to different degrees, just as the force of gravity acting upon the scleroscope hammer induces different degrees of cold-working. Curve D, when compared with B, therefore shows the effect which cold-working produces. The differences in rebound on different steels are so small that the forms of Curves B and D are almost exactly similar. If the first rebound of the scleroscope hammer

TABLE I.—SCLEROSCOPE HARDNESS OF VANADIUM STEEL FOR DIFFERENT STRESSES.

(Elastic Limit, 78,000 lbs. per sq. in.)

Stress, lbs. per sq. in.	Scleroscope Hardness.
0	36.5
10,000	36.8
20,000	36.8
35,000	37.0
60,000	37.0
83,000	40.0
98,000	43.0

responds to the cold working it fails to explain the difference of form existing between Curves B and A.

In the scleroscope the kinetic energy available for doing work on the metal under test is a constant. A certain fraction of this energy is spent in doing work of permanent deformation. The energy restored to the hammer by elasticity is the difference between the total and the amount spent in deformation, heat, etc. Now Shore* assumes that the amount of energy spent in producing a comparatively large permanent deformation in a soft metal is greater than the energy spent in producing the comparatively small deformation in the hard metal. The energy restored by elasticity would then be less for the soft metal than for the hard metal and consequently the rebound would be less. This assumption is not substantiated by our knowledge of the behavior of hard and soft metals under stresses beyond the elastic limit. Lieutenant-Colonel Martell has in fact shown conclusively that the energy required to displace a unit volume of metal by impact of any form of pyramidal hammer is greater for hard metals than for soft metals. From the energy standpoint we would therefore be compelled to assume that the energy spent in producing the small deformation in hard metals might as well be equal to or greater than the energy required to produce the larger indentation in the soft metal.

The hammer makes a permanent indentation in every metal. Therefore, in some part of the indentation it comes into contact with metal stressed beyond the elastic limit and in some part with metal stressed just up to the elastic limit. The rebound of the hammer can only be caused by the recovery from strains within or just beyond the elastic limit. The energy restored to the metal must be proportional to the elasticity of the metal. This has been proved by Vincent, who showed that there is a maximum velocity of rebound whatever the velocity of impact may be. If a soft metal has a low elastic limit it might therefore give a less rebound than a hard metal with high elastic limit.

In Fig. 2 the elastic limits of thirteen steels investigated are compared with the hardness as obtained by the scleroscope. These results show conclusively that there is no constant ratio between hardness by the scleroscope and the elastic limit of the material. For example, Steel 1 has a lower elastic limit than Steel 2. It shows a higher tensile hardness than Steel 2; that is, its percentages of elongation and contraction are much less. Its ultimate strength is higher. Yet the Shore instrument as well as the Brinell shows that Steel 1 is harder than Steel 2. The reader can find other examples from the curves of Fig. 2.

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* *American Machinist*, Nov. 14, 1907.

This result was to be expected, because the curves of Fig. 1 show that the size and form of the permanent indentation made by the hammer modify the heights of rebound. The regulation scleroscope hammer also produces different forms and sizes of indentation. For a hard steel the universal hammer is practically a blunt hammer; for a soft steel it is a pointed hammer. The significance of the scleroscope now becomes apparent. The height of rebound which is caused by the elasticity of the metal is modified by the form and size of the permanent indentation. If the indentation

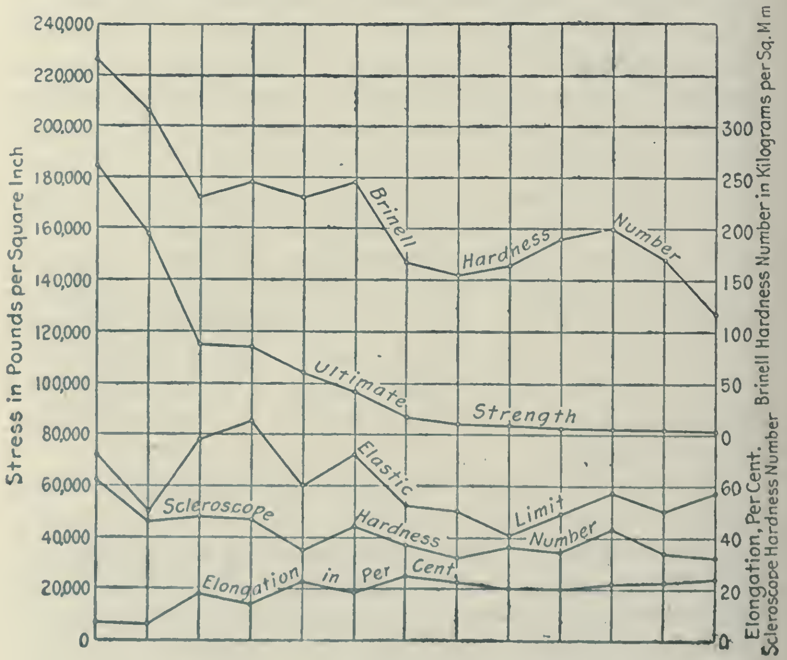

FIG. 2.—Comparison of Hardness and Tensile Tests.

is comparatively large, as it is for a soft metal, the rebound is retarded. If it is small, as it is for a hard metal, the hammer rebounds with greater freedom. For metals like steels in the cast or annealed condition, in which the height of rebound due to elasticity is approximately the same, the scleroscope gives an indication of the resistance offered to permanent deformation. Since the modulus of elasticity of hardened steels is not different from that of ordinary steels, it may easily be seen that the detection of differences in hardness is dependent upon very slight differences of permanent indentations. This explains why hardened steels when tested by the scleroscope should have the surfaces prepared with the greatest care, and even then it will be difficult to obtain accurate results.

Since it has been shown that the indications of the scleroscope are a function of the elasticity within the elastic limit and of the size of the deformation produced beyond the elastic limit, it is clear that the results obtained by the scleroscope are comparable only for the same class of metals. It is also clear that rubber, glass, paper and similar substances should not be compared with metals because for these substances elasticity alone affects the height of rebound.

In Curve A of Fig. 1, there is inserted among the steels four alloys of copper and tin. The elastic height of rebound is the same as that of steels. The fact that they show hardness equal to and greater than that of steels is therefore easily explained.

The relation existing between hardness by the Brinell method and by the scleroscope as shown by the curves is not a constant one even for steels. That there is a relation is evident, since the Shore hardness is a function of resistance to indentation as well as of the elastic limit. For steels of the same elastic limit the relation between hardness by the Brinell and Shore instrument should be a linear one, since the hardness indications are entirely dependent on the resistance to indentation.

The Brinell test owes not a little of its importance to the fact that at different times comparative results between it and the ultimate strength of steel have been adduced to show the existence of a simple relation between the two. The investigations of Kurth* have gone far to show that no simple relation exists, at least not for the two chemically pure metals which he investigated. He has shown, however, that if a metal has its elastic limit raised by repeated stressing in the tensile machine, the ratio of change in elastic limit to change of hardness is a constant for geometrically similar indentations. The curves of Fig. 2 show that no simple relation exists between hardness and ultimate strength of the alloy steels investigated.

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* *Zeitschrift des Vereins Deutscher Ingenieure*, Vol. 52, 1908.

The relation between the elastic limit and the scleroscope results has been discussed. The curves show conclusively that the hardness tests by both the scleroscope and Brinell methods agree with what is generally known as tensile hardness; that is, low elongation with high tensile strength.

It is noteworthy that the ratio between the scleroscope and Brinell hardness is not a constant even for the thirteen alloy steels investigated.

[For Discussion of this paper see pages 740-743.—ED.]

THE PROPERTY OF HARDNESS IN METALS AND MATERIALS.

By A. F. Shore.

The Hardness Test in General.

Hardness testing instruments have during the past few years come into practically universal use, both in this country and abroad. The apparent suddenness of this popular movement may be due to more than one cause. One is surely the development of the automobile, and another perhaps the appearance, at what may be said to have been the psychological moment, of a cheap and rapid method of performing the test itself.

The scleroscope, which the author described at length at the last annual meeting of the Society, is now made semi-automatic and is capable of giving over 5,000 hardness readings per day. In some cases the instruments are used at intervals in checking up material and keeping an intelligent record of the happenings in and about the works. Thus every notable performance of tools or machine parts is investigated for future guidance, while naturally the ultimate causes of undue discrepancies and failures are ascertained. Hardness tests, even when applied to a limited extent, frequently point the way to economies. In this connection, it is quite safe to affirm that the value of the hardness test is not at this time fully appreciated. Yet collectively considered, and judging from the excellent results obtained in every branch of work by those who persevere, there would probably be little left to explain about hardness problems if the whole mass of data could be compiled.

In many instances the hardness test, without any other knowledge of the chemical constituents or of the heat treatment a metal has received, is of limited value; yet if the last-named conditions were known without the hardness value, they would not at all suffice at the present time. For instance, cast iron becomes softer with the addition of silicon up to between two or three per cent. It can then be machined very well if the chilling has been of such a rate as to give a low hardness reading. If all of this silicon is combined, the casting will become more and more difficult to machine as the hardness increases; and if the manganese content is not too high, the hardness test will be a direct indication of the machinability. Very frequently, however, this silicon will appear partially in another form, perhaps as silica, so that the combined silicon will be less. This apparently should give the metal a tendency to re-enter its original or low-silicon condition and thus become harder to the scleroscope or Brinell machine; but since the silica will oppose the action of the combined silicon by making the metal weaker, and also oppose the hardening as recognized by the two above methods, a casting of this kind would show a lower hardness reading. However, since it is now charged with silica, or stone, it has developed a new property, namely, a form of attrition resistance which will destroy the edge of steel cutting-tools. Here the difficulty usually begins, and the tendency is to declare a hardness testing apparatus useless. The question once more arises, "Does any one method lend itself to the true measurement of the several kinds of hardness, of which this appears to be one?" The answer is, that oxidized silicon no more imparts hardness to a casting as a *stress-resisting body* than do crystals of carborundum when in a bar of soft lead. As a further example, soft lead charged with an abrasive will quickly destroy the hardest steel cutting-tool. The question now is, "How can we measure the machinability of cast iron when, as is usually the case, the chemical content is unknown?"

Since we have seen that in this instance the measurement of either hardness or attrition resistance alone will not suffice, it is evident that both of these properties must be determined. The scleroscope will show the physical or stress-resisting hardness. As to the attrition resistance, it is obvious that the diamond scratch method will indicate physical hardness rather than attrition resistance, because the diamond is so hard that it will readily cut abrasives which would destroy a steel tool. We can, however, use the steel drill method, in which case certain conditions must be observed. For example, the hardness of the drill must be adapted for testing the particular material; that is, the drill must be just hard enough to penetrate a few inches under the most favorable conditions. The same drill, when used with a casting of the same physical hardness but with a higher attrition resistance, would fail quicker; and if the percentage of abrasive is very high, it would fail practically at once. Thus, a $\frac{5}{16}$-in. drill fifty per cent. harder than cast iron will, under favorable conditions, drill a number of inches of this material, but would fail quickly if the iron contained abrasives which would make machining difficult. The proper drill to be used also depends somewhat upon the toughness; that is, a drill suitable for testing cast iron or even cast steel, would not be suitable for testing forged tool and alloy steels. The ratio of the hardness of test drills to that of forged steels should be about 2 to 1. The hardness required in regular shop tools is from 2½ or 3 to 1, thus showing that the test drills above mentioned are comparatively soft.

The best machine, perhaps, for making these drill tests for machinability is an ordinary sensitive drill press. A $\frac{5}{16}$-in. drill is perhaps the most convenient. The drilling speed should be normal, and such lubricants should be used as are employed in practice.

Exhaustive tests have often been made to determine the efficiency of a drill without knowing the hardness of either the drill or the material to be cut. For example, the author once witnessed a series of tests to destruction with both carbon and high-speed steel drills, to determine the importance of the hardness test on drills. The results obtained were strikingly irregular. Some drills cut hundreds of inches, while others cut only a few inches. An investigation was made to ascertain the reasons; it was discovered that the material drilled was ordinary slag-wrought-iron, and that the sudden failures were due to an occasional vein of flinty slag. The tests were then repeated on carefully annealed tool steel for the sake of its uniformity. This test, however, involves such a vast amount of work and permits the entrance of so many unknown and unfavorable factors, such as the failure to supply oil uniformly at all times, that it is apparently not practical. Hence the need of adapting the hardness of the tool to that of the material in testing the machinability of the material, and of adapting the hardness of the material to that of the tool itself in testing the staying powers of the tool against the material.

It is of course understood that in machining metals, cutting tools of at least an average quality must be used. The author wishes to lay special stress upon this point, because the use of an inferior tool, or a superior tool which had the temper drawn on the edges as the result of more or less careless dry grinding, gives a wrong indication of the machining qualities of the material, which reflects not only upon the hardness test in general, but upon the manufacturer of the material as well.

THE HARDNESS TEST AS APPLIED TO ROLLING-MILL PRODUCTS.

Hardness testing is now universally practiced by the rolling mills, particularly where rolling or drawing is done cold, and is especially applicable to non-ferrous metals. In this respect the hardness test has become a decided convenience in several ways.

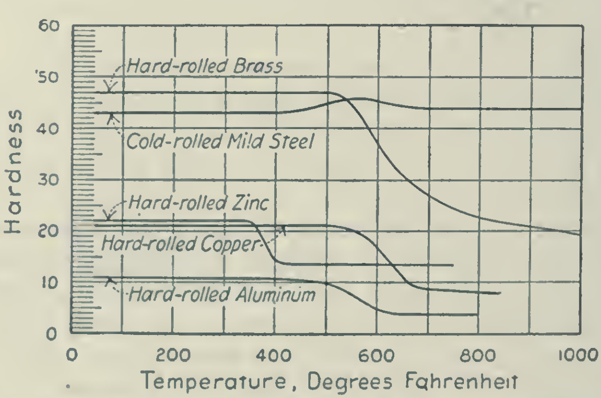

FIG. 1.—Effect of Heat Treatment on the Hardness of Cold-Rolled Metals.

In ordering material to specification it was formerly, and in fact is largely to this day, the practice of the consumer to send samples of the metals which have given satisfaction in the sheet, bar, or wire, with the understanding that the mill would be able to make a similar article. Formerly, however, this was not entirely possible, for the hardness was an unknown quantity; and even though the foreman could tell by experience very nearly what was wanted, it was difficult for him to convey his ideas to the workmen so that they in turn could carry out his orders. The tension test helped to some extent, but in pre-determining a given hardness it was not wholly feasible to clip off test pieces from the marketable sheets, etc., as they were being passed through the rolls or as they were being heat treated. With the quick hardness test, the method of pre-determination of the characteristics of the product becomes very simple. Orders that specify the hardness numbers can receive immediate attention by the mill foreman, while those that refer to samples are sent to the testing department, where the hardness tests are made and the results marked on the shop orders.

There are several methods of pre-determining hardness in cold-rolled material in which the men become expert provided they have a suitable measuring instrument to guide them. After the billet has been selected, a certain amount of rolling will produce a given hardness, while any over-hardness produced can be readily corrected by annealing to certain temperatures which are different for different kinds of alloys or metals. Fig. 1 shows the effect upon the hardness of different metals of re-heating after cold rolling. It will be seen that cold-rolled steel acts differently from non-ferrous metals and even high-carbon steels which have been hardened by quenching; instead of the hardness decreasing at the quenching point, where tool steel, copper or brass rapidly become soft, it becomes slightly harder. The hardness does not drop until very close to the critical or non-magnetic point, when it drops very suddenly to its normal level.

THE HARDNESS TEST AS APPLIED TO CUTTING TOOLS.

Hardness testing is now commonly practiced in connection with the manufacture of tools, but not as extensively as it might be. Progress in this direction has been somewhat slow, due to lack of knowledge or the unwillingness to benefit by existing knowledge. In some instances the hardness test alone will not indicate the cutting qualities of the tool, and it is on this account that some have thus far not made use of the test. A number of manufacturers are advancing the following argument: Three drills may be selected, measuring, for example, 90 hard each, while they have been treated in widely different ways. Drill 1, a Bessemer steel containing about 0.50 per cent. carbon, is hardened to 90, where it is left without tempering. This will make a very poor tool. Drill 2, containing 1.10 per cent. carbon, is hardened to 105 and drawn down to 90. This will make a drill of remarkable strength and cutting efficiency at high speeds. Drill 3, taken from the same bar as Drill 2, has been grossly over-heated and allowed to soak in the fire, so that it is crystallized and shows an initial hardness of 90. Without tempering, it will show the same reading as the best possible drill at this hardness, yet it is so brittle and weak that its edge would crumble up as soon as used. It is claimed by these manufacturers that owing to such conditions it is not possible to pick the best tool out of a series of poor ones. This is a mistake, however, for the following reasons: In the first place, it would not seem rational for any manufacturer to knowingly work up Bessemer steel for the purpose of making good tools, along with other steels which are really good, with the hope of being able to select the good tools from the bad ones after they are finished. Again, it would not seem rational for any manufacturer who used fine steel to permit gross neglect in the heat-treating department, just because they are able to pick out the spoiled tools in the inspection room. However, should an error have been made in the first place by the mixing of an inferior steel with a good steel, the good steel could be easily selected immediately after quenching a sample by its superior hardness, providing the heat treatment had been right. Again, if the hardness test had not been applied until the tools had all been tempered at a known temperature, it would still be possible to select the best tools by their superior hardness, for it is a fact that temper-drawing is more injurious to low-carbon steels than to high-carbon steels, under the same heat treatment. If nothing should be known about the heat treatment of the carbon steel, a good tool may be selected as that which shows a high scleroscope reading and a certain amount of fileability. This is indicative of a high initial hardness, and also of considerable temper drawing, to which the strength of the tool is due.

A hardness test on high-speed steel may be misleading unless some judgment is exercised. For example, two drills are hardened, showing respectively 90 and 100 hard after leaving the quenching bath. If no further test is made the better tool cannot be selected, for it is the red hardness in this steel that counts. Consequently we must test the red hardness as follows: Reheat the sample to about 600° F. and test again. The one that shows a high hardness after this treatment will make the most rapid cutting tool, assuming that its hardness is fitted for the work. Here again it will be seen that if the hardness test is not properly applied one can easily be misled.

THE HARDNESS TESTING OF RUBBER AND SIMILAR MATERIALS.

In the discussion of the writer's paper on the scleroscope, read before the Society at the last annual meeting, the statement was made that the rebound of the scleroscope drop hammer on rubber was as high as that on cast iron and steel. It was the author's contention at that time, as now, that the scleroscope can only be applied to materials which are capable of taking a permanent set under the impact of its drop hammer; and since soft rubber does not take a permanent set, the scleroscope cannot measure its hardness.

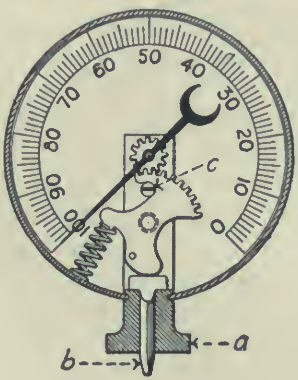

FIG. 2.

During the past year we have devised an instrument for measuring the hardness and resilience of rubber and similar materials, which is shown in Fig. 2. In testing the hardness, a blunt-pointed pin *b* is pressed into the rubber by means of a standardized spring, and the depth of penetration of the pin indicated on a dial, as is clearly shown in the figure. This dial reading is a measure of the hardness of the material. In testing the resilience, the pin *b* is locked in its normal position and made to penetrate the material to the full length of the projecting point. The locking button *c* is then released, allowing the pin to yield under the pressure of the rubber. The movement of the indicator arm is a measure of the resilience of the material.

GENERAL DISCUSSION ON HARDNESS.

The Chairman. MR. R. W. HUNT (*in the Chair*).—These papers certainly are very interesting and of great importance. They show an immense amount of patient labor and observation, and I know they will make a valuable addition to our literature. They are worthy of careful attention. Will Mr. Howe kindly open the discussion?

Mr. Howe. MR. H. M. HOWE.—We must all agree with what the Chairman has said about the value of these papers, the labor that they represent, and the importance of the subject. We all appreciate that if we could get a quick and cheap test of this kind to displace the regular tensile test, it would be a very great advantage. The expense of the Brinell test or the Shore test is, of course, insignificant compared with that of the tensile test. It is not to be expected that such displacement will ever be universal; for there will be cases in which we will want to go beyond the indications of a test like either the Brinell or Shore test, which report on only one property, and use the tensile test, because this reports on four somewhat independent properties, and therefore is much more likely to detect faulty material. If all four of these properties are normal, that raises a greater presumption that the material is normal than would be raised by the certificate of any hardness test that the hardness alone is normal.

Yet there are cases in which the presumption is very strong that, if the hardness is normal the material is otherwise normal; and here these hardness tests should have great value. For instance, where the open-hearth or Bessemer process is carried out with great regularity under very constant conditions, this presumption may be justified, especially if human life would not be endangered by any failure of the product in service.

In addition to their cheapness, these tests often have a great advantage in being applicable where one could not apply a tensile test, because of the fact that the tensile test requires specimens of considerable size, which often cannot be got out of the object which we are making without destroying that object, or without going to very great expense beforehand in making the object sufficiently large to enable us to get a large test piece. All this is very expensive. **Mr. Howe.**

But when we come to apply these tests to widely varying conditions we must expect to find surprises and disappointments. It is very generally told, and I believe it is true although I will not vouch for it, that one of the best materials to saw marble with is soft wrought iron, or soft steel. We know that they have been used to a very great extent. Glass cutters formerly cut glass with copper disks by putting sand on the disks; of course it was not the hardness of the disk nor the hardness of the saw that caused either of these materials to be selected; nevertheless, under the particular conditions, the soft steel or wrought iron saw and the copper disk seemed to resist abrasion better than much harder materials. At any rate they were used to a very great extent for purposes for which it would be supposed a hard material was needed, and yet neither the saw nor the disk was hard in the way in which we generally recognize hardness. The Brinell and Shore tests would give no indication of the fitness of the soft iron or copper for doing this work.

We feel called upon to demand of some of these various testing instruments, that they shall demonstrate their trustworthiness by the most convincing proof; and I think that the Shore instrument stands out prominently in that class; I mean that the value of its report as an index of service usefulness is far from self-evident.

Hardness does not represent any one quality. It represents under a wide range of conditions of use the resultant of an aggregate of properties. If you have a conglomerate material like steel which is composed of two or three or possibly more distinct constituents, then the important properties which contribute to hardness are the cohesion between the particles of each constituent among themselves, and the adhesion between adjoining particles of the different constituents where they abut against each other. Such cohesion and adhesion underly the tensile strength and elastic limit. Hence the use of hardness tests in place of tensile tests. So we ask of these hardness tests whether they really do report truly as to cohesion and

Mr. Howe. adhesion. We can very readily understand that when, as with the Brinell test, you push into the material, the resistance to that push must be very closely bound up with this cohesion and adhesion. In the case of the scleroscope you do not find anything of the kind; the cohesion is not necessarily related to the rebound. It may be or it may not. If it is, very convincing evidence of that fact should be required.

It is an interesting fact that the first published results that I know of—there may be earlier ones—of the test by the Brinell as against the Shore instrument, gave results which were almost parallel. They were like hysteresis curves of the same family, and almost strictly parallel throughout. This is encouraging, because here is an indication that both of these different tests give harmonious results, which, as far as it goes, tends to show that they both give a fair measure of the cohesion.

The things to bear in mind in all of these hardness tests is, first, that there is a great need for them, they fulfill a great want; second, the kind of tests which is going to be most valuable will differ according to differing conditions of use. It remains to be determined, to be shown by actual test, actual empirical test in the present state of our knowledge, which instrument will give results which are most truly indicative of the value of the material for any particular purpose in mind. One instrument may give the best and most trustworthy indications of the fitness of the material for the rim of a driving wheel. Another instrument may give the best indication as to the power of the rail itself to resist abrasion; and so on, for fifty different conditions. We cannot predict which instrument will fit all of these conditions and will give the most indicative results, results which will indicate most conclusively, most trustworthily, the relative value of the different materials to undergo the several conditions of service.

The Chairman. THE CHAIRMAN.—Will Mr. Stoughton, who participated in the joint preparation of one of the papers, add to the discussion of the subject?

Mr. Stoughton. MR. BRADLEY STOUGHTON.—Mr. Chairman, there has been so much said on this subject and such an amount of data presented, that really I do not know that I can have much more to add. I may say one word as to the test that Mr. Macgregor

Mr. Stoughton. and myself carried on in regard to the comparison between the scleroscope and the old Turner sclerometer, which is, as you remember, a diamond point drawn across the surface. In our case we did not follow out the original Turner method of drawing it across the surface and back to determine what weight would give the first visible scratch, but instead drew it across under a constant and uniform load in all cases, and then measured the width of the scratch under the microscope. We carried on that same series of tests on four different materials, and it is interesting to know that the reading of the scleroscope and the sclerometer were roughly comparable in the four cases. The steel in these cases varied all the way from a very hard specially heat-treated steel to a soft steel.

Mr. Fowler. MR. G. L. FOWLER.—Mr. Macgregor spoke of some abrasion tests that he made in connection with the hardness test. I should like to know the number of revolutions and whether he runs his whetstones wet, or dry; also, what provisions were taken in order to maintain a constant temperature of the metal to be abraded? Those are a few points that have a very important bearing on the rate of abrasion.

I should also like to ask whether the authors make any connection between the power of a metal to resist abrasion, and its hardness?

Mr. Macgregor. MR. J. S. MACGREGOR.—We recognize a distinct difference between hardness as such and abrasion, and have made no attempts to correlate the two.

In answer to the question as to the temperature of the specimens under abrasion, the tests were all conducted in a room at fairly constant temperature, and the stones were operated dry. At no time was the temperature of the specimens allowed to rise to a degree sufficient to cause over-heating.

Mr. Devries. MR. R. P. DEVRIES.—There are as many kinds of hardness as there are different kinds of stress by which hardness can be determined. The hardness tests in common use do not measure resistance to abrasion. Although manganese steel rails show great resistance to abrasion this property is not determined by the Brinell or Shore hardness tests. The many claims made for certain forms of hardness tests have brought about confusion in this respect.

SPECIFICATIONS AND GRADING RULES FOR DOUGLAS FIR TIMBER: AN ANALYSIS OF FOREST SERVICE TESTS ON STRUCTURAL TIMBERS.

By McGarvey Cline.

This discussion of the tests on Douglas fir is presented to the Society since it bears more or less directly on the work of Committee D-7 on Standard Specifications for the Grading of Structural Timber, formerly Committee Q. The series of tests upon which it is based is more completely discussed in Forest Service Bulletin 88, which will be available for distribution some time during the present summer.

A complete analysis of all tests on structural timbers made at the laboratories of the Forest Service is nearing completion and will doubtless be available to the public within the next year. In this publication the general problem of grading structural timber will be taken up.

Defects.

The defects encountered in green Douglas fir lumber may be classified as follows: (1) Pitch seams and shakes; (2) cross grain; (3) knots.

These defects are practically the basis of all grading rules and their number and character largely determine the use to which the product of the sawmill is put. Material which is free from them is usually manufactured into finish, flooring, and other high-grade products in which the appearance of the wood is of prime importance. It is also generally believed that, aside from their effect on the appearance of the wood, these defects influence greatly the strength and other mechanical properties of the timber in which they occur.

A special series of tests was made on 5 by 8-in. car sills with a 15-ft. span, and on small specimens cut from the uninjured parts of the sills after they were tested. Tests on the smaller specimens were for the purpose of studying the influence of such elements as rate of growth, summer wood, etc., and to determine the quality of the clear wood in each of the car sills tested. The following discussion of the influence of defects on the strength of Douglas fir is based largely on this series of tests. On large beams of this series the load was applied at the third point and a careful sketch was made of the beam showing the number, size, and location of defects and the manner in which it failed.

1. *Pitch Seams and Shakes.*—The pitch seams noted in the specimens were narrow, although in some cases several inches long. The seams invariably occurred between two annual rings, causing only a slight disturbance in the grain of the specimen. In no case did their presence have any apparent effect on the results of the tests. It is reasonable to suppose, however, that large pitch pockets or pitch seams which cause a decided derangement in the grain of the specimen will prove detrimental to its strength. Shakes are seldom observed in green Douglas fir timbers, partly due to the fact that they seldom become evident until the wood is partially seasoned. The presence of shakes near the neutral axis of the beam, i. e., about midway between the top and bottom faces, may seriously influence the strength of the specimen by decreasing its resistance to horizontal shear. The fact that none of the green sills failed in horizontal shear indicates the absence of such shakes in the specimens tested.

2. *Cross Grain.*—Cross grain may be divided into two general classes:

(*a*) *Diagonal Grain.*—In sawing lumber when the plane of the saw is not approximately parallel to the axis of the log, the grain of the lumber cut is not parallel to its edges; in such cases the grain is termed diagonal.

(*b*) *Spiral Grain.*—In Douglas fir, as well as in other species, the fibers composing each year's growth are in some trees arranged spirally instead of vertically. The reason for this is not definitely known. The danger of spiral grain as a defect depends largely on what might be termed the pitch of the spiral. Its presence is not generally noticeable from a casual inspection of the piece, since it does not show in what is commonly spoken of as the visible "grain" of the wood, because the prominent "grain" in soft-wood lumber is in reality a sectional view of the annual rings cut longitudinally, and since the annual rings are not distorted by spiral grain, the appearance is not changed thereby. A careful inspection, however, of the medullary rays, particularly on the tangential or bastard section, will invariably reveal spiral grain, since the rays necessarily incline with the spiral direction of the fibers around the trunk. Spiral grain may readily be determined also by splitting a small piece radially.

Knots frequently cause a considerable disturbance of the grain, and such disturbances are also called cross grain. They seriously affect the strength of a beam when they occur so near the edge as to interrupt the continuity of the grain on the edge. All forms of cross grains may be regarded as serious defects. Of the 85 car sills composing this special series of tests 23 showed cross grain, and in 12 cases the failure was due to cross grain. In the other 11 cases the full compressive strength of the wood was developed, i. e., the beam first failed in compression before the final failure due to cross grain occurred. It was observed that beams showing cross grain to a marked extent were almost invariably weak.

3. *Knots.*—The effect of knots on the strength of timber is largely dependent on their position, size, and number, and on the condition of the wood around them. These elements are so intimately associated with each other and occur in such infinite numbers and combinations, that it has been impossible to make specific deductions regarding the relative effect of different kinds and sizes of knots, but a study of their general relation to the results of the tests and to the failures warrants certain deductions that may prove of help in framing grading rules and specifications.

(*a*) *Effect on Shearing Strength and Strength in Compression at Right Angles to Grain.*—The effect of knots on the shearing strength of wood was not investigated directly. The tests on air-dried beams, however, in which pieces very frequently failed in horizontal shear, indicate that knots do not decrease the shearing strength of the wood but rather tend to increase it by acting as pins, thus preventing the sliding of one surface over the other. They also have practically no influence in decreasing the strength in compression at right angles to the grain.

(*b*) *Effect on Strength in Compression Parallel to Grain.*—Table I shows the effect of different classes of knots on the strength in compression parallel to the grain. In this table the strength of compression blocks containing knots from ½ in. to 1½ ins. in diameter and of blocks containing knots over 1½ ins. in diameter, is compared with the strength of clear specimens. This comparison indicates that the presence of knots appreciably decreases the strength of Douglas fir in compression parallel to the grain, the decrease being approximately 20 per cent. in the case of knots greater than 1½ ins., and 5 per cent. in the case of knots less than ½ in. in diameter,

TABLE I.—DOUGLAS FIR, GREEN MATERIAL WITH DEFECTS.

Average values. Compression parallel to grain. Size, 6 x 6 x 18 ins.

CONDITION OF MATERIAL.		Number of Tests.	Rings per Inch.	Percentage Summer Wood.	Percentage Moisture.	Lbs. per Cu. Ft.		Compressive Strength at Elastic Limit, lbs. per sq. in.	Crushing Strength at Maximum Load, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.
						As Tested.	Oven Dry.			
Clear.	High, 10 per cent . .	13	21.7	53^{11}	36.1	43.4	33.4	4,390	4,959	2,707,000
	Average.	130	11.8	42^{109}	30.4	38.1	29.2	3,099	3,918	1,321,000
	Low, 10 per cent . . .	13	5.3	31^{11}	25.7	33.2	25.8	2,070	2,850	623,000
Pin Knots (knots ½ in. or less in diameter).	High, 10 per cent . .	6	21.0	52^{5}	36.6	43.6	33.1	3,808	4,525	2,681,000
	Average.	62	10.4	41^{46}	31.6	37.7	28.6	2,931	3,698	1,401,000
	Low, 10 per cent . . .	6	4.7	33^{5}	27.4	32.5	24.5	2,017	2,857	642,000
Standard Knots (knots ½ to 1½ ins. in diameter).	High, 10 per cent . .	23	17.5	55^{17}	39.7	43.8	33.4	3,798	4,538	2,467,000
	Average.	227	9.0	41^{168}	30.9	37.8	28.9	2,708	3,386	1,187,000
	Low, 10 per cent . . .	23	4.4	26^{17}	24.7	32.7	31.0	1,832	2,428	576,000
Large Knots (knots over 1½ ins. in diameter).	High, 10 per cent . .	10	19.0	53^{8}	37.2	44.5	33.8	3,426	4,367	2,119,000
	Average.	97	9.4	39^{83}	29.9	38.0	29.3	2,406	3,062	940,000
	Low, 10 per cent . . .	10	3.5	24^{8}	24.7	33.0	25.5	1,669	2,129	472,000

(*c*) *Effect on Beams.*—Efforts to determine the influence of knots on the strength of beams are attended with considerable difficulty due to the complicated distribution of stresses. Fig. 1 shows the modulus of rupture, fiber stress at elastic limit, and modulus of elasticity for each of the green car sills tested. The average modulus of rupture and modulus of elasticity, as well as the crushing strength, secured from the tests on small, clear beams, 2 by 2 by 30 ins., and compression specimens 2 by 2 by 8 ins., cut from each of the car sills, are also shown. The following explanation will assist in interpreting the diagram:

1. The tests are arranged in the diagram from left to right in

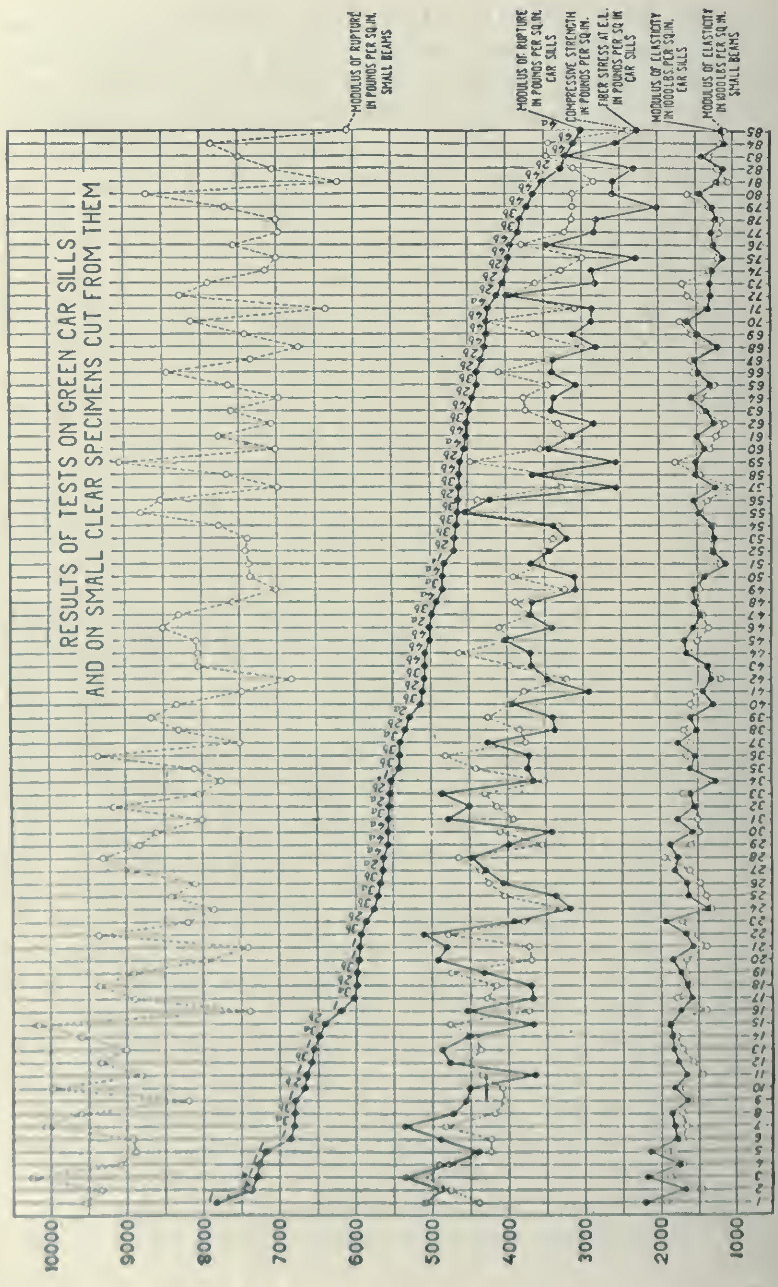
RESULTS OF TESTS ON GREEN CAR SILLS
AND ON SMALL CLEAR SPECIMENS CUT FROM THEM
MODULUS OF RUPTURE IN POUNDS PER SQ.IN. SMALL BEAMS
MODULUS OF RUPTURE IN POUNDS PER SQ.IN. CAR SILLS
COMPRESSIVE STRENGTH IN POUNDS PER SQ.IN.
FIBER STRESS AT E.L. IN POUNDS PER SQ IN CAR SILLS
MODULUS OF ELASTICITY IN 1000 LBS. PER SQ.IN. CAR SILLS
MODULUS OF ELASTICITY IN 1000 LBS PER SQ.IN. SMALL BEAMS
10000
9000
8000
7000
6000
5000
4000
3000
2000
1000

FIG. 1.

order of the breaking strength, or modulus of rupture, of the car sills, and all the other results secured from each car sill are shown on the same vertical line with its modulus of rupture.

2. The Arabic numerals along the horizontal axis are for reference only.

3. The Arabic numerals and letters at the points indicating the modulus of rupture for each car sill indicate the classification of the car sills with respect to the defects they contain. The scheme of classification used is as follows, each beam being divided into the volumes indicated by Fig. 2:

Class 1. Clear.

Class 2 (*a*). Containing knots in Volume 3 only, all knots being less than 1½ ins. in diameter.

(*b*). The same as Class 2*a* except that some of the knots are 1½ ins. or greater in diameter.

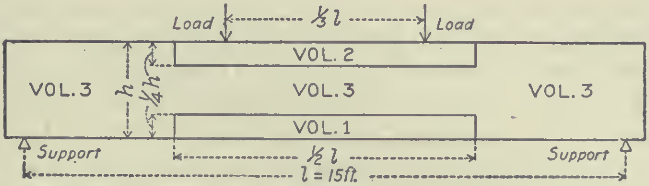

FIG. 2.

Class 3 (*a*). Knots less than 1½ ins. in diameter in Volumes 2 and 3 only, Volume 1 being clear.

(*b*). The same as Class 3*a* except that some of the knots in Volumes 2 and 3 are 1½ ins. or over in diameter.

Class 4 (*a*). Scattered knots in all volumes, those in Volumes 1 and 2 being less than 1½ ins. in diameter.

(*b*). The same as Class 4*a* except that some of the knots in Volumes 1 and 2 are 1½ ins. or greater in diameter.

While the above classification is based, in a general way, on the size and location of the knots it also indicates fairly well the total number of knots occurring in the different specimens; i. e., in general, timbers in Classes 3 and 4 contain more knots than those in Class 2.

The attention of the reader is called to the following points:

1. That the moduli of elasticity for the large beams and the small clear beams cut from them are approximately equal. This would seem to indicate that the modulus of elasticity is not influenced by the presence of defects in the large specimens. In other words, knots do not affect the stiffness of Douglas fir timbers, this factor apparently being dependent almost entirely upon the quality of the clear wood.

2. That the fiber stress at the elastic limit for large beams does not vary with their modulus of rupture, but is, with very few exceptions, practically equal to the crushing strength of the small clear specimens. This is an extremely significant fact because it indicates that the elastic limit of Douglas fir beams is not greatly affected by the presence of knots. It will be noticed, however, that in the car sills having a low modulus of rupture, in general the elastic limit falls below the crushing strength of the clear wood, while in the sills having a relatively high modulus of rupture the fiber stress at the elastic limit frequently exceeds the crushing strength of small clear specimens. This would apparently indicate that the elastic limit is slightly influenced by the presence of defects.

3. By comparing the modulus of rupture for large beams with the modulus of rupture and crushing strength of the small clear specimens, it will be seen that in Class 1 the modulus of rupture is considerably higher than the crushing strength of the clear wood, falling approximately midway between this function and the modulus of rupture for small beams. As the number and seriousness of the defects in the large beams increase, it will be observed that the modulus of rupture approaches in value the crushing strength of the clear wood and the elastic limit of the beam. In the low-grade material there is very little difference between the modulus of rupture of large beams and their elastic limit, and the crushing strength of the clear wood. It will also be observed that, as the modulus of rupture of the large beams approaches in value the crushing strength of the clear wood, the difference between it and the modulus of rupture of the small clear beams increases. These relations apparently indicate that the presence of knots influence most the breaking strength of the beams in which they occur.

4. The beams in Class 4 have knots within their middle half on the bottom face or within two inches of the bottom edges. It will be noticed that in the lower class of large beams the modulus of rupture is relatively much closer to the elastic limit of the beam and to the crushing strength of the clear wood. In Class 4*b*, beams containing large knots on the tension face, this tendency is most marked.

5. It will be observed that in Class 1 the modulus of rupture is almost 50 per cent. greater than the fiber stress at the elastic limit and the crushing strength; this fact, in conjunction with the one just stated, would seem to indicate that the principal effect of knots upon timber beams is to decrease the energy required to break the beam after the elastic limit is reached. The ability to resist rupture after the elastic limit is passed indicates toughness.

Large knots and knots in great number are associated with and indicative of poor wood. Fig. 1 shows a gradual decrease in the modulus of rupture of small clear beams and in the crushing strength of clear specimens cut from the lower-grade timbers containing such knots. As the quality of the wood fiber thus decreases, a general dropping off in the compressive strength and the fiber stress at the elastic limit is observed. It is noticeable that even in the beams having a very low modulus of rupture, when the tests on small clear specimens indicate a good quality of fiber, the elastic limit of the beam and its modulus of elasticity are almost invariably higher than they are in beams having approximately the same breaking strength but in which the quality of the wood fiber is poor.

Table II summarizes the results of tests according to the classes indicated on Fig. 2. It also gives the maximum and minimum values for each class. This table emphasizes the following points:

1. The average properties of Class 1, or clear beams, are considerably higher than the average for the other classes containing defects more or less serious in character. The range indicated by the maximum and minimum values, however, is practically as great in clear material as it is in material containing defects, and clear material is sometimes weaker than material containing knots.

2. It will be noticed that in each of the other classes the

average values for the (*a*) group are considerably higher than the average of the (*b*) group, indicating that in general the size of a knot is significant in determining its influence upon the strength of the timber in which it occurs.

3. Class 3, group for group, is slightly superior to Class 2. It appears, therefore, that knots in Volume 2 are at least no more serious than knots in Volume 3. (Similar analyses made, however, on tested loblolly pine show that knots on the compression face decrease the strength slightly more than knots in Volume 3.)

TABLE II.—DOUGLAS FIR CAR SILLS, CLASSIFIED ACCORDING TO SIZE AND POSITION OF KNOTS.

Green material. Size, 5 x 8 x 180 ins.

Grade		Number of Tests.	Rings per Inch.	Percentage Summer Wood.	Percentage Moisture.	Lbs. per Cu. Ft.		Fiber Stress at Elastic Limit, lbs. per sq. in.	Modulus of Rupture, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.
						As Tested.	Oven Dry.			
Class 1	Maximum	....	27.0	62	33.1	45.9	35.5	5,350	7,840	2,220,000
	Average	14	16.1	45	30.2	39.7	30.5	4,588	6,586	1,746,000
	Minimum	....	5.9	33	28.7	32.8	25.4	3,650	4,810	1,124,000
Class 2*a*	Maximum	....	13.9	62	30.5	40.5	31.0	4,500	5,640	1,795,000
	Average	4	9.2	43	29.5	37.2	28.8	3,898	5,370	1,619,000
	Minimum	....	6.9	33	28.5	34.8	27.0	3,400	5,010	1,538,000
Class 2*b*	Maximum	....	15.0	57	38.2	41.7	32.1	4,870	6,660	1,923,000
	Average	17	9.4	42	30.0	36.6	28.2	3,484	4,905	1,520,000
	Minimum	....	4.9	32	25.4	32.0	24.7	2,300	3,270	1,124,000
Class 3*a*	Maximum	....	21.2	42	33.8	42.5	33.3	5,370	6,810	1,841,000
	Average	7	15.2	37	30.1	37.7	29.0	4,151	5,833	1,702,000
	Minimum	....	8.8	32	27.6	35.3	27.4	3,090	4,840	1,547,000
Class 3*b*	Maximum	....	22.2	51	32.8	39.1	30.3	5,090	6,800	1,855,000
	Average	19	10.1	35	29.4	36.2	28.0	3,682	5,151	1,450,000
	Minimum	....	4.3	27	25.7	33.9	25.9	2,550	3,800	1,236,000
Class 4*a*	Maximum	....	13.0	45	44.6	38.1	29.3	4,480	5,630	1,865,000
	Average	8	8.0	34	31.9	34.7	26.4	3,398	4,785	1,502,000
	Minimum	....	4.7	26	25.6	30.4	22.6	2,250	3,000	1,155,000
Class 4*b*	Maximum	....	16.6	56	40.1	39.0	30.6	4,020	5,080	1,653,000
	Average	16	8.2	38	29.9	35.0	27.0	3,066	4,161	1,386,000
	Minimum	....	3.5	20	27.2	31.1	23.8	2,000	3,000	1,111,000

4. Class 4, group for group, is considerably weaker than the other classes, indicating decidedly that knots in Volume 1 are considerably more serious than knots in other parts of the beam.

FAILURE.

The manner in which the test specimens fail is also indicative of the relative seriousness of different defects. The four types of failures observed in the special series of tests on green car sills are as follows:

1. *Compression.*—This failure consists of the buckling of the fibers in the upper half of the beam. It usually begins to form on the top shortly after the elastic limit is passed and as it develops extends downward in the piece, sometimes almost reaching the middle of the cross section before complete failure occurs. Frequently two or more compression failures develop almost simultaneously.

2. *Cross-Grain Tension.*—Wood is comparatively weak in tension at right angles to the grain. This is especially true of straight-grained woods that split readily, such as Douglas fir. When the beam has a diagonal or a spiral grain the internal stresses develop a tensile force which acts approximately at right angles to the grain. This condition almost invariably results in what is termed a failure in cross-grain tension.

3. *Tension Failure.*—When the fibers on the bottom side of the beam are torn apart in a manner similar to that in which the fibers in a string are torn apart when it is broken, the failure is called a tension failure.

4. *Horizontal Shear.*—Sometimes the beam under load splits, the split extending from the end of the beam to the middle, and in some cases from both the ends. This is called a failure in horizontal shear. Such failures, it will be shown later, are comparatively common in air-dried material and in green timber where the ratio of the height of the beam to the length of span is relatively large. None of them were observed in the tests on green car sills.

Of the 85 car sills tested in the special series of tests upon which this discussion is based, 68 cases or 80 per cent. failed in compression first. In 51 of the cases the compression failure was apparently influenced by a knot on the compression face or on the side near the compression face. It would seem, however, from the results shown in Table I that the presence of knots does not greatly decrease the crushing strength of Douglas fir. In 45 of the

68 cases the final failure was in tension, usually due to a knot or cross grain. In 8 of the 68 cases the final failure was in tension and apparently not influenced by defects. The remaining 15 out of the 68 failed entirely in compression; these pieces were entirely clear in the lower half. The pieces that did not fail in compression first had, with two exceptions, either cross grains or large knots on the tension face.

This summary of the failures apparently indicates that the tensile strength of Douglas fir is much greater than its compressive strength parallel to the grain. On this account, unless there are unusually serious defects on the tension side of the beam, the full compressive strength of the wood will be developed before failure occurs. The fact that in so many cases, 51 out of 68, the first failure in compression was apparently influenced by a defect, indicates that knots in the compression half of the beam lessen to a certain extent the ultimate strength of the timber. The elastic limit, however, is not greatly affected by such knots. It is shown in Table I that the elastic limit in compression is decreased approximately 5 per cent. by the presence of knots 1½ ins. or less in diameter, while it is decreased approximately 13 per cent. by knots greater than 1½ ins.

In the green bridge stringers tested, compression failure was also the most common. One hundred and seventy-five such stringers were tested, and were divided according to their modulus of rupture into three classes. Seventy-one of the stringers developed a modulus of rupture of over 6,500 lbs. per sq. in.; 70 of these failed first in compression. Seventy developed a modulus of rupture between 5,000 and 6,500 lbs. per sq. in.; 64 of this group failed first in compression. Thirty-four stringers developed a modulus of rupture of less than 5,000 lbs. per sq. in.; 14 of this group failed first in compression.

In the tests that did not fail first in compression, as in the tests on green car sills, the first failure was in tension or cross-grain tension, due respectively to large knots on the tension face and to a spiral or diagonal grain, or in horizontal shear. Out of the 175 green stringers tested, 3 failed first in horizontal shear and 31 had secondary failures in horizontal shear, but in such cases the shearing failure did not occur until the primary failure had developed to a considerable extent.

GRADING RULES AND SPECIFICATIONS.

Grading rules and specifications for structural timbers are primarily for the purpose of classifying them on the basis of strength and durability, and only recently have engineers given any serious consideration to the subject. A few years ago it was possible to secure the highest grade material at a very moderate cost and we find early specifications almost invariably calling for material free from such defects as might even be suspected of impairing its strength and durability.

With the increased consumption of lumber and the rapid depletion of the supply of high-grade material available for structural purposes, the user of structural timber has been forced to modify specifications and to accept material which a few years ago would have been rejected. It is reasonable to suppose that further modification will have to be made with changing conditions of supply and demand.

Almost without exception structural timber is graded in a green condition. On this account the following comparison of different grading rules and specifications for Douglas fir is based on data secured from tests on green materials; they are typical of those now in use for structural sizes. The comparison is of interest in that it indicates along what lines specifications and grading rules can be modified to the best advantage.

Specification A: Used by one of the leading transcontinental railway systems:

> "All timber must be of the best description of the kind required. It must be sawed square and to proper dimensions. It must be free from all loose, large, or unsound knots, sap, sun cracks, shakes, wanes, or other imperfections or defects which would impair its strength or durability."

Specification B: Used by one of the leading railway systems operating east of the Mississippi River:

> "Fir stringers, bridge ties and plank must be from good, sound, live, straight and close-grained Douglas fir, sawed square edge and true to dimensions ordered, free from splits, shakes, loose, rotten, or grouped knots

and other defects. Sound knots 2 ins. in diameter or less will be permitted if not less than 4 ins. from the center of the knot to any edge. Ninety per cent. of the stringers and ties must be free from sap; 10 per cent. may have sap not in excess of 2 ins. on each of the two edges only."

Specification C: Isthmian Canal Commission:

"Stringers of Douglas fir shall show not less than 85 per cent. heart on any face and not less than 70 per cent. on any edge; it shall show not less than an average of 12 annual rings to the inch. Sound knots less than 3 ins. in diameter shall be permitted in the vertical faces of the stringer at points not less than one-fourth the depth from the edge of the piece."

Specification D: Export Grading Rules of the Pacific Coast Lumber Manufacturers' Association; adopted 1903.*

"*Selects:* Shall be sound, strong lumber, good grain, well sawed. Will allow in sizes 6 by 6 ins. and less, knots not to exceed 1 in. in diameter; sap on corner one-fourth the width and one-fourth the thickness; small pitch seams when not exceeding 6 ins. in length. In sizes over 6 by 6 ins., knots not to exceed 2 ins. in diameter, varying according to the size of the piece; sap on corner not to exceed 2 ins. on both face and edge; pitch seams not to exceed 6 ins. in length. Defects in all cases to be considered in connection with the size of the piece and its general quality.

"*Merchantable:* This grade shall consist of sound, strong lumber, free from shakes, large, loose or rotten knots, and defects that materially impair its strength; well manufactured and suitable for good substantial constructional purposes. Will allow occasional variations in sawing or occasional scant thicknesses, sound knots, pitch seams, and sap in corners, one-third the width and one-half the thickness. Defects in all cases to be considered in connection with the size of the piece and its general quality.

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* These rules have been modified since their adoption, but the changes have been made to express the conditions with greater clearness rather than to indicate a different quality of timber. The rules quoted were in force when the material used in these tests was selected and graded.

"*Seconds:* This grade shall consist of lumber having defects which exclude it from grading as Merchantable. Will allow knots and defects which render it unfit for good substantial constructional purposes, but suitable for an inferior class of work."

Specification E: This specification was first compiled by Committee D-7 (formerly Committee Q) of the American Society for Testing Materials, and with the exception of the requirement for Douglas fir stringers in Section 4 below, was adopted by that Society. (The requirement for Douglas fir timber is contained in the Proposed Standard Specifications for Douglas Fir and Western Hemlock Bridge and Trestle Timbers submitted to the Society in June, 1910,* which have not as yet been adopted by the Society). With minor modifications this specification was adopted by the American Railway Engineering Association as its standard specification for bridge and trestle timbers. It has also been adopted by the Yellow Pine Manufacturers' Association as a basis for grading yellow-pine bridge and trestle timbers. The specification is printed in the form adopted by the American Railway Engineering Association.

STANDARD SPECIFICATIONS FOR BRIDGE AND TRESTLE TIMBERS.

(To be applied to solid members and not to composite members.)

1. "*General Requirements.*—Except as noted, all timber shall be cut from sound trees, true and straight, and sawed standard size; shall be square-edged, close-grained, solid and out of wind; free from defects such as injurious ring shakes and crooked grain, unsound or loose knots, knots in groups, decay, large pitch pockets, or other defects that will materially impair its strength.
2. "*Standard Size of Sawed Timber.*—Rough timbers sawed to standard size means that they will not be over $\frac{1}{4}$ in. scant from the actual size specified. For instance, a 12 by 12-in. timber shall measure not less than $11\frac{3}{4}$ by $11\frac{3}{4}$ ins.

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* *Proceedings*, Vol. X, 1910, p. 155.

3. "*Standard Dressing of Sawed Timber.*—Standard dressing means that not more than $\frac{1}{4}$ in. shall be allowed for dressing each surface. For instance, a 12 by 12-in. timber after being dressed on four sides shall measure not less than $11\frac{1}{2}$ by $11\frac{1}{2}$ ins."

No. 1 R. R. Grade. Longleaf Yellow Pine and Douglas Fir.

4. "*Stringers.*—Longleaf pine shall show not less than 85 per cent. heart on the girth anywhere in the length of the piece; provided, however, that if the maximum amount of sap is shown on either narrow face of the stringer, the average depth of sap shall not exceed $\frac{1}{2}$ in. Douglas fir shall show not less than 90 per cent. heart as measured above. Knots greater than $1\frac{1}{2}$ ins. in diameter will not be permitted at any section within 4 ins. of the edge of the piece."

No. 2 R. R. Grade. Longleaf and Shortleaf Yellow Pine, Douglas Fir, and Western Hemlock.

10. "*Stringers.*—Shall be square-edged, except that they may have 1 in. wane on one corner. Knots shall not exceed in their largest diameter one-fourth the width of the face of the stick in which they occur, and shall in no case exceed 4 ins. Ring shakes shall not extend over one-eighth of the length of the piece."

DISCUSSION OF SPECIFICATIONS.

In addition to the general statements such as "strong, good timber," etc., we find in the various specifications quoted statements dealing specifically with:

(1) Quantity of sapwood permitted; (2) sawing; (3) grain; (4) knots, size, position and number; (5) checks, shakes, and pitch pockets.

The relation of most of these elements to the mechanical properties of the timbers tested have been discussed in another part of this bulletin, but a brief recapitulation of the facts brought out will assist in the critical examination of the quoted specifications.

1. *Sapwood.*—The sapwood specifications are inserted to

secure material having the desired durability. They are readily complied with in Douglas fir since the trees have a comparatively small amount of sapwood and practically all structural timbers are cut from that portion of the tree nearest the heart.

2. *Sawing.*—Attention has been called to the effect of diagonal and cross grains upon the strength of structural timbers. Diagonal grain, due to the sawing, is a serious defect and there is ample justification for guarding against it in specifications and grading rules. The results of the tests indicate that a slant in the grain as great as 1 in. in 45 ins. can be permitted without seriously affecting the strength of the timber, but if the diagonal grain is much more pronounced than this, the timber if heavily loaded may fail suddenly in cross-grain tension some time before the compressive strength of the wood is developed.

3. *Grain.*—The specifications regarding grain are of two general characters: those dealing with the straightness of the grain, and those dealing with the rate of growth, indicated by the number of rings per inch. Any waving or crossing of the grain, such as is frequently caused by knots, may prove a serious defect if the disturbance breaks the continuity of the grain near the edges of the timber. When, however, the continuity of the grain within an inch or two of the edges is not disturbed, wavy or cross grain has little significance as a defect in structural timber.

The average rate of growth is also of little importance in grading structural timbers, no apparent relation existing between it and the mechanical properties of the timber.

4. *Knots.*—The effect of knots on the strength of Douglas fir in structural sizes was discussed. It was shown that the significant factors with respect to knots are their size, position, and the condition of the wood around them. In a general way, the results of the tests also show that a large number of knots is almost invariably indicative of a poor quality of wood.

5. *Checks, Shakes, etc.*—Checks and shakes are only occasionally encountered in green Douglas fir; checks, however, almost invariably develop as the timber seasons. Except in sticks of the lowest grade the development of checks does not reduce the strength of the timber below that in a green condition, since the increase in the strength of the wood due to seasoning more than offsets the weakening effect of the checks. Under certain conditions, the

presence of checks tends to decrease the durability of the wood by making it more vulnerable to the attack of insects and wood-destroying organisms. Especially is this true of treated timber where the checks extend through that portion of the wood which is impregnated with the preservative.

To determine the effectiveness of the different grading rules and specifications quoted, an attempt was made to apply them to the 175 green bridge stringers tested. The results of this analysis will be discussed under the designations given to the different specifications.

Specification A.—The wording of this specification is so general that it practically leaves the whole question of grading to the judgment of the inspector. With a high-grade inspector such specifications frequently give very satisfactory results, both to the purchaser and to the selling party. Results secured from its application, however, will vary with the judgment of the inspector, and on this account the use of such specifications is frequently the cause of more or less serious misunderstanding between the parties concerned. It was thought impracticable to use this specification in grading the material tested. It is cited merely as being typical of a class of specifications in use.

Specification B.—This specification does not allow knots more than 2 ins. in diameter and no knots at all are permitted unless the center of the knot is at least 4 ins. from the edge.

Only 19 of the 175 stringers tested passed this knot specification, indicating that it is by all means too severe.

The average fiber stress at the elastic limit, modulus of rupture, and modulus of elasticity of the stringers passing this specification were, respectively, 4,887, 7,414, and 1,728,000 lbs. per sq. in. The maximum values of these functions were respectively, 5,850, 8,890, 1,945,000, while the minimum values were, respectively, 4,100, 6,440, and 1,523,000.

Specification C.—The pertinent provisions of this specification are: first, that the average rate of growth shall not be less than 12 rings per inch; second, knots greater than 3 ins. in diameter are not allowed; and third, knots less than 3 ins. in diameter are permitted on the vertical faces at points not less than one-fourth of the depth from the edge of the piece.

Only 9 stringers of the 175 tested met this specification. As in the specification just preceding, the fact that no knots at all were permitted within a certain specified distance of the edges was the reason for rejecting most of the material. The rate-of-growth specification was also the cause of throwing out some material which passed the knot specification.

The average fiber stress at the elastic limit, modulus of rupture, and modulus of elasticity of the stringers passing this specification were, respectively, 4,835, 7,062, and 1,767,000 lbs. per sq. in. The maximum values were, respectively, 5,540, 7,930, and 1,970,000; the minimum, 4,300, 5,320, and 1,565,000.

Specification D.—These grading rules contain very few specific statements regarding the character of the different defects which will be permitted, but like Specification A they leave a great deal to the judgment of the inspector.

All of the material tested was graded according to these rules by one of the inspectors of the lumber-manufacturers' associations cooperating in the work.

Tables III and IV give, respectively, the average properties as well as their range for bridge stringers and car sills graded in accordance with these rules.

Specification E.—Table V gives the average properties and their range for the different grades defined in this specification. The specification defines only two grades, but in this analysis of the test data the stringers which failed to make either of these grades have been classified as culls.

Specifications D and E are evidently the most practical of those quoted. To further compare their efficiency in grouping material according to its mechanical properties, the results of the tests are shown graphically in Fig. 3. For each grade indicated in the diagram, the modulus of rupture and modulus of elasticity are shown to the right of the heavy vertical line and the fiber stress at elastic limit to the left of this line. The vertical scale is in pounds per square inch, except that part of it which indicates the modulus of elasticity which should be read in thousands of pounds per square inch. The length of the shaded portions between the horizontal lines is proportional to the total number of tests falling within the limits indicated on the vertical scale. The points within the shaded areas show the grouping of the individual tests.

TABLE III.—DOUGLAS FIR, GREEN MATERIAL WITH DEFECTS; GRADED ACCORDING TO PACIFIC COAST EXPORT GRADING RULES.

Average values. Bending. Size, 8 x 16 ins. x 15 ft. 6 ins.

Grade.		Number of Tests.	Rings per Inch.	Percentage Summer Wood.	Percentage Moisture.	Lbs. per Cu. Ft.		Fiber Stress at Elastic Limit, lbs. per sq. in.	Modulus of Rupture, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.
						As Tested.	Oven Dry.			
Select	High, 10 per cent	6	21.4	41 ^{5}	35.4	42.6	32.7	5,420	8,150	1,962,000
	Average	59	11.9	35 ^{52}	31.2	38.9	29.6	4,346	6,753	1,654,000
	Low, 10 per cent	6	6.2	27 ^{5}	26.3	34.5	25.9	3,221	4,910	1,340,000
Merchantable	High, 10 per cent	8	20.0	43 ^{5}	35.9	42.4	32.2	5,180	7,670	1,880,000
	Average	79	10.7	34 ^{51}	31.5	36.8	28.0	3,895	5,878	1,481,000
	Low, 10 per cent	8	5.5	27 ^{5}	25.7	32.1	24.5	2,780	3,915	1,133,000
Seconds	High, 10 per cent	4	17.4	38 ^{3}	35.1	40.3	31.1	5,340	7,413	1,714,000
	Average	37	10.0	32 ^{32}	30.1	36.9	28.4	3,538	5,188	1,328,000
	Low, 10 per cent	4	4.4	25 ^{3}	24.7	32.2	24.7	2,283	3,175	921,000

TABLE IV.—DOUGLAS FIR, GREEN MATERIAL WITH DEFECTS; GRADED ACCORDING TO PACIFIC COAST EXPORT GRADING RULES.

Average values. Bending. Size, 5 x 8 x 15 ins.

Grade.		Number of Tests.	Rings per Inch.	Percentage Summer Wood.	Percentage Moisture.	Lbs. per Cu. Ft.		Fiber Stress at Elastic Limit, lbs. per sq. in.	Modulus of Rupture, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.
						As Tested.	Oven Dry.			
Select	High, 10 per cent	3	23.7	56	37.2	45.0	34.6	5,280	7,497	2,187,000
	Average	25	14.8	42	30.6	38.7	29.7	4,320	6,147	1,698,000
	Low, 10 per cent	3	6.6	32	27.0	34.0	25.9	3,187	4,827	1,262,000
Merchantable	High, 10 per cent	3	17.8	57	32.6	40.4	31.5	4,597	6,353	1,848,000
	Average	30	9.4	38	29.2	35.6	27.6	3,532	4,946	1,513,000
	Low, 10 per cent	3	4.3	27	26.2	31.5	24.1	2,580	3,710	1,221,000
Seconds	High, 10 per cent	3	17.3	50	37.9	40.0	30.6	4,790	6,457	1,848,000
	Average	30	8.7	38	30.4	36.1	27.7	3,344	4,618	1,419,000
	Low, 10 per cent	3	4.0	27	26.5	31.2	23.9	2,177	3,100	1,125,000

A study of the diagram brings out the following points:

1. That according to Specification D the 175 stringers tested were made up of 59 selects, 75 merchantables, and 37 seconds; according to Specification E, they were made up of 54 R. R. No. 1, 66 R. R. No. 2, and 54 culls.

2. Specification E succeeds in eliminating from the highest grade, material having a modulus of rupture less than 6,000 lbs. per sq. in. with the exception of one stick which developed a modulus of rupture of approximately 4,200 lbs. per sq. in. This exception makes the extreme range in strength of the grades "R. R. No. 1" and "selects" the same.

3. The grade R. R. No. 2, with four exceptions, rules out material having a modulus of rupture of less than 5,000 lbs. per sq. in., the extreme range in the modulus of rupture being from 4,200 to 7,800 lbs. per sq. in. The merchantable grade admits 14 sticks having a modulus of rupture less than 5,000 lbs. per sq. in. and shows an extreme range in the modulus of rupture of from 3,500 to 8,800 lbs. per sq. in.

TABLE V.—DOUGLAS FIR, GREEN MATERIAL WITH DEFECTS; GRADED ACCORDING TO THE STANDARD SPECIFICATIONS OF THE AMERICAN RAILWAY ENGINEERING ASSOCIATION.

Average values. Bending. Size, 8 x 16 ins. x 15 ft. 6 ins.

Grade.		Number of Tests.	Rings per Inch.	Percentage Summer Wood.	Percentage Moisture.	Lbs. per Cu. Ft.		Fiber Stress at Elastic Limit, lbs. per sq. in.	Modulus of Rupture, lbs. per sq. in.	Modulus of Elasticity, lbs. per sq. in.
						As Tested.	Oven Dry.			
R. R. No. 1........	High, 10 per cent ..	5	22.7	44	35.9	43.6	33.6	5,800	8,468	1,964,000
	Average...........	54	12.0	36	31.4	39.2	29.9	4,516	7,108	1,678,000
	Low, 10 per cent ...	5	6.2	29	24.9	35.4	26.6	3,374	5,750	1,303,000
R. R. No. 2........	High, 10 per cent ..	8	18.0	41	35.8	41.5	31.8	5,006	7,430	1,875,000
	Average...........	67	10.3	34	31.3	37.2	28.3	4,057	6,116	1,528,000
	Low, 10 per cent ...	8	5.6	27	26.7	33.2	25.2	3,109	4,761	1,239,000
Culls..............	High, 10 per cent ..	5	19.3	39	34.9	40.0	31.0	4,656	6,700	1,715,000
	Average...........	54	10.7	32	30.5	36.1	27.7	3,330	4,845	1,310,000
	Low, 10 per cent ...	5	4.4	26	24.7	30.9	23.6	2,292	3,232	915,000

4. The extreme range in the seconds is from 3,000 to 8,300 lbs. per sq. in., while the culls of Specification E range in strength from 3,000 to 7,600 lbs. per sq. in.

5. It will be noticed that 40 of the culls, or 74 per cent. of the timbers excluded from the grades R. R. No. 1 and R. R. No. 2, were stronger than the weakest timbers of the latter grade. Thirty-one seconds, or 84 per cent. of the seconds, were stronger than the weakest timbers in the merchantable grade.

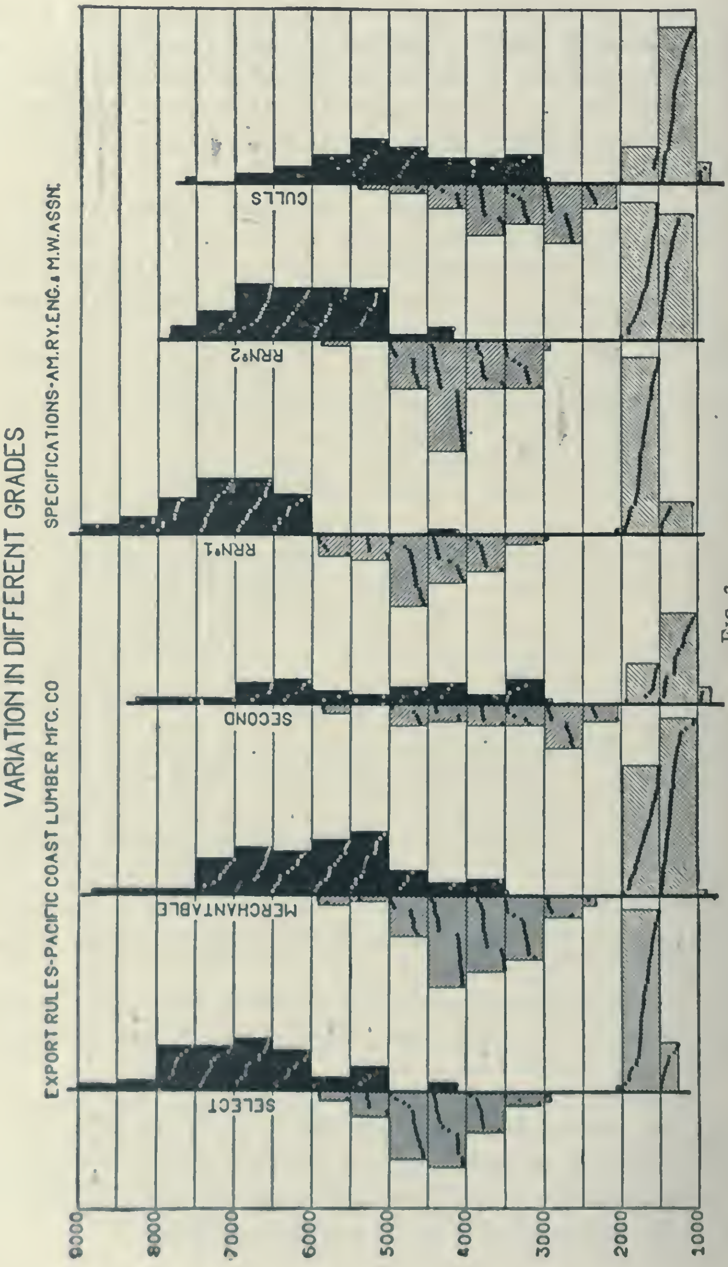
VARIATION IN DIFFERENT GRADES
SPECIFICATIONS-AM.RY.ENG.& M.W.ASSN.
EXPORT RULES-PACIFIC COAST LUMBER MFG. CO
CULLS
RRN°2
RRN°1
SECOND
MERCHANTABLE
SELECT
9000
8000
7000
6000
5000
4000
3000
2000
1000

FIG. 3.

6. The effectiveness of the specifications in grading material according to its fiber stress at elastic limit and its modulus of elasticity is also shown in the diagram. Specification E apparently is effective in culling material having a fiber stress at elastic limit of less than 3,000 lbs. per sq. in. Approximately 60 per cent. of the material culled, however, had a fiber stress at elastic limit greater than this.

The value of a specification or grading rule depends largely upon its reliability in excluding material which is deemed undesirable and in admitting material adapted to the needs of the purchaser.

The range in the properties of the different grades considered in Fig. 3, indicates that Specification E is somewhat more reliable in grouping material according to the merchantable properties than Specification D. On the other hand, more material having the required properties is culled by Specification E than by Specification D; 40 sticks as good as those admitted to R. R. No. 2 are thrown into the lowest grade in order to exclude the 14 sticks falling below the minimum strength shown in the R. R. No. 2. In Specification D, 30 timbers as good as those included in the merchantable grade are thrown into the lowest grade in order to exclude the 5 pieces falling below the minimum strength shown in the merchantable grade.

So many factors, which can not be detected by a visual inspection, influence the properties of structural timbers that the question of formulating grading rules which can be relied upon to exclude material falling below a specified strength is one extremely doubtful of solution. The one timber having a modulus of rupture of approximately 4,000 lbs. per sq. in. which passed the specification for R. R. No. 1 makes it unsafe to rely upon greater strength in timbers of this grade than in timbers of the next lower grade. The practicability, therefore, of having more than one grade for Douglas fir in structural sizes seems questionable.

Various attempts were made to introduce other factors, such as summer wood and rate of growth, with the idea of modifying Specification E so that fewer sticks having a modulus of rupture greater than 4,000 lbs. per sq. in. would be excluded, but none of the attempts were attended with much success. This specification seems to be as practical and effective as any that have been devised for grading Douglas fir. The results of the tests show, however, that much of the material falling in the two lowest grades (seconds and culls) has considerable structural merit and could be safely and economically used in false work and structures of a temporary character.

SOME FURTHER EXPERIMENTS UPON THE ABSORPTION, POROSITY AND SPECIFIC GRAVITY OF BUILDING BRICK.

By D. E. Douty and L. L. Beebe.

In a paper presented before the Society three years ago,* the desirability of obtaining more extended information in regard to the absorptive capacity of the common kinds of building brick was expressed.

Since that time Mr. James E. Howard, formerly of the Watertown Arsenal Testing Laboratory, and at present Engineer-Physicist of the Bureau of Standards, has presented two papers before the National Brick Manufacturers' Association upon the subject and has emphasized the indefiniteness of the term "absorption."†

If "absorption," "porosity," or "percentage of voids" is to become an element in the Standard Specifications for Building Brick, an accurate definition and uniform standard method of determination is certainly necessary. Leaving out of the question the discussion of the advisability of the inclusion of porosity as an element of the specification, a subject to be more properly considered in connection with the Standard Specifications themselves, we have attempted to obtain some further information along the same lines as outlined in the previous paper.

Of the twenty-three varieties of brick which were the subject of that paper, nine were selected covering the range of absorptive capacity in as nearly equal intervals as possible in the light of the preliminary information then obtained.

As it also seemed desirable to obtain some information, for the purposes of comparison, on the specific gravity of the ground material as well as the normal brick, four of the samples were selected covering the range, and the specific gravity of the ground material, passing different sizes of sieves, was obtained. Samples of the brick were first crushed to pass a No. 20 sieve, thoroughly mixed and divided into portions to be subsequently crushed to pass Nos. 40, 60, 80, 100, and 200 sieves. These determinations were made with the Le Chatelier flask after all the moisture had been driven off; the results are given in Table I.

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* "The Influence of the Absorptive Capacity of Brick upon the Adhesion of Mortar," D. E. Douty and H. C. Gibson, *Proceedings*, Vol. VIII, 1908, pp. 518-530.

† National Brick Manufacturers' Association, Buffalo meeting, February 5, 1909. abstracted in *Engineering News*, March, 1909, Vol. 6, No. 10, p. 273.

TABLE I.—COMPARISON OF THE SPECIFIC GRAVITIES OF GROUND MATERIAL TO PASS DIFFERENT SIZES OF SIEVES.

LE CHATELIER FLASK.

Brick No.	Sieve Number.					
	20	40	60	80	100	200
2........	2.568	2.590	2.600	2.604	2.613	2.636
18........	2.457	2.490	2.506	2.522	2.533	2.547
10........	2.462	2.510	2.538	2.545	2.552	2.580
13........	2.582	2.626	2.644	2.648	2.659	2.689
PYKNOMETER.						
18........	2.433	2.487	2.517	2.527	2.534	2.550

Check determinations on Brick 18 were made with the pyknometer. As may be observed from the table, the increase in value of the specific gravity up to No. 60 sieve is quite marked. In subsequent comparison, the specific gravity of ground material passing No. 60 sieve is used as a basis.

Table II is a comparison of the specific gravities of the nine bricks with the absorption by the boiling method.

TABLE II.—COMPARISON OF THE SPECIFIC GRAVITIES OF NINE BRICKS WITH ABSORPTION BY THE BOILING METHOD.

Brick No.	Specific Gravity of Ground Material.	Specific Gravity of Normal Brick, Maximum Absorption.	Specific Gravity of Normal Brick Dried.	Voids,* per cent.	Absorption by Weight, per cent.	Absorption by Volume, per cent.
3........	2.66	2.64	1.89	28.9	14.02	26.5
13........	2.64	2.64	2.02	23.5	10.62	21.45
2........	2.60	2.61	2.03	21.9	11.20	22.7
20........	2.65	2.62	2.04	22.9	11.20	22.8
21........	2.60	2.58	2.00	23.1	10.62	21.2
8........	2.64	2.59	2.10	20.5	8.78	18.4
18........	2.51	2.48	2.04	17.7	8.45	17.2
16........	2.64	2.58	2.22	10.9	5.60	12.4
15........	2.64	2.53	2.32	12.1	2.95	6.8

* Percentage of voids $= 100 - \left(\frac{\text{Specific gravity of normal brick dried.}}{\text{Specific gravity of ground material.}} \times 100.\right)$

The specific gravity of normal brick, maximum absorption, was obtained by boiling quarter portions of the samples for 4 hours in hydrant water and determining the specific gravity by the method of suspension in distilled water, correction being made for the higher specific gravity of the water absorbed. As may be noticed, the values in this column very nearly approach the values for the specific gravity of ground materials and would probably equal them if maximum absorption had been obtained.

The specific gravity of normal brick dried was obtained by drying quarter portions of the brick to constant weight, coating with shellac varnish and baking in an oven at approximately 215° F. until hard, repeating the process of coating and baking until the absorption was so slight as to not materially affect the values obtained by the suspension method. The specific gravity of the shellac was found to be 1.067 and a correction made to allow for the coating.

The ratio of the specific gravity of the normal brick dried to the specific gravity of the ground material, multiplied by 100, expresses the percentage of solid matter present in the normal brick; this subtracted from 100 per cent. gives the percentage of voids.

Absorption by weight was obtained from the increase in weight of the specimens used in the determination of the specific gravity of normal brick, maximum absorption.

Absorption by volume was obtained from the product of the absorption by weight and the specific gravity of normal brick. It may be observed that these values approach very nearly the values for percentage of voids. That Bricks 2 and 16 show higher absorption by volume than percentage of voids may be ascribed to the fact that specimens from bricks with a normal specific gravity higher than the average were probably selected.

Table III is a comparison of the results obtained by five methods of determining absorption with both whole and half specimens which were previously dried to a constant weight at 100° C.

In "Partial Immersion" the specimens were immersed to a depth of ½ in.

In "Total Immersion" the specimens were submerged to a depth of ½ in.

In "Total Immersion and Boiling," the specimens were submerged for 7 days and then boiled for 1 hour. All partial immersion tests were conducted under as uniform conditions of air humidity and temperature as possible, i. e., 100 per cent. humidity and about 68° F.

In "Boiling," the specimens were boiled 4 hours and weighed as soon as cool enough to handle and also 24 hours afterwards. In some cases there was a slight decrease after 24 hours and in others an increase.

In the "Vacuum Test" the specimens were subjected to a reduced pressure of about 68 cm. of mercury for 1 hour and without breaking the vacuum, water was allowed to flow in until the specimens were completely covered and then subjected to a pressure of about 35 lbs. for 1 hour.

As Table III is primarily a comparison of methods, the averages of absorption may be assumed to indicate the relative values of these different methods.

Rate of Absorption.—Table IV gives a comparison of the rate of absorption for partial and total immersion of whole and half bricks by weight over an extended period of time.

At the expiration of 110 days the bricks were boiled four hours and the percentage of absorption obtained in this way was taken as the maximum or 100 per cent. (See Table III.) The percentage rate at various periods was computed from this maximum.

For both whole and half bricks, it may be observed that approximate maximum absorption is attained at an earlier period and the rate of absorption is higher in the case of total immersion than partial immersion, except in a few cases which can be accounted for by a variation in the specimens themselves.

In both total and partial immersion, approximate maximum absorption is attained at an earlier stage and the rate of absorption is greater in the case of the half bricks than whole bricks.

Tests were made on several of the bricks to determine the effects of repeated absorption and drying. After repeating absorption and drying ten times no appreciable change in the percentage of absorption or loss in weight could be observed.

Permeability.—For the purpose of determining whether any relation exists between the size or percentage of voids and the absorptive capacity, half bricks were mounted on the Amsler-

TABLE III.—COMPARISON OF RESULTS FROM FIVE METHODS OF DETERMINING ABSORPTION WITH WHOLE AND HALF SPECIMENS, PREVIOUSLY DRIED TO CONSTANT WEIGHT AT 100° C.

Brick No.	Partial Immersion 90 days.				Total Immersion 90 days.				Immersion 7 days and Boiling 1 hour.			
	Whole.		Half.		Whole.		Half.		Whole.		Half.	
	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.
3	13.4	25.3	16.7	31.6	13.8	26.1	13.9	26.3	14.6	27.6	15.3	28.9
13	12.1	24.4	12.5	25.3	11.5	23.2	10.5	21.2	11.1	22.4	10.5	21.2
2	10.7	21.7	10.4	21.1	10.8	21.9	11.3	22.9	11.2	22.7	10.2	20.7
20	10.6	21.6	11.0	22.4	10.2	20.8	11.0	22.4	11.5	23.5	11.3	23.1
21	9.2	18.4	10.7	21.4	10.3	20.6	10.3	20.6	11.0	22.0	10.4	20.8
8	8.4	17.6	8.9	18.7	10.1	21.2	9.3	19.5	8.9	18.7	8.9	18.7
18	8.0	16.3	8.0	16.3	7.9	16.1	8.1	16.5	8.4	17.1	8.3	16.9
16	3.8	8.4	4.3	9.5	5.1	11.3	4.9	10.9	3.8	8.4	5.0	11.0
15	4.2	9.7	3.5	8.1	3.6	8.3	3.5	8.1	6.3	2.7	4.7	10.9
Average	.....	18.2	.....	19.4	.....	18.8	.....	18.7	.....	18.3	.....	19.1

Brick No.	Boiling.				Vacuum.				Total Immersion 110 days and Boiling 4 hours.			
	Whole.		Half.		Whole.		Half.		Whole.		Half.	
	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.	Wt.	Vol.
3	14.0	26.5	13.2	25.0	13.1	24.8	13.4	25.3	14.4	27.2	14.3	27.0
13	12.8	25.9	12.8	25.9	9.7	19.6	11.0	22.2	11.9	24.1	11.1	22.5
2	11.7	23.8	11.2	22.7	9.8	19.9	10.5	21.3	11.6	23.6	11.8	24.0
20	11.1	22.6	11.2	22.8	10.7	21.8	10.6	21.6	10.7	21.8	11.3	23.0
21	11.0	22.0	10.2	20.4	10.5	21.0	10.8	21.6	11.2	22.4	10.9	21.8
8	10.0	21.0	10.7	22.5	9.2	19.3	8.0	16.8	10.8	22.7	9.9	20.8
18	8.5	17.3	8.6	17.5	7.2	14.7	7.2	14.7	8.7	17.7	8.7	17.7
16	6.0	13.3	5.6	12.4	3.0	6.7	3.3	7.3	6.0	13.3	5.7	12.7
15	4.1	9.5	4.2	9.7	4.8	11.1	3.5	8.1	4.9	11.4	4.4	10.2
Average	.....	20.2	.....	19.9	.....	17.7	.....	17.8	.....	20.5	.....	20.0

Laffon permeability apparatus, shown in Fig. 1, and subjected to a pressure of about 2 kilograms per sq. cm. (285 lbs. per sq. in.). The molds for the apparatus are of hollow cast iron in the shape of a truncated cone, clamped to a bed plate through which connection is made for water pressure. The bricks were placed flat and central in the mold, with a flange water-tight bushing on the lower side to prevent the water from carrying free lime through the brick from the lime-cement mixture in which they were mounted, thus decreasing the flow. A plain bushing

TABLE IV.—COMPARISON OF RATE OF ABSORPTION OF WHOLE AND HALF BRICKS BY WEIGHT OVER AN EXTENDED PERIOD OF TIME.

(PER CENT. OF TOTAL ABSORPTION BY WEIGHT.)

PARTIAL IMMERSION, WHOLE BRICKS.

Brick No.	10 min.	30 min.	1½ hours.	5 hours.	24 hours.	7 days.	17 days.	34 days.	90 days.	110 days.
3.....	64.7	72.7	73.4	78.4	84.2	90.0	92.1	92.8	96.7	97.9
13.....	26.2	42.9	67.5	73.0	81.8	87.3	90.5	91.3	96.1	96.1
2.....	45.7	62.9	66.4	70.7	76.7	85.3	87.9	89.7	94.0	94.0
20.....	71.5	72.4	72.4	72.4	73.2	80.1	81.9	83.6	91.4	91.4
21.....	70.4	70.4	70.4	70.4	70.4	74.3	78.0	80.9	87.6	88.5
8.....	37.0	44.6	50.4	59.8	68.5	80.5	83.7	85.9	90.2	92.4
18.....	50.0	61.1	61.1	64.4	68.9	75.6	80.0	83.4	88.9	88.9
16.....	20.8	21.2	21.8	28.0	38.0	40.0	56.0	62.0	78.0	78.0
15.....	21.8	27.3	34.6	43.6	45.5	60.0	65.5	67.3	76.3	76.3
Average	45.3	52.8	57.6	62.3	67.5	74.8	79.5	81.9	88.8	89.3

TOTAL IMMERSION, WHOLE BRICKS.

3.....	70.9	72.9	75.0	78.5	84.1	88.9	91.0	93.1	95.8	95.8
13.....	44.5	63.0	67.2	72.4	79.0	86.6	89.9	92.5	96.6	96.6
2.....	60.4	60.4	62.9	65.4	72.4	81.0	84.5	88.8	93.2	93.2
20.....	71.0	71.0	74.8	76.6	79.4	84.2	84.2	89.7	95.3	97.2
21.....	66.9	67.9	67.9	71.4	74.1	81.3	83.9	85.7	92.0	92.0
8.....	57.4	60.2	62.9	67.6	75.9	85.2	87.0	87.9	93.5	93.5
18.....	59.8	62.1	62.1	64.4	70.2	74.8	78.2	82.8	90.8	91.9
16.....	26.2	33.9	38.5	43.1	50.8	61.5	66.2	72.3	78.5	81.6
15.....	28.6	36.8	42.9	42.9	49.0	53.1	59.2	69.4	73.5	73.5
Average	54.0	58.7	61.6	64.7	70.5	77.4	80.5	84.7	89.9	90.6

PARTIAL IMMERSION, HALF BRICKS.

3.....	64.1	76.7	82.1	85.1	89.2	91.1	94.1	95.3	100.0	100.0
13.....	30.5	46.9	69.5	74.2	81.3	87.5	89.1	91.4	97.7	97.7
2.....	50.9	61.1	62.9	67.5	68.5	83.3	87.9	89.8	96.3	96.3
20.....	73.9	73.9	73.9	73.9	75.6	79.8	80.6	83.2	92.5	93.3
21.....	68.4	69.2	69.2	70.9	74.4	78.6	81.2	83.8	91.5	94.1
8.....	42.3	50.5	57.7	61.8	71.2	80.4	82.5	85.6	91.8	91.8
18.....	51.8	60.0	60.0	63.5	68.3	75.3	80.0	83.5	94.1	94.1
16.....	19.6	20.0	27.3	36.4	43.6	50.9	60.0	63.6	78.2	78.2
15.....	28.9	33.3	44.4	46.7	51.1	60.0	62.2	64.4	77.8	77.8
Average	47.8	54.6	60.8	64.4	69.2	76.3	79.7	82.3	91.1	91.5

TOTAL IMMERSION, HALF BRICKS.

3.....	71.4	72.7	74.8	78.4	83.2	88.8	91.6	94.5	97.2	97.2
13.....	46.8	59.4	64.8	72.0	79.2	83.8	88.3	90.1	94.6	94.6
2.....	64.4	64.4	66.1	68.6	77.2	83.0	84.8	89.0	95.8	95.8
20.....	72.6	73.4	74.4	77.8	79.6	84.0	86.7	90.3	97.3	97.3
21.....	68.8	68.8	70.6	71.6	78.0	81.6	82.5	85.3	94.5	95.5
8.....	51.6	59.6	62.6	65.7	73.8	83.9	85.9	87.9	94.0	94.0
18.....	59.8	63.2	63.2	65.6	71.3	73.6	78.3	85.1	93.1	93.1
16.....	31.6	40.4	43.9	47.4	54.4	61.4	70.2	75.4	87.8	87.8
15.....	29.6	31.8	40.9	43.2	47.7	47.7	56.8	65.9	79.5	79.5
Average	55.2	59.3	62.4	65.6	71.6	76.4	80.6	84.8	92.6	92.8

TABLE V.—RESULTS OF PERMEABILITY TESTS.

Brick No.	Specific Gravity of Normal Brick Dried.	Specific Gravity of Ground Material.	Voids, per cent.	Permeability, cu. cm. per sq. cm. per min.	Absorption by Volume, per cent.
3........	1.89	2.66	28.9	1.600	26.5
13........	2.02	2.64	23.5	0.500	21.5
2........	2.03	2.60	21.9	1.957	22.7
20........	2.04	2.65	22.9	2.000	22.8
21........	2.00	2.60	23.1	3.230	21.2
8........	2.10	2.64	20.5	0.844	18.4
18........	2.04	2.51	17.7	0.570	17.2
16........	2.22	2.64	10.9	0.282	12.4
15........	2.32	2.64	12.1	0.594	6.8

was placed on the upper side of the brick in line with the lower one, and neat cement, with a small amount of hydrated lime added to insure absolute impermeability of the mounting, was used. The permeability specimens were placed for several hours in distilled water before mounting to prevent absorption from the cement mixture. Distilled water was used to obtain the results here given, which are only preliminary and confined to the average of three runs of 1½ hours' duration on single specimens.

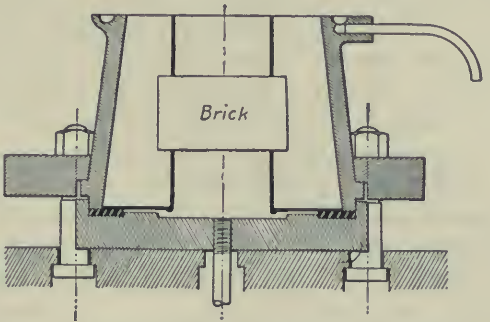

FIG. 1.—Amsler-Laffon Permeability Apparatus.

Although the data on the permeability test are incomplete in that they are confined to a very limited number of specimens, a comparison of the last three columns of Table V would seem to indicate that the largest values of permeability are obtained for those bricks in which the absorption by volume most nearly approaches the percentage of voids.

DISCUSSION.

The Chairman. MR. A. N. TALBOT (*in the Chair*).—This matter of the absorption of water by brick has been of interest for a long time to those who have been directly connected with the testing of brick, and the variations, uncertainties and difficulties in making these tests have been known to many. This paper, with the valuable data given in it, explains many of the apparent variations which have been found at various times in the absorptive value of different qualities of brick. It is of value to us in giving information along this line. It is now open for discussion.

Mr. Schuyler. MR. MONT SCHUYLER.—There are two variables which enter into the final density of a brick pressed from dry clay. Before burning, some bricks may be of a greater density than others, due to the inclusion in the dies of different amounts of clay. Then the burning will shrink the various bricks by amounts depending mainly upon the temperatures reached and partially upon their initial density. In no case, however, will identical firing reduce bricks of different initial densities to the same final density. In its failure to recognize this the present tentative specifications of this Society are deficient.

The value of any brick made from a suitable clay is dependent upon the burning rate and temperature reached. Due to high initial density, bricks that are obviously soft and unfit frequently show an absorption in 24 hours of less than 10 per cent. The converse is frequently true in the case of well-burned bricks.

A method for differentiating hard from soft brick perhaps may be found in the rate of absorption. As a rule soft bricks absorb about 90 per cent. of their 24-hour absorption within two minutes, while, in the case of hard bricks, the 2-minute absorption is approximately 97 per cent. of the 24-hour absorption.

These peculiarities are found in the brick manufactured in St. Louis, and I believe that investigation of brick manufactured in other places by the same process will prove this to be a general rule and perhaps a demonstration that absorption is a function of capillarity.

THE CHAIRMAN.—Will Mr. Douty tell us the reason the absorption is larger when the brick is wholly immersed than when it is partially immersed? **The Chairman.**

MR. D. E. DOUTY.—Mr. Chairman, I cannot explain that on any positive evidence. It may be due partly to evaporation from the surface of the brick, and partly to the fact that the capillary action in the case of partially immersed brick is not sufficient to draw the water into all the little spaces. In the previous paper and in a good deal of the data which have been published, the objection made to the total immersion method was that the air became entrapped in the small spaces in the voids, and therefore the water could not enter; and that the partial immersion method would enable the capillary action to fill those spaces gradually and the air in consequence be completely driven out. I am inclined to think that the method of boiling for four hours, which can be done in an ordinary pail over an ordinary fire, and the determination made with an ordinary Fairbanks balance of reasonable sensitiveness, is the best method of obtaining the total saturation of the brick. **Mr. Douty.**

THE CHAIRMAN.—May I ask if the rate of absorption in the boiling test throughout the 24 hours was determined, and whether it is necessary to have as long a period as 24 hours? **The Chairman.**

MR. DOUTY.—In previous experiments we had attempted to make a determination of the rate of absorption. It is our opinion that one hour is sufficient for bricks to obtain practical saturation; but I should not want to give that positively as the time without further experimenting and more accurate determinations. **Mr. Douty.**

MR. EDGAR MARBURG.—I should like to ask the authors what, in their experience with different samples of brick of supposedly the same quality, they find to be about the average number of bricks necessary for a fair test? **Mr. Marburg.**

MR. DOUTY.—I cannot answer that question. I should suggest, however, that I do not believe an absorption test based on less than ten bricks would be reliable. As has been pointed out, brick is a material that varies a great deal in its structure and formation; and I think to make a test accurate and reliable there should be at least ten specimens selected at random for the test. **Mr. Douty.**

A STUDY OF THE RATTLER TEST FOR PAVING BRICK.

By M. W. Blair and Edward Orton, Jr.

Introduction.

The rattler test for determining the relative resistance of paving bricks to impact and abrasion has been in use since the late eighties, when the paving of streets with brick began to make headway in America. The simplicity and competitive features of this mode of testing has from the first strongly appealed to all concerned. The early mode of testing was to put two or more bricks of as many different samples as were in competition, into a rattler of appropriate size, fill the bulk of the residual space with scrap iron of any sort available, and rotate the barrel at any convenient speed for from one to six hours, and determine the losses of each variety. Of course, under such conditions, there was no likelihood of the original operator being able to check his results, and no possibility of rattler tests made in different places being compared at all.

The first efforts at standardization of the test began in 1895 and resulted in 1901 in the rather wide acceptance of a method known as the N. B. M. A. test. For a much fuller historical statement of the beginnings of the rattler test, see the chapter by A. N. Talbot, Bulletin 9, Geological Survey of Illinois.

This standard has been the criterion upon which hundreds of millions of paving bricks have been judged. It has done great good, by reducing to comparative order a condition which had been chaotic. It was recognized by the committee of engineers and manufacturers who framed the N. B. M. A. specifications, that these were not as full and well defined as was desirable, but it was considered unwise and premature to attempt too rigid and precise definition for fear of stopping the use of the test entirely. Some of the points left open or poorly defined in these specifications have since been shown to be capable of exerting a very appreciable disturbing effect.

Criticisms of the rattler test have, therefore, been occurring at all times since its adoption, and while not able to bring about the overthrow of the test, they have been bringing about in the minds of all concerned a steadily increasing desire for more exact and better defined specifications. The National Paving Brick Manufacturers Association in 1909 decided to undertake an investigation, with a view to supplying this need. They employed Mr. Marion W. Blair to conduct the investigation for them. Some time after this work was initiated, but before actual testing began, Professor Edward Orton, Jr., came forward with a proposition to cooperate in the proposed investigation by making duplicate studies at his own expense, and by exchange of data with Mr. Blair, to more fully test out the value of all proposed changes in specifications. This offer was accepted by the Association and joint work was begun in April, 1910, and continued into June of 1911. This paper sets forth the results of this study, in part, together with recommendations for a proposed standard method of making the rattler test.

PLAN OF THE INVESTIGATION.

The plan decided upon was as follows:

First: To equip both operators with identical machines, made from the same patterns, in the same foundry.

Second: To equip both operators with a large supply of paving bricks from ten different manufacturing plants, these samples to be selected so as to secure a variety in type of product but as much uniformity within each sample as the nature of the product permits.

Third: To make a series of duplicate tests under conditions to be carefully prescribed in advance, with a view to determining what degree of concordance could be obtained with the standard charge of cubic cast-iron shot as prescribed by the old N. B. M. A. tests.

Fourth: To make similar duplicate series using spherical cast-iron shot, and also with no shot, and following in other respects the same procedure before described, with a view to reducing the cost of making the test without decreasing its efficiency.

Beside these comparative studies of the rattler test, there were proposed and carried out certain other studies, such as the difference in quality of bricks representing the different parts of the kiln, the possible use of the absorption test as a mode of grading material for the rattler test, etc., which are not germane to the subject here under discussion, and to which no further attention will be drawn in this paper. These data will be presented in another paper elsewhere.*

In carrying out the above outline, a carefully prepared set of rules was agreed upon. These rules covered storage, mode of selection of samples, system of designating tests on each sample, number of tests of each kind to be made, mode of conducting the absorption test, selection of charges of bricks from those graded by absorption, condition of rattler (especially the staves), dimensions and quality of shot, mode of rejection of worn-out shot, mode of starting with charges of new shot, quality of iron in the shot, speed and duration of test, stopping and starting, weighing charges, computation of results, and many other minutiæ. The same blank forms for recording all data were also used by both operators.

If all of the above rules had been rigorously observed throughout by both operators, an earlier concurrence would undoubtedly have been obtained. From a lack of appreciation of the importance of exact observance, some differences of procedure did creep in from the beginning, and the most valuable knowledge obtained in the whole study has come from "running down" the discrepancies in the results thus produced, for it has demonstrated the importance of factors not heretofore suspected, and shown how rigid the conditions of the test must be to secure a proper concordance between different operators.

SERIES F.

The first ten letters of the alphabet were agreed upon to represent the ten different brands of bricks to be studied. The first sample to arrive was that designated by the letter F. The quantity was 2,100 bricks, which was equally divided between the two laboratories.

In equipping the two laboratories with cast-iron shot, it was provided that one common lot of each kind of shot, both cubic and spherical, was to be procured and divided. This was done, but with some delay. Prior to the arrival of the shot at Blair's laboratory, he had procured a supply of both cubic and spherical shot from the Over Foundry in Indianapolis, and started his tests.

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* Orton—"Some Observations on the Qualities of Paving Brick." *Transactions, American Ceramic Society*, Vol. XIII, p. 702.

Orton, in procuring the shot for distribution, had purchased from two different Columbus foundries. In all cases, both in Indianapolis and Columbus, the same specifications as to hardness of shot, quality of iron, etc., were used and the shot received was supposed by both operators to meet the requirements with sufficient exactness.

Thus, when the testing of Series F began, there were three different lots of shot in use, viz.:—spherical shot made by the H. Loudenslager Foundry Company, of Columbus, used by Orton; cubic shot made by O'Brien Brothers, of Columbus, used by Orton; cubic and spherical shot made by the Over Foundry, of Indianapolis, used by Blair. None of these inequalities were recognized as important, at the beginning.

A slight further inequality in conditions was brought about by Blair using his machines for some preliminary testing on materials for Series H and K, which had come into his possession early, but which had not reached Orton's laboratory. Thus, when Series F was begun, Blair's two machines had made 30 or 40 tests apiece, while Orton's machine was new.

Omitting all needless details, Table I gives a condensed summary of the results of this series. The same data are shown graphically in Fig. 1.

The discussion of the results of Series F will be deferred until the data of Series G have been presented. Owing to failure to cooperate closely enough, the two laboratories were in a different stage of forwardness at the end of Series F, and Series G was tested and available for study at the same time that the Series F data were first exchanged. Up to this point, each laboratory had followed the prearranged schedule according to its own interpretation, and both Series F and G show similar differences in results due to this fact.

SERIES G.

The second material to be distributed jointly between the two laboratories was that predesignated as G. It consisted of 2,100 bricks representing upper, middle and lower sections of the kilns. The only differences between the conditions of the test in Series G and F were those due to the increased wear of the channel steel staves, which were warped and distorted by peening.

TABLE I.—SUMMARY OF RATTLER TESTS UPON BRICKS OF SERIES F.

Position of Bricks in Kiln.		Cubic Shot, 10 Bricks per Charge.		Spherical Shot, 10 Bricks per Charge.		No Shot, 15 Bricks per Charge.	
		Orton, 10 tests.	Blair, 10 tests.	Orton, 10 tests.	Blair, 10 tests.	Orton, 5 tests.	Blair, 5 tests.
Lower Bench..	Average...	16.68	19.89	19.93	17.41	22.68	21.2
	Maximum.	18.80	24.9	22.22	21.3	23.26	22.8
	Minimum..	14.67	17.3	17.74	15.3	22.19	19.0
Middle Bench	Average...	16.10	18.31	19.47	16.35	20.51	20.3
	Maximum.	17.50	20.2	20.81	17.9	21.93	21.0
	Minimum..	14.61	17.3	17.81	14.5	19.00	19.1
Upper Bench..	Average...	18.52	19.35	20.93	17.83	24.19	21.4
	Maximum.	20.40	20.6	23.75	20.5	25.23	22.8
	Minimum..	16.66	18.4	18.28	14.7	22.49	20.2

Conditions of the Tests.—1,800 revolutions at 30 per minute.

Barrel, 28 ins. diameter by 20 ins. long.

Staves, 6-in. steel channels.

Cubic shot charge: 225 lbs. 1½-in. cubes, and 75 lbs. 2½ by 2½ by 4½-in. blocks.

Spherical shot charge: 225 lbs. 1⅞-in. spheres, and 75 lbs. 3¾-in. spheres.

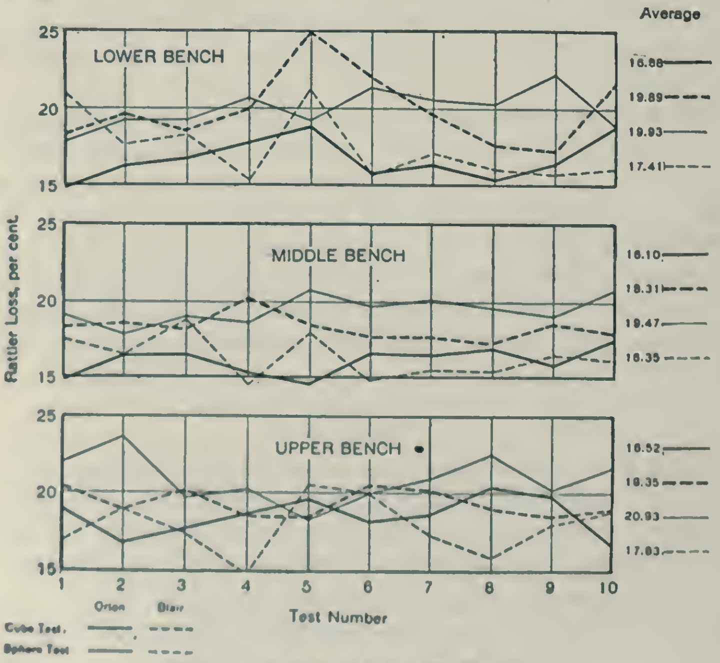

FIG. 1.—Curves of Series F (Partial).

The results of Series G are summarized in Table II and plotted in Fig. 2.

TABLE II.—SUMMARY OF RATTLER TESTS UPON BRICKS OF SERIES G.

Position of Bricks in Kiln.		Cubic Shot, 10 Bricks per Charge.		Spherical Shot, 10 Bricks per Charge.		No Shot, 15 Bricks per Charge.	
		Orton, 10 tests.	Blair, 10 tests.	Orton, 10 tests.	Blair, 10 tests.	Orton, 5 tests.	Blair, 5 tests.
Lower Bench..	Average...	16.94	17.68	20.07	15.42	22.74	21.60
	Maximum.	19.44	19.90	21.69	17.50	23.93	23.10
	Minimum..	15.63	17.00	19.06	13.70	21.88	21.00
Middle Bench.	Average...	17.80	20.32	22.07	18.64	23.69	21.90
	Maximum.	18.83	24.70	29.70	23.20	25.04	24.30
	Minimum..	16.60	17.20	19.71	16.30	22.05	20.80
Upper Bench..	Average...	17.90	18.80	21.94	17.33	24.33	24.70
	Maximum.	20.77	19.40	22.60	18.90	24.45	25.30
	Minimum..	16.72	18.20	21.20	15.60	24.19	24.20

Conditions of the Tests.—Same as in Table I, except those due to distortion of the staves by peening.

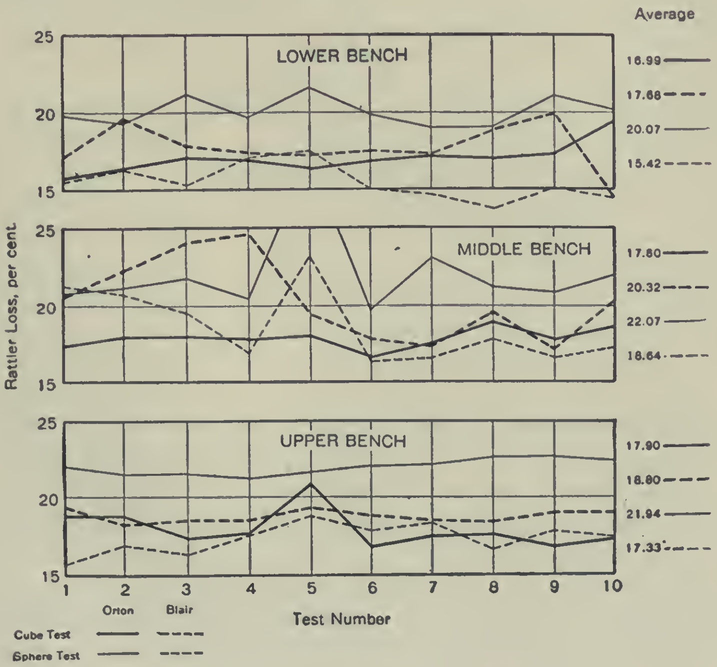

FIG. 2.—Curves of Series G (Partial).

Discussion of the Evidence of Series F and G.—The following points may be noted:

1. The operators do not check each other in any particular in either series, in comparing cube charges against cube charges, or sphere charges against sphere charges.

2. The concurrence in averages in those charges in which no shot is used is much better, though not as good as expected.

3. Orton's results show his cube tests averaging 3 per cent. lower than his sphere tests.

4. Blair's results show his cube tests averaging 2 per cent. higher than his sphere tests.

The radical failure of the two laboratories to check each other, and the curious direct reversal of the relation between cubic shot and spherical shot in the two laboratories, caused a halt in the work until some cause could be ascertained.

The operating conditions were carefully compared. The speed of Blair's machine was often a little less than Orton's, but not enough to cause apprehension at the time. The condition of the rattlers was then discussed. In both laboratories, the channel-iron staves were seriously warped by the peening of the inner surface under the millions of blows. This warpage was worse on Orton's machine, as it did the work alone, while at Blair's laboratory the work was divided between two machines. In Orton's machine, the diameter had been reduced an inch by this warpage and the surface was rendered rough by the edges of the staves warping inwards, so that a better chance was offered for the shot to "climb" in running, and thus fall farther when they went back. At the end of Series G, Orton's machine had made 180 tests and Blair's machines about 127 tests each. A divergence in conditions had thus developed.

Orton now discovered for the first time that Blair was not using the shot which had been sent from Columbus, but was using the "Over" shot which he had had made while waiting for the Columbus shot to arrive. The divergence in conditions thus involved the shot charges as well as the staves, and it was decided to make a fresh start.

Meanwhile, some additional data had been obtained by Orton before the above serious situation had been fully disclosed. This work was upon Series A and E, which had been received while tests of Series F and G were in progress. This is summarized in Table III:

TABLE III.

PARTIAL RESULTS OF SERIES A, BY ORTON.

Position in Kiln.		Cubic Shot.	Spherical Shot.
Lower Bench	Average (5 tests)	20.76	23.54
	Maximum	22.05	24.14
	Minimum	18.86	22.58
Middle Bench	Average (5 tests)	18.97	21.95
	Maximum	20.42	23.28
	Minimum	18.08	20.92
Upper Bench	Average (5 tests)	19.99	23.40
	Maximum	21.53	25.17
	Minimum	18.36	22.28

PARTIAL RESULTS OF SERIES E, BY ORTON.

Lower Bench	Average (5 tests)	20.15	23.56
	Maximum	21.97	24.63
	Minimum	19.19	22.53
Middle Bench	Average (5 tests)	18.39	21.06
	Maximum	19.64	22.16
	Minimum	17.03	19.18

These results merely still further confirm that, under the procedure and conditions prevailing in the Orton Laboratory, the sphere test gives results averaging 3 per cent. higher than the cube test.

SERIES C.

In order to obtain comparable conditions, one of Blair's machines and Orton's machine were stripped and simultaneously installed with a complete set of new 15.5-lb. medium-steel channel staves, the same as those originally furnished. Blair also rejected the Over shot and put in the Columbus shot charges, both cubic and spherical. A new sample, designated C, was divided between the two laboratories, giving 1,050 bricks from the upper, middle and lower benches, to each laboratory. The results of this comparison are shown in Table IV and are plotted in Fig. 3.

The concordance between the two laboratories is very much better than in any of the preceding work. Cube tests now check cube tests and spherical tests now check spherical tests, but the

TABLE IV.—SUMMARY OF RATTLER TESTS UPON BRICKS OF SERIES C.

Position of Bricks in Kiln.		Cubic Shot, 10 Bricks per Charge.		Spherical Shot, 10 Bricks per Charge.		No Shot, 15 Bricks per Charge.	
		Orton, 10 tests.	Blair, 10 tests.	Orton, 10 tests.	Blair, 10 tests.	Orton, 5 tests.	Blair, 5 tests.
Lower Bench..	Average...	21.71	22.44	22.76	23.40	27.27	24.62
	Maximum.	22.96	25.07	23.81	24.74	27.76	26.64
	Minimum..	20.40	21.76	21.14	22.02	26.17	20.89
Middle Bench.	Average...	23.67	23.83	22.79	24.61	27.45	26.35
	Maximum.	26.11	24.42	24.76	26.22	28.91	27.93
	Minimum..	21.01	22.22	20.71	23.36	26.29	24.23
Upper Bench..	Average...	21.93*	22.18†	22.25*	24.22†	25.74	24.53
	Maximum.	23.78	22.57	23.10	25.05	26.09	24.93
	Minimum..	20.72	21.34	21.59	22.27	25.54	23.85

*Seven tests. †Five tests.

Conditions of the Test.—Same as in Table I, except that each machine was equipped with new staves on starting, and the shot charges were of the same make in both laboratories. The cubic shot, however, were not made by the same foundry as the spherical shot.

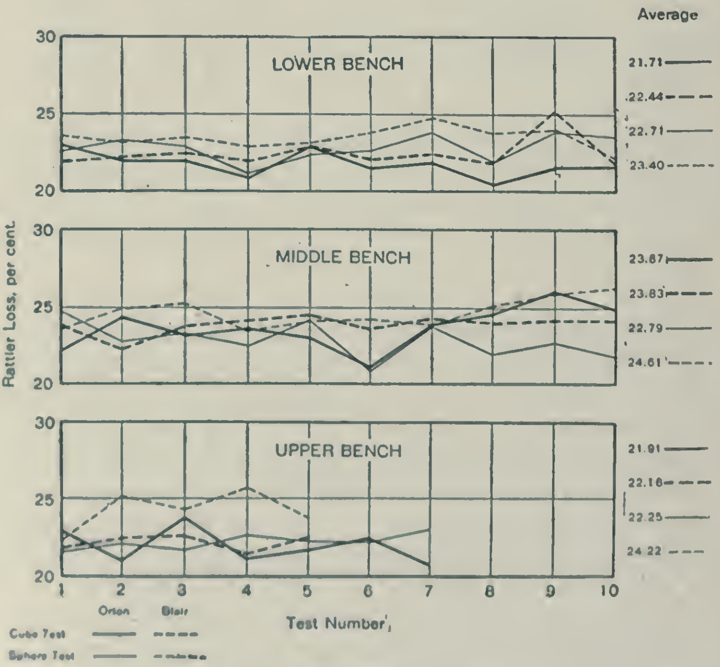

FIG. 3.—Curves of Series C (Partial).

relationship between the cube test and the sphere test has changed. In Orton's data, where the sphere test has heretofore given results 3 per cent. higher than the cubes, we now find it giving but slightly higher losses; while Blair, who heretofore found cubes giving persistently higher losses, now reverses his findings and shows spheres running 1.25 per cent. on the average higher than cubes.

When the investigation had reached this point, it was clear that the shot were in some way connected with our inability to check each other. Blair, therefore, sent these irons to Mr. L. A. Touzalin, Assistant Chief Chemist of the Illinois Steel Company, South Chicago, Ill., and they were carefully analyzed. The results are given in Table V.

TABLE V.—ANALYSIS OF CAST IRON SHOT.

Element.	O'Brien Cubes.	Over		Loudenslager Spheres.
		Cubes.	Spheres.	
Combined carbon........	0.45	1.46	1.59	2.76
Graphitic carbon.........	3.02	2.01	1.92	0.41
Silicon..................	1.73	1.63	1.24	0.96
Sulphur.................	0.08	0.18	0.18	0.15
Phosphorus.............	0.68	0.82	0.84	0.53
Manganese.............	0.51	0.28	0.27	0.27
Scleroscope readings	50.1	60.1	68.7	72.1

The data proved most illuminating. It showed the O'Brien iron to be practically a gray foundry iron, and very soft. The two batches from Over were of medium hard iron, but not nearly as hard as the white Loudenslager iron.

The relation between the hardness of the irons and their wearing power seems clearly indicated. Orton, in five series of tests, aggregating more than 100 comparisons, found spheres of hard white iron giving persistently higher losses than cubes of soft foundry iron. Blair, in two series of tests aggregating 60 comparisons, found spheres of moderately hard iron giving persistently lower results than cubes of the same iron. Blair, in one series of 25 comparisons, with hard spheres and soft cubes, checked Orton's findings, though with reduced differences between the two methods.

The foregoing evidence forced the conclusion that the composition of the metal to be used in the abrasive shot was a matter of critical importance—sufficient to completely overthrow any probability of making good checks between independent operators, unless using identical shot charges.

It was therefore concluded to abandon, at least temporarily, the scheme which had been originally planned, and take up a more critical investigation into the effects of different kinds of shot.

In order to still further simplify the work, it was desirable to continue the investigation with but one shape of shot. Before deciding which shape to employ, the data thus far obtained were carefully compared.

CUBIC SHOT *vs.* SPHERICAL SHOT.

The spherical shot had proven themselves superior in the following respects:

1. *Ease of Maintenance.*—The cubic shot loses corners and edges very rapidly. In the case of soft and medium foundry irons, the cubes have to be replaced at the rate of about 11 per charge, or 30 to 60 every five tests. This means that the average life of a cube is only 20 to 25 charges. Further, the point of rejection is a matter of some variability, depending upon the judgment of the operator. It is too laborious to accurately weigh each cube after each test, and any visual test involves the judgment as aforesaid. Spheres wear uniformly over their whole surface, from the beginning, and their rate of wear is much smaller than with new cubes. Cubes tend to become spheres by the wear of the rattler.

The relative rates of loss of iron with cubes and spheres of the same make are shown herewith:—

LOSSES IN POUNDS IN PERIODS OF FIVE TESTS.

	"Over" Cubes.	"Over" Spheres.
	9.25	1.35
	13.60	2.50
	7.15	1.10
	7.50	0.90
	7.85	1.30
	7.55	1.60
	6.00	....
Averages......	8.41	1.46

This shows six times as much wear on the cubes as on the spheres. Much more data could be given corroborating the above.

2. *Effectiveness.*—Where spheres and cubes of the same metal are used, so that no factor of variation is introduced except that of shape of the shot, the spheres are found to give lower abrasion losses. Tables VI and VII illustrate this fact.

TABLE VI. ORTON'S LABORATORY.

"LOUDENSLAGER" IRON (SECOND LOT).

BRICK, SERIES D. STAVES, LINED CHANNELS.

	Cubes.	Spheres.
	21.74	20.25
	25.70	21.50
	24.03	22.05
	23.75	19.24
	24.33	22.96
	23.71	22.06
	22.05	21.08
	24.20	21.49
	23.30	22.18
	22.27	21.98
Average.........	23.50	21.47
Maximum........	25.70	22.96
Minimum........	21.74	19.24

TABLE VII.—BLAIR'S LABORATORY.

"OVER" IRON OF MEDIUM HARDNESS.

	Series F (Lower Bench).		Series G (Lower Bench).		Series H (Middle Bench).	
	Cubes.	Spheres.	Cubes.	Spheres.	Cubes.	Spheres.
	18.3	21.0	17.0	15.4	29.9	29.0
	19.6	17.6	19.5	16.3	29.0	28.9
	18.5	18.3	17.9	15.2	33.3	27.9
	19.9	15.3	17.3	17.0	29.1	25.1
	24.9	21.3	17.2	17.5	30.8	26.3
	21.9	15.7	17.5	15.0	....	....
	19.5	17.1	17.3	14.6	....	....
	17.6	16.0	18.7	13.7	....	....
	17.3	15.7	19.9	15.0	....	....
	21.4	16.1	14.5	14.5	....	....
Average..........	19.8	17.4	17.96	15.39	30.4	27.4
Maximum........	24.9	21.3	19.9	17.5	33.3	29.9
Minimum.........	17.3	15.3	14.5	13.7	29.0	25.1

These illustrations are taken at random from a great mass of similar data, and show clearly that where the iron is the same the spheres give a consistently lower figure than the cubes, by about 2 per cent.

3. *Kind of Wear.*—Examination of the bricks tested does not disclose any characteristic differences in the kind of wear. The spheres seem to disclose cavities lying close to the surface of the bricks, just as the cubes do.

On the strength of the marked gain in economy of iron, and the slight difference in the rate of wear of the bricks, the spheres were selected as the basis of all future experiments.

A plan for a careful comparison of the behavior of shot made from different hard irons, with a view to fixing a chemical specification which would insure material of uniform value for this purpose, was now formulated. While the necessary supplies were being secured other work was being carried forward.

INTRODUCTORY WORK ON STAVES.

The stave originally adopted for the joint test was a plain 6-in. medium-steel channel, weighing 15.5 lbs. per linear foot. At the time that the first sets were replaced, as stated in reference to Series C, the peening action of the charge had warped each channel to a marked degree. It was evident that it would be impossible to successfully straighten them. And, as the warpage increased, the results of the test became more erratic and widely divergent, especially in Orton's machine, which was in the worst condition.

It will be recollected that at the beginning of Series C, both Orton's and Blair's machines were equipped with new channel steel staves of the original type.

During the time when Series C was being run, Blair used one of his machines to experiment upon a new set of channel staves lined with $\frac{3}{8}$-in. medium-steel wear-plates fastened to the face of the channel with 3 rivets, countersunk and dressed smooth. The results of the experiment were so encouraging as to justify the equipment of Orton's machine with a similar set of lined channel staves.

With machines in both laboratories equipped with lined channel staves, Series A and E were now carried nearly to completion. The shot used in both laboratories was Loudenslager's hard, white-iron spheres (Lot 1) and O'Brien's soft, gray-iron cubes.

Table VIII gives the summarized data of these two series.

TABLE VIII.—SUMMARY OF RATTLER TESTS, USING LINED STAVES.

BRICKS OF SERIES A (IN PART).

Position of Bricks in Kiln.		Cubic Shot, Soft Gray Iron.		Spherical Shot, Hard White Iron.		No Shot, 15 Bricks per Charge.	
		Orton, 5 tests.	Blair.	Orton, 5 tests.	Blair, 10 tests.	Orton, 5 tests.	Blair, 5 tests.
Lower Bench..	Average...	18.07	No data obtained.	20.97	20.34	25.12	23.57
	Maximum.	18.57		22.69	22.05	26.21	25.67
	Minimum..	16.87		20.08	18.33	22.98	22.29
Middle Bench.	Average...	16.65		18.12	19.14	23.25	21.73
	Maximum.	18.71		19.15	20.11	24.84	22.90
	Minimum..	15.24		17.22	17.15	21.58	20.00
Upper Bench..	Average...	18.26		18.48	19.84	22.26	21.28
	Maximum.	20.68		19.28	22.05	23.55	23.27
	Minimum..	16.20		17.03	17.42	21.88	20.27
BRICKS OF SERIES E (IN PART).							
Lower Bench..	Average...	18.01	17.71	20.85	18.23*	25.72	21.78
	Maximum.	18.73	19.07	22.70	19.66	26.66	22.66
	Minimum..	16.55	16.99	20.15	17.00	24.78	21.03
Middle Bench.	Average...	16.61	15.63	18.49	17.25*	23.15	20.87
	Maximum.	17.36	16.04	19.31	18.23	23.51	21.30
	Minimum..	15.36	15.21	16.84	15.60	22.74	20.44
Upper Bench..	Average...	16.52	17.06	17.26	17.36*	21.26	21.35
	Maximum.	17.45	17.77	17.72	18.01	22.65	21.89
	Minimum..	15.33	16.00	16.11	16.80	19.83	20.32

The salient points to be noted in Table VIII are as follows:

1. Both operators agree fairly well on both cube *vs.* cubes, and sphere *vs.* spheres.

2. As between cubes and spheres, the data merely serve as a check on Series C, since the newly planned shot were not yet available and the old spheres and cubes were still being used. The hard spheres were inflicting from 1.5 to 1.75 per cent. greater loss than the soft cubes, on the average, and the quality of the iron is thus again seen to overweigh the difference in the shape of the shot.

Examining now the influence of the new lined staves and the old un-lined staves, the comparison in Table IX can be compiled from Orton's data. It is shown graphically in Fig. 4.

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* Average of 5 tests, instead of 10 as in column above.

TABLE IX.—TESTS WITH LINED AND UN-LINED STAVES.

Comparisons on Five Tests Each.

SERIES A.

Position of Bricks in Kiln.		Old Staves. (Badly warped and rough.)	New Staves, Lined. (Smooth.)
Lower Bench	Cubes	20.76	18.07
	Spheres	23.54	20.97
Middle Bench	Cubes	18.97	16.65
	Spheres	21.95	18.12
Upper Bench	Cubes	19.99	18.26
	Spheres	23.40	18.48

SERIES E.

Position of Bricks in Kiln.		Old Staves. (Badly warped and rough.)	New Staves, Lined. (Smooth.)
Lower Bench	Cubes	20.15	18.01
	Spheres	23.56	20.85
Middle Bench	Cubes	18.39	16.61
	Spheres	21.06	18.49

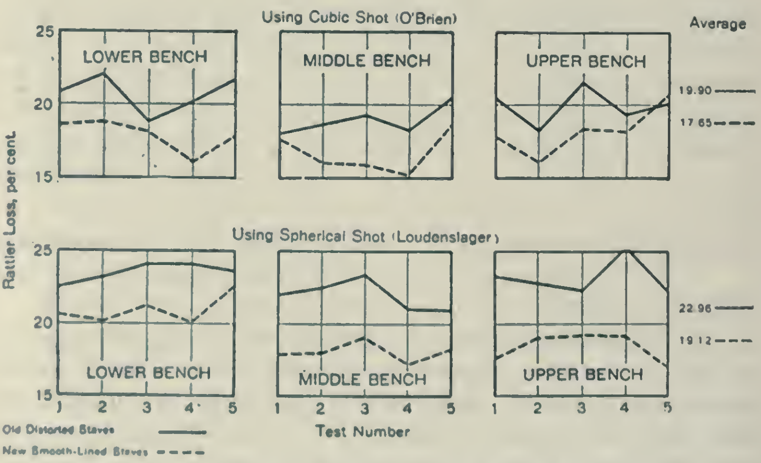

FIG. 4.—Influence of Condition of Staves on Rattler Losses. Series A (Orton's Results).

The rough interior of the barrel, affording better opportunity for the shot to climb, and thus fall farther, and also for bricks to strike against rough edges of staves, is thus seen to occasion an increased loss of 2.8 per cent. on the average.

At the end of this work the lined stave was still in excellent condition and presented only very slight convexities between rivets, due to peening.

THE CHICAGO OR "NO SHOT" TEST.

The data obtained by this process have been shown in their place in all of the tables thus far, but little attention has been given to them in the discussions. At this point the evidence was reviewed, to decide whether or not to continue this test.

For convenience, the averages of the "Chicago tests" of each series are assembled in Table X.

TABLE X.—"CHICAGO TEST" COMPARISONS.

Bench.	Orton.		Blair.		Remarks.
	Averages of 5 tests.	General Average (15 tests).	Averages of 5 tests.	General Average (15 tests).	
SERIES F.					
Lower....	22.68	22.46	21.20	21.0	Staves warped; interior of barrel rough.
Middle....	20.51		22.80		
Upper....	24.19		19.00		
SERIES G.					
Lower....	22.74	23.58	21.60	22.73	Staves very badly warped; interior of barrel very rough.
Middle....	23.69		21.90		
Upper....	24.33		24.70		
SERIES C.					
Lower....	27.27	26.82	24.62	25.16	Staves new; interior of barrel smooth.
Middle....	27.45		26.35		
Upper....	25.74		24.53		
SERIES A.					
Lower....	25.12	23.67	23.57	22.19	Staves lined; interior of barrel smooth.
Middle....	23.25		21.73		
Upper....	22.66		21.28		
SERIES E.					
Lower....	25.72	23.37	21.78	21.33	Staves lined; interior of barrel smooth.
Middle....	23.15		20.87		
Upper....	21.26		21.35		

The foregoing data show the following points:

1. The results in Blair's laboratory average 1.25 per cent. lower than in Orton's. The same fact can be found in the shot tests up to this point, and later in this report in the tentative standard series. The explanation arrived at is a difference of speed, being below the proper limit in Blair's machine. This difference was slight in the beginning, but became more important as the tests progressed, as shown later.

2. The tests run higher in actual losses than in the corresponding shot tests. Selecting for comparison the tests executed with Loudenslager spherical shot (first lot) we have the values shown in Table XI.

TABLE XI.—COMPARISON BETWEEN GENERAL RESULTS OF NO SHOT AND SPHERICAL SHOT TESTS.

Series.	No Shot.		Spherical Shot.	
	Orton's Averages.	Blair's Averages.	Orton's Averages.	Blair's Averages.
F.........	22.46	21.00	20.51	.....
G.........	23.58	22.73	21.16	.....
C.........	26.82	25.16	22.64	23.89
A.........	23.67	22.19	19.19	19.80
E.........	23.37	21.33	18.87	17.61

These figures are in nearly all cases the average of 15 charges each. The early sphere tests by Orton on Series A and E were excluded from the comparison on account of condition of staves. The sphere-test averages given for Series A and E are from the tests made when the staves had been lined. The corresponding all-brick charges were run at the same time and hence are comparable. The table shows a difference between the two methods amounting to about 3½ per cent. on the averages.

3. The same characteristic tendencies are shown in the various materials when tested by either of the two methods. Fig. 5 is a plot of the data given in Table XI.

From the foregoing, it may well be questioned why the shot test should not be abandoned and the simpler and cheaper all-brick test be revived.

So far as the evidence we have secured in this work is concerned, there is no reason why such a change might not be made.

But, at the time that the old all-brick test was abandoned in favor of the shot test, it was demonstrated by Talbot and others that the shot charges had superior selective wear, where hard and soft bricks occurred in the same charge. In those days, when material was not as uniformly hard as it is at present, the importance of continually watching for soft bricks was keenly felt, and it is not at all sure that this necessity does not still exist in many localities.

For this reason it was thought best not to recommend the change from the shot test, unless a more searching inquiry is made to demonstrate fully the relative selective capacity of the two methods. The present work, while interesting, was always performed on hard brick of rather uniform quality, and hence does not afford a good chance to study the whole problem.

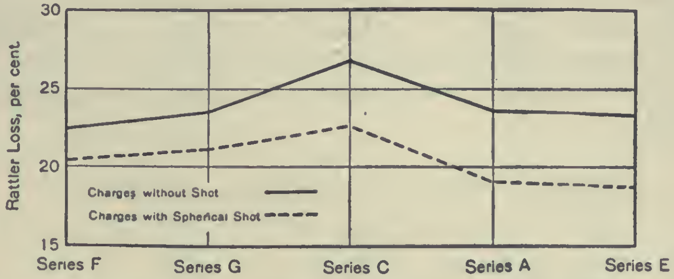

FIG. 5.—Comparison between Average Abrasion Losses of Charges without Shot and with Spherical Shot (Orton's Results).

THE RELATIVE PERFORMANCE OF SPHERICAL SHOT CHARGES MADE FROM DIFFERENT VARIETIES OF HARD IRON.

Three manufacturers, all represented to be able to furnish a homogeneous and uniform grade of very hard white iron, were employed to prepare batches of spherical shot of the two sizes. Each manufacturer was to furnish two separate batches, separating the first order from the second by a time interval of several weeks, in order to see whether the quality furnished by the same plant in consecutive shipments would vary much. There was great delay in getting these various lots and dividing them between the two laboratories, and of one variety of shot but one batch was ever secured.

With these various lots of hard-iron spherical shot, a series of tests was made upon a new lot of bricks, Series D. A shipment of 3,000 of this brand was obtained and divided equally between the laboratories. The shipment was a commercial one, and not selected for testing purposes as the previous lots had been. It therefore showed a somewhat greater variation within itself than some of the others.

Table XII gives the data obtained in making this shot comparison.

The Loudenslager Shot.—The physical character of this shot was poor. While fairly accurate in weight, they were very frequently full of blow-holes or dimpled with sink-holes, due to contraction in cooling. They were brittle, easily crushed with a heavy hammer, and occasionally broke in the rattler.

The analyses of the two lots of the Loudenslager shot are as follows:

	Lot 1.	Lot 2.
Combined Carbon	2.76 per cent.	1.48 per cent.
Graphitic Carbon	0.41 " "	1.47 " "
Silicon	0.96 " "	1.66 " "
Manganese	0.27 " "	0.18 " "
Phosphorus	0.53 " "	0.72 " "
Sulphur	0.15 " "	0.20 " "

The average results (10 tests each) given by these two lots of shot, all other things being equal, were:

	Lot 1.	Lot 2.
Orton	23.37	21.47
Blair	21.86	20.15

The second lot of shot varied widely in character from the first and gave distinctly lower wearing power in the rattler. The maximum and minimum figures are also lower in the second series, showing that it is not accidental. The lower results obtained with the second lot are exactly in line with the facts disclosed in Series F and G, where the Loudenslager shot (first lot) was compared with the Over shot from Indianapolis, whose composition was very similar to that of the second lot of Loudenslager shot.

The wide divergence in chemical composition in the Loudenslager material was deemed by us a sufficient reason for giving it no further consideration as a possible future standard material.

TABLE XII.—COMPARISON OF WORK OF DIFFERENT SPHERICAL SHOTS OF HARD IRONS.

II. LOUDENSLAGER FOUNDRY COMPANY, COLUMBUS, O.

	Lot 1.		Lot 2.	
	Orton.	Blair.	Orton.	Blair.
	21.06	21.25	20.25	17.94
	23.06	22.68	21.50	20.34
	20.96	20.91	22.05	18.70
	24.31	23.17	19.24	20.41
	23.63	22.95	22.96	20.14
	24.60	20.71	22.06	20.64
	23.37	21.14	21.08	20.49
	22.58	22.85	21.49	23.37
	26.41	21.87	22.18	19.03
	23.63	21.16	21.98	20.50
Average...	23.37	21.86	21.47	20.15
Maximum.	26.41	23.17	22.96	23.37
Minimum..	20.96	20.71	19.24	17.94

NATIONAL MALLEABLE CASTINGS COMPANY, INDIANAPOLIS, IND.

	21.78	20.50	24.25	21.39
	22.71	21.86	23.24	20.91
	21.47	20.61	21.78	22.57
	22.36	21.75	23.20	21.76
	21.31	20.86	24.40	21.20
	21.91	21.69	23.44	20.52
	20.95	21.99	24.29	21.22
	21.88	20.25	24.72	23.47
	20.79	21.08	22.12	22.14
	19.57	20.27	24.60	20.31
Average...	21.47	21.08	23.60	21.54
Maximum.	22.71	21.99	24.72	23.47
Minimum..	19.56	20.25	21.78	20.31

AMERICAN CAR AND FOUNDRY COMPANY, ST. LOUIS, MO.

	21.47	19.68	Not furnished.	
	21.70	21.41		
	22.58	19.45		
	21.94	19.11		
	24.43	20.85		
	21.47	18.43		
	24.00	19.90		
	21.80	21.04		
	22.61	20.53		
	24.07	.....		
Average...	22.60	20.04		
Maximum.	24.43	21.41		
Minimum..	21.47	18.43		

We had believed that its process of manufacture and the quality of materials would automatically, or by the self-interest of the maker, operate to keep its constitution and hardness about uniform, but the above demonstrates that this view was incorrect.

The American Car and Foundry Company Shot.—The physical character of this shot was a little better than the preceding, but not much. It was roughly cast and had some blow-holes, but was tough. None of it broke in the rattler. It cost more than twice as much as the preceding, but with little real gain in quality. Its uniformity is unknown, as no second batch was secured.

The analysis, and that of Loudenslager No. 1 for comparison, are given below.

	American Car and Foundry Company.	Loudenslager. (Lot 1.)
Combined carbon...	2.72 per cent.	2.76 per cent.
Graphitic carbon...	0.89 " "	0.41 " "
Silicon............	0.53 " "	0.96 " "
Manganese.........	0.31 " "	0.27 " "
Phosphorus........	0.109 " "	0.53 " "
Sulphur............	0.179 " "	0.15 " "

The average results (10 tests each) given by these two lots of shot, were:

	American Car and Foundry Company.	Loudenslager. (Lot 1.)
Orton.............	22.60	23.37
Blair..............	20.04	21.86

The influence of the increased proportion of graphite in the American Car and Foundry Company shot is apparent, even where the combined carbon is so closely similar.

The National Malleable Castings Company Shot.—The physical character of this material was excellent. It was beautifully smooth and regular, free from blow-holes, very tough, and absolutely unbreakable with a sledge. The chemical composition of every heat from which spheres were cast was obtained from the heat-book of the company and was exceedingly regular. The metal was not a cupola-melted cast iron. It was melted in an air-furnace, and held there, subject to chemical influences which gradually converted its carbon wholly into the combined condition. It was tapped out and cast only when test showed it to be converted to this new condition. It was not, therefore, properly comparable with an ordinary cast iron shot, though obtainable at about the same price. Table XIII gives the analyses of the two lots.

TABLE XIII.

	Lot 1.			Lot 2.		
	Average of 22 Analyses.	Maximum.	Minimum.	Average of 10 Analyses.	Maximum.	Minimum.
Combined carbon	2.74	2.96	2.51	2.88	3.21	2.75
Graphitic carbon	....	....	....	....	....	....
Silicon.........	0.82	1.00	0.70	0.78	0.92	0.67
Manganese.....	0.26	0.30	0.19	0.23	0.26	0.20
Phosphorus.....	0.17	0.18	0.15	0.19	0.25	0.15
Sulphur........	0.051	0.07	0.03	0.05	0.05	0.04

The results of the rattler test (average of 10 tests each) conducted with this shot are:

	Lot 1.	Lot 2.
Orton..............	21.47	23.60
Blair	21.08	21.54

The remarkable uniformity of the above material and its physical perfection constitute a strong endorsement for the use of shot produced by this process of manufacture.

Influence of Combined vs. Graphitic Carbon.—A study of the above data seems to point to the conclusion that the graphite is associated with the diversified wearing power of different irons. The data of Table XIV lend color to this view.

TABLE XIV.

Combined Carbon.	Graphitic Carbon.	Averages of 10 Tests Each.	
		Orton.	Blair.
2.88	trace	23.60	21.54
2.74	trace	21.54	21.08
2.76	0.41	23.37	21.86
2.72	0.89	22.60	20.04
1.48	1.47	21.47	20.15

A similar effect can also be observed in comparing graphitic cast-iron staves and hard white-iron staves. The effect of the graphite probably is that of slight lubrication, promoting flow and decreasing opportunity for "climbing" of the shot, which reduces impact. A small piece of soap introduced into a rattler exercises a very pronounced similar effect.

For the above reasons, it is believed that the air-furnace iron, devoid of all but traces of graphitic carbon, offers far the greatest chance of uniformity of any kind of iron attainable.

Influence of Silicon.—Silicon is known to strongly affect the brittleness of cast iron. It does not affect the hardness of the metal itself, at least not within the limits considered in the present connection. Cast iron high in silicon would be more likely to crush in the rattler. Air-furnace iron is more likely to be low in this substance than cupola-melted iron.

Manganese.—Manganese lends toughness, if the quantity be high, but it is not feasible to secure cast iron high in manganese except by making it to order and at great cost.

Phosphorus and Sulphur.—Phosphorus makes "cold short" or brittle cast iron. Sulphur makes "hot short" steel—its effect on cast iron is probably in the direction of brittleness.

Table XV gives a comparison of the relative behavior of the iron constituents of the charges mentioned above.

TABLE XV.—LOSSES IN WEIGHT (IN POUNDS) OF CHARGES OF IRON SPHERES; (300 LBS.) IN PERIODS OF FIVE RATTLINGS EACH.

Period.	Loudenslager Spheres.		National Malleable Casting Company's Spheres.
	Lot 1.	Lot 2.	Lot 1.
1	2.06	1.57	0.87
2	3.00	2.45	0.37
3	1.75	1.76	0.57
4	1.25	....	0.62
5	1.75	....	0.63
6	....	....	0.56
7	....	....	0.93
Averages.	1.96	1.93	0.65

The greater brittleness of the Loudenslager material, due to its higher silicon phosphorus and sulphur, is strongly shown in the relative losses in weight of iron. Many crushed and broken Loudenslager spheres were found in the rattler and removed from time to time, but never one of the National Malleable Company's make. The presence of 0.41 per cent. of graphite in the former is not able to offset the effect of the above-mentioned ingredients.

THE RELATIVE PERFORMANCE OF DIFFERENT TYPES OF STAVES.

The work on Series A and E showed clearly that the lined channel stave was a very great improvement over the plain one. Because of its compound structure, it was thought proper to subject it to a searching comparison with other types of simple, single-piece staves, before deciding on its adoption.

There had been some researches made at Chicago and Cleveland on the stave question, and although these results were unpublished we were informed that they indicated that differences in the thickness, rigidity and hardness of the staves influenced the results very markedly. Hence, it was decided to procure a series of different staves and test them out in a competitive way, by running ten tests in each laboratory as follows:—

1. Lined channel staves, as described.
2. Steel plates, $\frac{1}{4}$ in. thick.
3. Steel plates, $\frac{1}{2}$ in. thick.
4. Steel plates, $\frac{3}{4}$ in. thick.
5. Manganese steel plates, $\frac{3}{4}$ in. thick.
6. Soft cast iron plates, $\frac{3}{4}$ in. thick.
7. Hard white cast iron plates, $\frac{3}{4}$ in. thick.

The manganese steel staves and both hard and soft cast iron staves were made from the same drawing, but different patterns were prepared in each case. In making these stave tests the Loudenslager spheres (Lot 1) were used exclusively by both operators. The comparison was run upon Series D material, the same as that used for the shot tests just discussed. Table XVI gives the data obtained.

Analysis of the Data of the Stave Test.—The theory upon which the stave investigation was begun was that the rigidity or springiness of the stave was a matter of profound concern. This idea has been somewhat urgently advocated as the result of tests made in Chicago a year or two ago. Studying the results of our series, the data in Table XVII may be marshalled.

TABLE XVI.—SHOWING RELATIVE PERFORMANCE OF DIFFERENT TYPES OF STAVES.

	Lined Channels.		½-in. Steel Plate Staves.		⅜-in. Steel Plate Staves.		¼-in. Steel Plate.	
	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.
	21.06	21.25	21.40	23.72	20.49	21.52	20.45	20.32
	23.06	22.68	22.28	22.65	22.35	22.53	21.72	18.42
	20.96	20.91	21.21	24.57	22.13	21.26	22.61	20.72
	24.31	23.17	21.57	23.33	22.02	22.52	22.47	21.38
	23.63	22.95	22.61	23.24	22.87	21.25	25.12	22.02
	24.60	20.71	23.87	21.69	22.22	20.98	20.49	19.84
	23.37	21.14	22.05	24.46	20.86	21.83	22.06	21.37
	22.58	22.85	23.01	25.25	21.54	20.76	25.12	22.60
	26.41	21.87	25.64	22.54	22.16	20.76	22.48	19.63
	23.63	21.16	23.30	23.55	23.08	22.66	22.43	21.66
Average	23.37	21.86	22.59	23.50	21.97	21.60	22.49	20.796
Maximum	26.41	23.17	25.64	25.25	23.08	22.53	25.12	22.60
Minimum	20.96	20.71	21.21	21.69	20.49	20.76	20.45	18.42

	Manganese Steel Plate.		Cast Iron (Soft Gray).		Cast Iron (Hard White).	
	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.
	20.37	21.00	19.39	20.26	20.61	Not Tested.
	21.23	21.50	19.43	19.18	20.13	
	20.78	20.56	19.47	17.09	21.93	
	21.81	19.71	20.64	21.39	21.55	
	23.44	20.28	21.13	20.37	20.43	
	20.21	20.87	21.41	20.02	20.00	
	21.15	19.54	17.69	19.56	20.05	
	20.97	21.70	21.24	20.04	21.25	
	20.55	22.14	19.65	19.57	20.84	
	20.76	21.61	19.62	18.12	19.73	
Average	21.12	20.79	19.96	19.56	20.65	
Maximum	23.44	22.14	21.41	21.39	21.93	
Minimum	20.21	19.54	17.69	17.09	19.73	

TABLE XVII.

Variety of Stave.	Orton's Results, 10 tests.	Blair's Results, 10 tests.	Combined Results, 20 tests.
¼-in. steel plate (medium soft)	22.59	23.50	23.04
½-in. " " " ")	21.97 21.36	21.60	21.78
¾-in. " " " ")	22.49	20.79	21.64
15.5-lb. 6-in., medium steel channel, lined with ¾-in. medium steel wear plates	23.37	21.86	22.61
3-in. cast staves (manganese steel, intensely hard and rigid)	21.12	20.79	20.95
¾-in. cast staves (soft machinery iron)	19.96	19.56	19.76
¾-in. " " (hard white iron)	20.65	.....	.....

1. Comparison of the data for the first four staves, all of which are made of medium steel, and which are arranged in order of rigidity, does not lend strength to the foregoing view. The fluctuations seem erratic and not greater than the variability of the material tested might easily explain.

2. A second idea in the stave test was that hard surfaces would react upon an impinging brick differently from a soft or slippery graphitic surface. The known superiority of the special steels made for abrasion and grinding machinery, led us to try the use of a manganese steel stave. This material is not only intensely hard, but also very tough and may be bent considerably before breaking. The comparison of the data in the last four sets of staves—soft steel, hard steel, soft cast-iron, hard cast-iron—shows:

(*a*) That the hard staves, both cast-steel and cast-iron, show less losses than the soft-steel staves.

(*b*) That the soft cast-iron stave shows smaller losses than any other kind of stave.

These data, while not conclusive, or indicating that the material of which the staves are made is of vital importance, do still seem to show that the metal of which the stave is made has some slight effect on the results. It was considered to be sufficient to justify the exclusion of a choice of stave material and warrant the specification of one material only as the standard equipment.

3. The third point developed by this study is that the form, or the ability to retain its original form, is more important than anything else. The early tests of Series F, G, A, and E, all showed conclusively that as the staves became distorted by peening, they created a rough interior to the barrel and thus changed the

conditions, and made the results more erratic, and increased the actual rate of wear by 2 per cent. or more. This is very clearly shown in Table IX.

The data show that while the thin (and therefore springy) steel-plate staves are not inherently worse than others when new, they are not to be recommended because they peen rapidly and become distorted so soon. If used at all, they would require very frequent straightening, which is costly and likely to be neglected.

The recommendation in favor of the channel steel stave is its superior rigidity compared to a flat steel plate. But the work of Series F and G and parts of Series A and E show clearly that it is still too easily distorted to be a good investment, and also, when distorted, it is practically out of the question to straighten it again, while a flat plate can be straightened by any blacksmith.

The use of a face-plate to take up the peening and wear, and permit the channel stave to furnish the permanent support for it, has proved a very satisfactory step. The distortion of the unlined channel staves was so bad after 150 tests that they were continually stripping off heads of bolts, or tearing out bolt holes. Five boxes of bolts were used in repairs on one machine in about 200 tests. The lined channel stave completely avoids this difficulty since it remains rigidly in position; its bolts seldom work loose and never pull in two, or strip. The wear plate, on the other hand, being riveted firmly in its place at three points on its center line, and free to expand in most all directions under the peening action of the shot, is under no great stress, and while it will occasionally work loose and require re-riveting, this repair can be done easily and quickly by any ordinary mechanic or testing-machine operator. The peening action causes the wear plate to buckle up between rivets, presenting a slightly convex surface and fitting down tightly on the channel around the edges of the plate. This convexity does not exceed ½ in. in height after 150 tests. The cost of relining the channel is not great and thus the staves can easily be kept in good condition.

RESUMPTION OF THE ORIGINAL COMPARISONS.

The completion of the foregoing work now brought us to a point where the original plan could be resumed: that is, to determine what degree of concordance could be obtained between two

TABLE XVIII.—COMPARISONS BY THE REVISED METHOD OF TESTING.

	Series A. (Middle Bench Only.)		Series C.		Series D.		Series E. (6 from Middle Bench and 4 from Lower Bench.)		Series F. (4 Top, 3 Middle, 3 Lower.)	
	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.
	18.78	19.22	23.80	23.78	21.78	20.50	18.55	18.08	17.51	16.16
	20.06	18.96	24.44	23.82	22.71	21.86	18.23	19.89	18.44	16.06
	20.24	18.29	23.95	24.11	21.47	20.61	20.09	16.81	18.75	15.42
	18.72	17.83	24.97	23.84	22.36	21.75	18.08	17.89	18.63	17.25
	19.47	19.65	24.49	23.09	21.31	20.86	19.02	18.20	17.99	16.80
	19.26	18.94	.....	.....	21.91	21.69	18.83	19.03	17.30	19.72
	18.96	17.24	.....	.....	20.95	21.99	19.60	17.82	17.48	17.49
	19.89	18.88	.....	.....	21.88	20.25	20.54	15.69	19.51	15.38
	20.38	19.54	.....	.....	20.79	21.08	20.87	16.82	18.70	17.86
	20.12	19.77	.....	.....	19.56	20.27	19.37	17.84	17.74	14.44
Average...	19.59	18.83	24.33	23.72	21.47	21.08	19.31	17.80	18.20	16.95
Maximum.	20.38	19.77	24.49	24.11	22.71	21.99	20.87	19.89	19.51	19.72
Minimum..	18.72	17.24	23.80	23.09	19.56	20.25	18.08	15.69	17.30	14.44

	Series G. (4 Top and 6 Middle.)		Series H. (All Middle Bench.)		Series K.		Series M.		Series P.	
	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.	Orton.	Blair.
	19.14	16.35	25.67	27.20	20.30	16.40	18.41	21.03	16.40	15.34
	17.90	15.50	30.47	25.59	18.42	17.42	18.08	17.59	16.46	16.43
	18.45	17.68	30.23	28.87	17.68	17.41	20.36	19.26	16.81	16.95
	18.24	17.27	25.66	24.41	19.42	16.49	18.40	18.62	17.40	17.70
	18.60	17.35	29.76	26.03	18.97	18.54	18.78	17.97	16.84	.....
	19.38	16.54	23.33	27.76	17.59	17.56	19.64	21.99	.....	.....
	18.53	17.11	25.07	24.59	20.23	17.20	17.24	16.94	.....	.....
	19.35	16.22	28.34	25 07	18.57	18.04	19.25	19.67	.....	.....
	19.13	16.37	29.44	27.26	18.46	18.23	18.38	17.91	.....	.....
	18.81	17.58	30.25	25.65	19.63	18.37	20.20	21.65	.....	.....
Average...	18.77	17.39	27.82	26.24	18.92	17.56	19.07	19.25	16.78	16.60
Maximum.	19.58	17.68	30.47	27.76	20.30	18.54	20.37	21.65	17.40	17.70
Minimum..	17.90	15.50	23.33	24.41	17.59	16.40	17.24	16.94	16.40	15.34

operators when testing the same brick and following the same specifications. Irregularities due to variation in the quality of the shot and the kind and condition of the staves, which had not previously been known, could now be provided for. The other variable factors in the rattler test, such as rate of rotation, duration of rotation, number of bricks per charge, etc., having been covered in the old specifications and in use for ten years, had been rather well determined, and no developments in this investigation had given any reasons to suggest their overthrow.

For the new comparisons, the following plan was made:

First: To secure twenty charges of ten bricks each from ten different manufacturers of paving bricks—a total of 200 charges.

Second: These were to be divided equally between the two laboratories to be tested by the new plan.

Third: The samples to be selected were to be taken as far as possible from the remainder of the large car-load samples used in Series A, C, D, E, F and G. The other four samples were to be secured direct from the manufacturers and were to represent what they considered to be first-class material—perhaps not their best, but certainly well above the average in uniformity and exterior excellence. Of the six samples taken from the old series, there were not enough bricks available from a single bench of the kiln to make the requisite 20 charges of each brand.

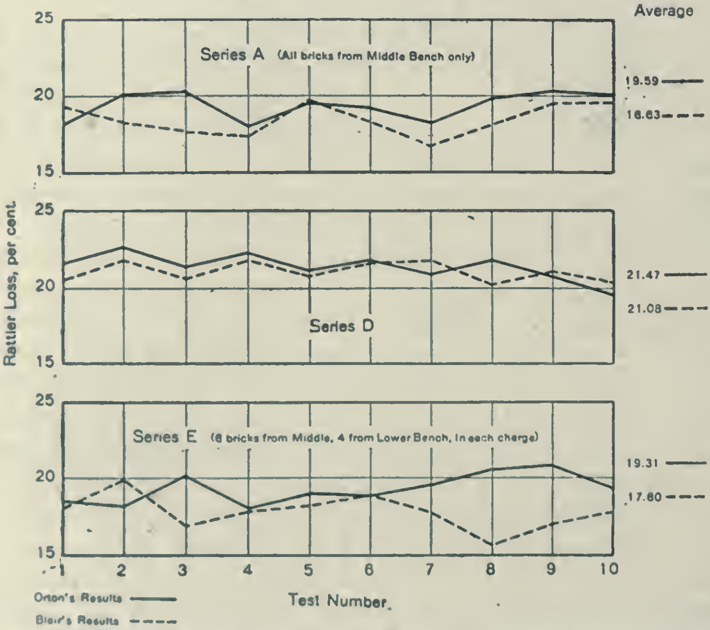

FIG. 6.—Degree of Concurrence attained with Improved Spherical Shot and Lined Steel Staves.

In this case, charges were compounded from two or even three benches. When this was done, each charge was made to contain the same number of bricks from the several benches as every other charge in the same series, thus making the charges strictly comparable.

Fourth: The method of testing was as follows:

(*a*) The shot charge should consist of ten $3\frac{3}{4}$-in. spheres, weighing as nearly as possible 75 lbs., and as many $1\frac{7}{8}$-in. spheres made by the National Malleable Castings Company, the properties of which have been discussed, as would bring the combined weight to 300 lbs.

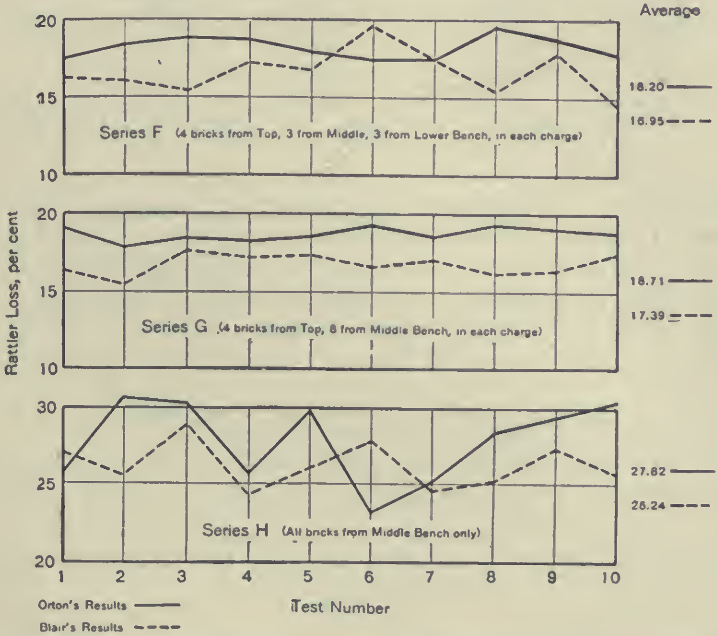

FIG. 7.—Degree of Concurrence attained with Improved Spherical Shot and Lined Steel Staves.

(*b*) The lined channel steel staves were to be used and relined whenever peened to a serious extent, and in no case to be run more than 150 tests without renewal.

(*c*) Other conditions were to remain the same as in the original procedure.

The results of these comparisons are given in Table XVIII, and are plotted in Figs. 6, 7 and 8.

The study of the foregoing shows that Orton's averages are regularly higher than Blair's. The amounts are not large: +0.76, 0.61, 0.39, 1.51, 1.25, 1.36, 1.58, 1.36, 0.18, and one −0.18. The average of these amounts is +0.88, Orton over Blair. If the sign of these differences was not practically one way they would be construed as satisfactory checks. But, since they point persistently in one direction, there must be a reason for it.

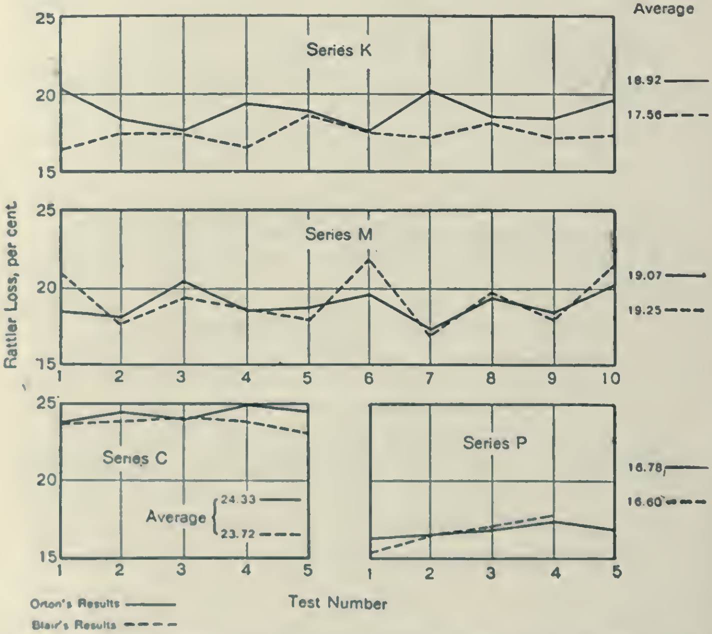

FIG. 8.—Degree of Concurrence attained with Improved Spherical Shot and Lined Steel Staves.

Influence of Speed of Rotation.—The data of each observer was again searched and a difference in speed was found, which had been noted earlier in the joint work and which was trifling at that time,

TABLE XIX.—COMPARISON OF RATTLER LOSSES AND SPEED OF REVOLUTION.

	Orton.		Blair.	
	Rattler Losses.	Average Speed, R. P. M.	Rattler Losses.	Average Speed, R. P. M.
	18.55	30.12	18.08	29.75
	18.23	29.71	19.89	29.50
	20.09	29.62	16.81	27.75
	18.08	29.71	17.89	28.33
	19.02	29.75	18.20	27.69
	18.83	30.17	19.03	30.10
	19.60	30.04	17.82	29.38
	20.54	30.34	15.69	27.75
	20.87	30.25	16.82	28.57
	19.37	30.42	17.84	28.33
Average...	19.31	30.01	17.80	28.71

but which had now become much more considerable. Table XIX from Series E, which shows about the highest average discrepancy between Orton and Blair, was selected to illustrate this point.

There would seem to be but little doubt that these low speeds on Blair's machine during the winter months of 1910–11 were responsible in whole or in part for the persistently low results which he secured.

In order to obtain a final comparison, in a laboratory in which this speed factor could be corrected, arrangements were made for Blair to use a newly installed machine in the Laboratory of the

TABLE XX.—COMPARISON, SERIES F (SECOND), UNDER CONTROLLED SPEED CONDITIONS.

	Orton.			Blair.		
	Loss.	R.P.M.	Remarks.	Loss.	R.P.M.	Remarks.
	17.25	30.04	Lining was near its point of rejection; viz., 128–133 tests. It was warped and peened considerably.	15.78	30.00	Lining was new and in good order; only 3 or 4 tests had been run on it.
	16.94	30.08		15.24	29.84	
	19.27	30.08		16.59	29.75	
	17.55	30.04		19.13	29.71	
	18.92	30.04		17.03	29.71	
	16.55	30.22	New lining was installed complete and these five tests run upon it without preliminary wear.	18.56	29.75	
	15.86	30.25		16.20	29.75	
	15.44	29.96		14.58	29.99	
	15.65	29.75		16.04	29.79	
	18.46	29.96		14.59	29.71	
Average.	17.19	30.04		16.37	29.80	

Ohio State Highway Commission. This machine was a duplicate of that used in the rest of this study. The bricks were 200 in number taken from a commercial shipment standing upon the street ready for laying. Each operator received 100 and made 10 tests. The results are given in Table XX.

The last five tests made by Orton in the above table, and the entire ten by Blair are about as closely comparable as can be obtained. The first five tests by Orton were made with the lining about ready for rejection, which may or may not have affected the results.

There are too few data available to prove with certainty that low speed was the cause of Blair's results running lower than Orton's in the final ten comparisons, but this presumption has been somewhat strengthened by the above test.

CONCLUSIONS.

1. Spherical shot, of the quality used in the latter part of this study, have very great advantages over cubic or rectangular shot. The losses of weight of the shot themselves is reduced to a mere trifle, the condition of the charge changes very slowly, and is much cheaper to maintain.

2. The lined steel channel stave has the advantage of any other kind known to the writers in that it can be kept in good order with the minimum expense or attention. The distortion due to peening, which is shown by all staves, is here taken up by the wearplate, which is inexpensive and easily renewed.

3. The speed of rotation is believed to exert enough influence on the results to justify closer limits than were permitted under the old standards. It is believed to be one very fruitful cause of the difficulties experienced by operators in checking each other.

4. There remain some other factors of the rattler test not covered in the old specifications, which have not as yet been investigated. Their influence may be found to be noticeable by careful study and it may be desirable to cover them by exact specification. Some of these points are (*a*) the method of supporting the rotating barrel; and (*b*), the method of driving the barrel, whether by steam engine, gas engine or electric motor, or by

direct, belt, rope, chain, or friction drive. In the opinion of the writers, no great importance attaches to these factors and some divergence of practice may continue in these respects without destroying the ability of different operators to check each other within the limits which the natural fluctuation of the material itself imposes.

5. The use of a uniform data sheet for recording and reporting rattler tests is very important. It greatly assists in securing uniformity in doing the work.

RECOMMENDATIONS.

In view of the above conclusions we present the following revised specifications for the rattler test upon paving bricks, to take the place of the specifications of the National Brick Manufacturers Association of 1901.

REVISED SPECIFICATIONS FOR THE RATTLER TEST UPON PAVING BRICK.

THE RATTLER

The machine shall be of good mechanical construction, self-contained, and shall conform to the details of material and dimensions, as set forth in the following specifications.

The Barrel.—The barrel of the machine shall be made up of the heads, headliners and staves.

The heads shall be cast with trunions in one piece. The trunion bearings shall not be less than two and one-half ($2\frac{1}{2}$) inches in diameter or less than six (6) inches in length.

The heads shall not be less than three-fourths ($\frac{3}{4}$) inch thick nor more than seven-eighths ($\frac{7}{8}$) inch. In outline they shall be a regular fourteen-sided (14) polygon inscribed in a circle twenty-eight and three-eighths ($28\frac{3}{8}$) inches in diameter. The heads shall be provided with flanges not less than three-fourths ($\frac{3}{4}$) inch thick and extending outward two and one-half ($2\frac{1}{2}$) inches from the inside face of head to afford a means of fastening the staves. The flanges shall be slotted on the outer edge, so as to provide for two (2) three-fourths ($\frac{3}{4}$) inch bolts at each end of each stave, said slots to be thirteen-sixteenths ($\frac{13}{16}$) inch wide and two and three-fourths ($2\frac{3}{4}$) inches center to center. Under each section of the flanges there shall be a brace three-eighths ($\frac{3}{8}$) inch thick and extending down the outside of the head not less than two (2) inches. Each slot shall be provided with recess for bolt head, which shall act to prevent the turning of the same. There shall be for each head a cast iron headliner one (1) inch in thickness and conforming to the outline of the head, but inscribed in a circle twenty-eight and one-eighth ($28\frac{1}{8}$) inches in diameter. This liner or wear plate shall be fastened to the head by seven (7) five-eighths ($\frac{5}{8}$) inch cap screws, through the head from the outside. These wear plates, whenever they become worn down one-half ($\frac{1}{2}$) inch below their initial surface level, at any point of their surface, must be replaced with new. The metal of which these wear plates are to be composed shall be what is known as hard machinery iron, and must contain not less than one (1) per cent. of combined carbon. The faces of the polygon must be smooth and give uniform bearing for the staves. To secure the desired uniform bearing, the faces of the head may be ground or machined.

The Staves.—The staves shall be made of six (6) inch medium steel structural channels twenty-seven and one-fourth ($27\frac{1}{4}$) inches long, weighing fifteen and five-tenths (15.5) pounds per linear foot.

The channels shall be drilled with holes thirteen-sixteenths ($\frac{13}{16}$) inch in diameter, two (2) in each end, for bolts to fasten same to head, the center line of the holes being one (1) inch from either end and one and three-eighths ($1\frac{3}{8}$) inches either way from the longitudinal center line.

The space between the staves will be determined by the accuracy of the heads, but must not exceed five-sixteenths ($\frac{5}{16}$) inch. The interior or flat side of each channel must be protected by a lining or wear plate three-eighths ($\frac{3}{8}$) inch thick by five and one-half ($5\frac{1}{2}$) inches wide by nineteen and three-fourths ($19\frac{3}{4}$) inches long. The wear plate shall consist of medium steel plate, and shall be riveted to the channel by three (3) one-half ($\frac{1}{2}$) inch rivets, one of which shall be on the center line both ways and the other two on the longitudinal center line and spaced seven (7) inches from the center each way. The rivet holes shall be countersunk on the face of the wear plate and the rivets shall be driven hot and chipped off flush with the surface of the wear plate. These wear plates shall be inspected from time to time, and if found loose shall be at once reriveted, but no wear plate shall be replaced by a new one except as the whole set is changed. No set of wear plates shall be used for more than one hundred (100) tests under any circumstances. The record must show the date when each set of wear plates goes into service and the number of tests made upon each set.

The staves when bolted to the heads shall form a barrel twenty (20) inches long, inside measurement, between wear plates. The wear plates of the staves must be so placed as to drop between the wear plates of the heads. These staves shall be bolted tightly to the heads by four (4) three-fourths ($\frac{3}{4}$) inch bolts, and each bolt shall be provided with lock nuts, and shall be inspected at not less frequent intervals than every fifth (5th) test and all nuts kept tight. A record shall be made after each such inspection, showing in what condition the bolts were found.

The Frame and Driving Mechanism.—The barrel should be mounted on a cast-iron frame of sufficient strength and rigidity to support same without undue vibration. It should rest on a rigid foundation and be fastened to same by bolts at not less than four (4) points.

It should be driven by gearing whose ratio of driver to driven should not be less than one (1) to four (4). The counter shaft upon which the driving pinion is mounted should not be less than one and fifteen-sixteenths ($1\frac{15}{16}$) inches in diameter, with bearings not less than six (6) inches in length and belt driven, and the pulley should not be less than eighteen (18) inches in diameter and six and one-half ($6\frac{1}{2}$) inches in face. A belt of six (6) inch double-strength leather, properly adjusted, so as to avoid unnecessary slipping, should be used.

ABRASIVE CHARGE.

(*a*) The abrasive charge shall consist of two sizes of cast-iron spheres. The larger size shall be three and seventy-five-hundredths (3.75) inches in diameter when new and shall weigh when new approximately seven and five-tenths (7.5) pounds (3.40 kilos) each. Ten shall be used.

These shall be weighed separately after each ten (10) tests, and if the weight of any large shot falls to seven (7) pounds (3.175 kilos) it shall be discarded and a new one substituted; provided, however, that all of the large shot shall not be discarded and substituted by new ones at any single time, and that so far as possible the large shot shall compose a graduated series in various stages of wear.

The smaller size spheres shall be when new one and eight hundred seventy-five-thousandths (1.875) inches in diameter and shall weigh not to exceed ninety-five-hundredths (0.95) pounds (0.430 kilos) each. Of these spheres so many shall be used as will bring the collective weight of the large and small spheres most nearly to three hundred (300) pounds, provided that no small sphere shall be retained in use after it has been worn down so that it will pass a circular hole one and seventy-five-hundredths (1.75) inches in diameter, drilled in a cast-iron plate one-fourth ($\frac{1}{4}$) inch in thickness or weigh less than seventy-five-hundredths (0.75) pounds (0.34 kilos). Further, the small spheres shall be tested by passing them over such an iron plate drilled with such holes, or shall be weighed after every ten (10) tests, and any which pass through or fall below specified weight, shall be replaced by new spheres, and provided, further, that all of the small spheres shall not be rejected and replaced by new ones at any one time, and that so far as possible the small spheres shall compose a graduated series in various stages of wear. At any time that any sphere is found to be broken or defective it shall at once be replaced.

(*b*) The iron composing these spheres shall have a chemical composition within the following limits:

Combined carbon	Not less than 2.50 per cent.
Graphitic carbon	Not more than 0.10 per cent.
Silicon	Not more than 1 per cent.
Manganese	Not more than 0.50 per cent.
Phosphorus	Not more than 0.25 per cent.
Sulphur	Not more than 0.08 per cent.

For each new batch of spheres used the chemical analysis must be furnished by the maker, or be obtained by the user, before introduction into the charge, and unless the analysis meets the above specifications, the batch of spheres shall be rejected.

BRICK CHARGE.

The number of bricks per charge shall be ten (10) for all bricks of the so-called "block size" whose dimensions fall between from eight (8) to nine (9) inches in length, three (3) and three and three-fourths (3¾) inches in breadth and three and three-fourths (3¾) and four and one-fourth (4¼) inches in thickness. No block should be selected for test that would be rejected by any other requirements of the specifications.

The brick shall be clean and dried for at least three (3) hours in a temperature of one hundred (100) degrees Fahrenheit before testing.

SPEED AND DURATION OF REVOLUTION.

The rattler shall be rotated at a uniform rate of not less than twenty-nine and one-half (29½) nor more than thirty and one-half (30½) revolutions per minute, and eighteen hundred (1,800) revolutions shall constitute the standard test.

A counting machine shall be attached to the rattler for counting the revolutions. A margin of not to exceed ten (10) revolutions will be allowed for stopping. Only one (1) start and stop per test is regular and acceptable.

CALCULATION OF THE RESULTS.

The loss shall be calculated in percentage of the original weight of the dried brick composing the charge. In weighing the rattled brick, any piece weighing less than one (1) pound shall be rejected.

RECORDS.

(*a*) The operator shall keep an official book, in which the alternate pages are perforated for removal. The record shall be kept in duplicate, by use of a carbon paper between the first and second sheets, and when all entries are made and calculations are completed the original record shall be removed and the carbon duplicate preserved in the book. All calculations must be made in the space left for that purpose in the record blank, and the actual figures must appear. The record must bear its serial number and be filled out completely for each test, and all data as to dates of inspection and weighing of shot and replacement of worn-out parts must be carefully entered, so that the records remaining in the book constitute a continuous one. In event of further copies of a record being needed, they may be furnished on separate sheets, but in no case shall the original carbon copy be removed from the record book.

(*b*) The blank form upon which the record of all official brick tests is to be kept and reported is:

REPORT OF
STANDARD RATTLER TEST OF PAVING BRICK

IDENTIFICATION DATA

Serial No..................

Name of the firm furnishing sample..

Name of the firm manufacturing sample..

Street or job which sample represents..

Brands or marks on the brick..

Quantity furnished ....Drying treatment ....

Date received..................................Date tested..................................

Length..............................Breadth..............................Thickness ....

STANDARDIZATION DATA

Number of charges tested since last inspection

Weight of Charge (After Standardization)	Condition of Locknuts on Staves	Condition of Scales
10 Large spheres		
Small spheres		
Total		

Number of charges tested since stave linings were renewed

Repairs (Note any repairs affecting the condition of the barrel)

RUNNING DATA

Time Readings				Revolution Counter Readings	Running Notes, Stops, etc.
	Hours	Minutes	Seconds		
Beginning of test.					
Final Reading....					

WEIGHTS AND CALCULATIONS

Initial Weight of 10 Bricks...		Percentage Loss (Note.—The Calculation Must Appear)
Final Weight of Same........		
Loss of Weight..............		

Number of broken bricks and remarks on same..

I certify that the foregoing test was made under the specifications of

....

....and is a true record.

(Signature of Tester)

....

Date............................ Location of Laboratory

For the use and convenience of others, a drawing of a machine which meets the above specifications is furnished herewith. It is not a part of the specifications. It is important, however, in the opinion of the writers, that uniformity in apparatus and equipment shall prevail to the largest possible extent, as an aid in securing uniformity of results, and close adherence to the type of machine here presented is therefore greatly to be desired.

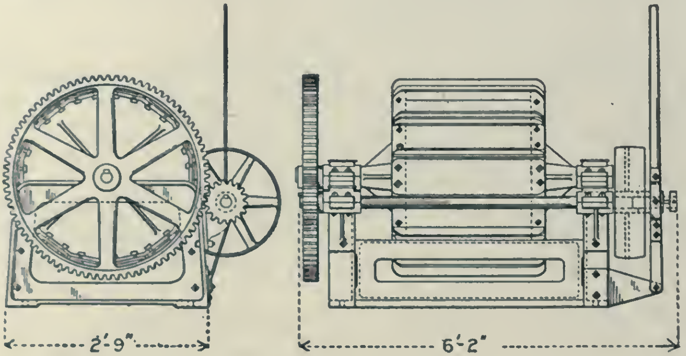

FIG. 9.

NEW TYPES OF IMPACT TESTING MACHINES FOR DETERMINING FRAGILITY OF METALS.

By T. Y. Olsen.

The impact test of steel, although recognized as one of the most important, and essential for determining the highest quality of a steel, is but little used commercially. No standards or specifications have been formulated, and a number of engineers have been experimenting along their own lines, with their own methods and design of machines, in developing tests of this character. The advent of the automobile in the past few years, and the consequent development of special and heat-treated steel, has greatly advanced the science of steel testing, and where a couple of tests would formerly have sufficed, we now have a large variety, with the tendency at present toward the most severe test, that of impact.

With reference to the method and type of specimen, impact testing may be divided into two general groups:

1. Breaking a nicked specimen over a die as a beam.
2. Breaking a nicked specimen over an anvil as a cantilever.

The first method was devised by Mr. Christian Fremont of France, to whom, more than any one man, is due the advancement of impact or shock testing. The specimen used is 8 by 10 mm. in section and 30 mm. long, having a nick 1 mm. wide by 1 mm. deep across the wider face. The die is 21 mm. long. This size of specimen and die is ideal, as the impact is confined to a very small surface. Valuable data can be obtained from the deformation of the broken specimen.

Various steels and treatments thereof have what may be termed a critical speed of impact. Within this speed limit, the work required to rupture a specimen will more or less vary, while beyond, it remains a constant, independent of any further increase in speed. With this in view Mr. Fremont made numerous experiments, and determined upon a height of 4 meters as the best drop for the hammer of his impact testing machine to develop a speed that would be sufficient to eliminate speed as a factor in the test.

The difficulty in any impact testing machine of this type is to measure the work required to cause rupture. This Mr. Fremont does by absorbing with springs the work left in the hammer after breaking the specimen, and measuring their compression. This method gives good results, but great care is required in the adjustment of the instrument which measures the motion of the springs.

Another impact testing machine of this type has been devised in which a pendulum weight used to apply impact is allowed to pass through the die after breaking the specimen and the work left in the pendulum recorded. This has the disadvantage of necessitating a wider die to allow the pendulum weight to pass through, and it makes a large and massive machine.

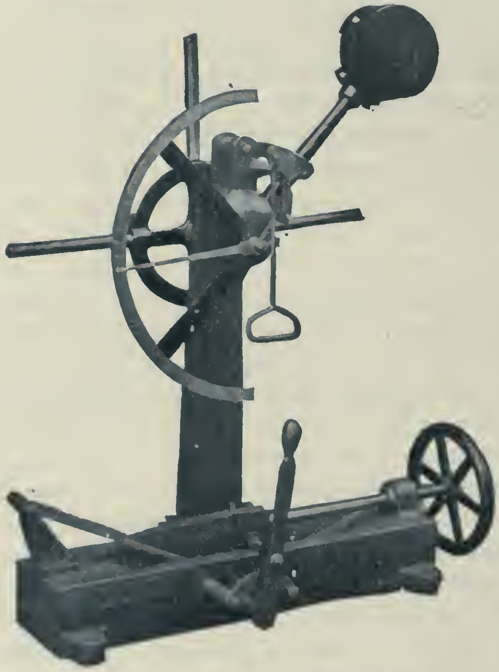

FIG. 1.—Impact Testing Machine.

The second method of impact testing, that of breaking the specimen over an anvil as a cantilever, is the one I wish to describe more fully, as well as a new machine of this type.

In this test, as in the Fremont, the impact is confined to a very small surface, and by using a pendulum to apply the impact, the work required to break a specimen can readily be accurately recorded.

A machine of this type can be designed for any desired height of fall and weight of hammer. The machine illustrated in Fig. 1 was designed with a hammer of 33⅓ lbs. weight and a fall of 3 ft., thus giving to the specimen an impact of 100 ft. lbs.

The speed of impact under these conditions is not such as to eliminate entirely the speed of impact as a factor, as previously described, but is quite sufficient to determine the fragility of one steel as compared with another, when tested under this speed. This is especially true when it is considered that the results of this or any other impact test are arbitrary, and the units of one cannot be compared with those of any other unless the same conditions are observed.

While a machine of this type could readily be designed to give considerably greater speed of impact, the advantage would be small compared with the ease and rapidity of operation, the compactness, and the small cost of the machine as designed, all of which are points to be considered in making an impact test available for general testing.

In this machine, the specimen is either 10 mm. square, 8 mm. by 10 mm., ⅜ in. square, or less, nicked on the widest face by a notch 1 mm. wide by 1 mm. deep. Referring to Fig. 1, the specimen is clamped against a tool-steel abutment by means of a jaw firmly tightened with a screw and hand-wheel. The hammer is held suspended by a latch which is released by pulling a handle. The hammer, after breaking the specimen, swings upward carrying the pointer with it. In this way the work required to break the specimen is indicated directly on the graduated scale.

The swaying of the hammer, after breaking the specimen, is stopped by means of a chain brake. The hammer is raised to the top and latched by turning the spider wheel at the back of the machine. The spider wheel is so designed, that when released after raising the hammer, it automatically moves back to its original position and is thus out of contact with the hammer on its downward stroke.

The breaking tool is in the center of the hammer, and is of such form and so inserted that it can readily be removed and replaced. The general construction and characteristics of the machine are so simple and apparent from the illustration that I do not think any further description is necessary. The machine has a length of 4 ft. 3 ins., a breadth of 1 ft. 4 ins., and a height of 4 ft., and is therefore of reasonable dimensions for easy handling and quick operation.

A NEW TYPE OF AUTOGRAPHIC TRANSVERSE TESTING MACHINE FOR RESEARCH TESTING OR REGULAR FOUNDRY PRACTICE.

BY T. Y. OLSEN.

The transverse test is most often used in general foundry practice to determine the physical characteristics of cast iron. The tension test is also largely used, but it requires a heavier and more expensive machine; and owing to the brittleness of cast iron, the specimen must be specially prepared and great care taken in gripping it in the machine if the best results are to be obtained.

The transverse specimen, however, requires no machining. It is usually either 1¼ ins. in diameter or 1 by 1 in. in section with 12-in. span, or 1 by 2 ins. in section with 24-in. span. The former size has been most generally adopted. The transverse breaking load is then approximately one-tenth of the ultimate tensile load of the same cast iron. The specimens are easily cast. The machines for testing them are small, compact, and inexpensive, so that they are well within the reach of any foundry interested in the quality of its daily output.

The foundry transverse testing machines in use at the present time give the breaking load very accurately, as well as automatically note the deflection of the specimen by means of a pointer moving in the arc of a circle over a graduated scale. The results thus obtained are very good so far as they go, but to-day there is a demand for a more complete machine, one that will automatically record the stress-strain diagram to such a scale that the deflection for any given load may be read to a thousandth of an inch. Any slight variation in the strength, stiffness, or other property of the test specimen, due to a difference in composition or treatment, can thus be quickly and conclusively determined.

From a practical view point, it is essential that an autographic transverse testing machine shall be adapted to the test bar used in general practice and that it shall be so arranged as to automatically trace the stress-strain diagram. It must also be simple in construction and of moderate cost. To cover these various points, a pendulum balance system of weighing was adopted as being the most sensitive and accurate automatic weighing device available.

Fig. 1 illustrates the general appearance of a completed machine of 5,000 lbs. capacity for testing a 1 by 1-in. specimen on a 12-in. span. A larger machine of 5,000 lbs. capacity for testing a 1 by 2-in. specimen on a 24-in. span is similar in all respects to the smaller machine.

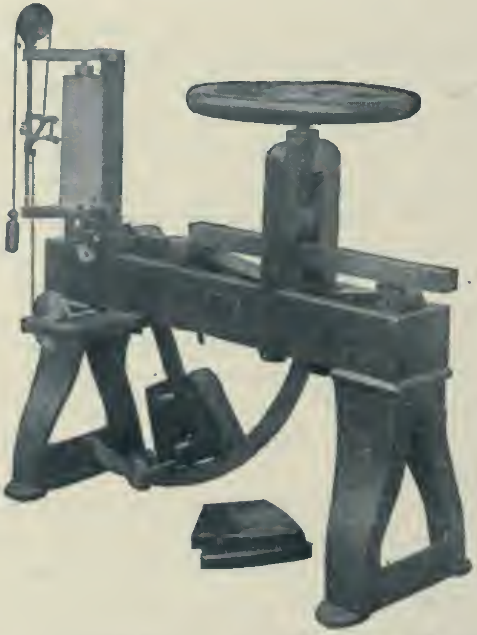

FIG 1.—Autographic Transverse Testing Machine.

The specimen is placed in the machine as shown and the load applied by the screw and hand wheel. The pressure on the specimen is transmitted through a lever system to a weighted pendulum and raises the pendulum, which automatically weighs the load on the graduated arc beneath the bed of the machine. The pendulum is so constructed that its weight can readily be changed by the addition of side pieces, so that the machine may be used with a capacity of either 2,500 or 5,000 lbs., depending upon the strength of the sample being tested.

The motion of the pendulum rotates the autographic drum, so that the load on the specimen is measured as the abscissa of the diagram.

The deflection is measured by the motion of a follow pin placed directly beneath the center of the specimen. This pin is connected to the recording pencil in such a manner that the motion of the pencil is vertical, and is multiplied twenty times; i. e., the ordinates of the curve measure the deflection.

The curve sheet is 5 ins. square, the abscissas reading to a maximum deflection of ¼ in., so that a deflection of 0.001 in. (0.02 in. on the diagram) may easily be approximated.

The pen is held against the recording drum by a very light torsion on the cord attached to the pen carriage. The curve sheet is held on the revolving drum by clips, and both drum and pencil may be so adjusted that the pencil will commence to trace the curve at the zero point of the curve sheet.

A series of pawls operate to catch the pendulum in the rack when the specimen breaks. The pendulum is released and returned to its initial position by turning a knob shown in the illustration.

The machine is easy to operate, and but little more time is required to make the autographic test than one determining only the maximum load and maximum deflection. The machine is also more accurate than a plain-lever machine, in that the load may be applied uniformly without stopping to weigh it, which, if done on a lever machine, requires two operators, whereas only one is required in the use of the autographic machine.

The stress-strain curve makes a record which can readily be filed away for future reference.

A NEW METHOD OF TESTING THE ENDURANCE OF CASE-HARDENED GEARS AND PINIONS.

BY J. S. MACGREGOR AND BRADLEY STOUGHTON.

There was presented to us the problem of testing the service durability of a case-hardened pinion and gear as compared with one having uniform composition and hardness throughout. In Figs. 1 and 2 is shown the general construction of the metal of the case-hardened material. The outside skin was carbonized in varying degrees (diminuendo), as indicated by the following analyses in carbon taken at different points, shown in Fig. 2:

A	1.009	per cent.
B	0.710	" "
C	0.408	" "
D	0.213	" "

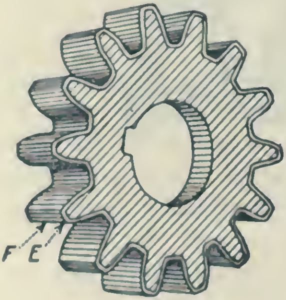

FIG. 1.

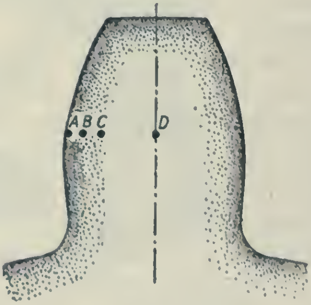

FIG. 2.

The other pinion had uniform analysis in carbon, about equivalent to the analysis at point *C* of the case-hardened pinion. Both pinions were heat-treated by special processes, but it is obvious that the surface hardness of the case-hardened pinion would be much greater and the hardness of the interior portion would be much less than that of the uncarbonized metal. Tests of hardness by the Brinell, Shore, and Turner methods confirmed this prediction.

It yet remained, however, to prove which material would give the greatest resistance to strains in service other than attrition. Pinions and gears subjected to the kind of service for which these were intended, if they ever fail by fracture, do so by the breaking off of a tooth at the base, and therefore tensile specimens as shown in Fig. 3 were cut as near the base of the teeth as possible At first, specimens were cut by means of emery wheels, etc., at the point marked *A* in Fig. 4. With the case-hardened pinion, however, this tensile test failed to disclose the true tensile properties of the metal because the hardened surface cracked off as soon as the steel began to stretch, leaving an undue strain on the interior. Therefore, tensile specimens were cut at the point marked *B* in Fig. 4, by which only the properties of the interior were tested. As might be expected, the higher-carbon, uniform-composition

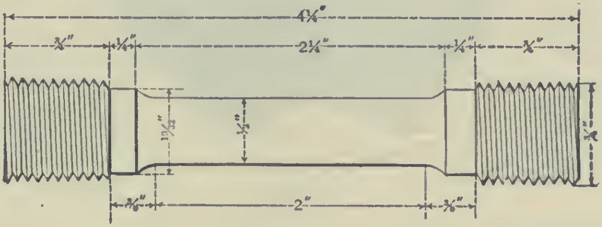

FIG. 3.

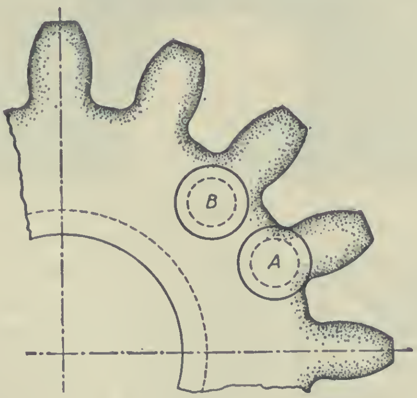

FIG. 4.

steel, showed less ductility and greater strength than the softer metal in the interior of the case-hardened specimens.

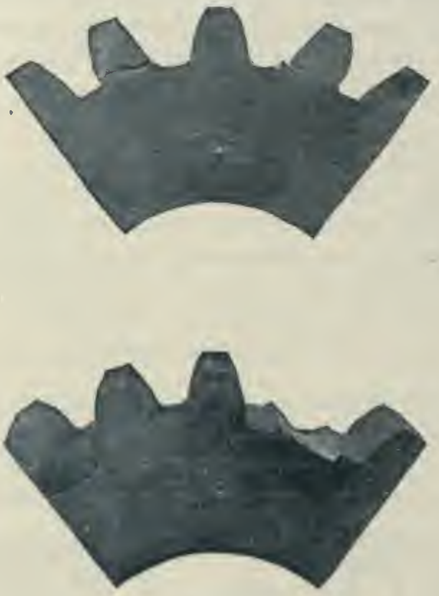

FIG. 5.

A static test was then made by cutting out sections of the pinions as shown in Fig. 5 and loading the teeth at the point of maximum strain in service by means of an ordinary compression-testing machine. The case-hardened pinions showed a high degree of toughness under this test, but not as great ultimate strength. The results of this test implied, however, that the metal would be stronger under an impact test than the harder steel of the other pinion.

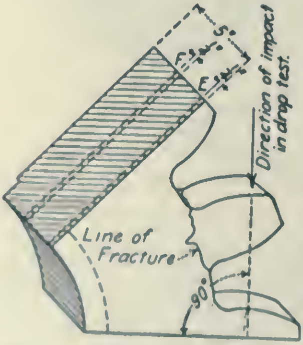

FIG. 6.

In an endeavor to so test the quality of each with respect to its resistance to shock, we determined to employ the Frémont testing machine, in which, as is well known to the members of this Society, the maximum resistance offered to a falling weight having sufficient force to break the specimen, is registered on a dial attached to the machine. The first specimens tested were in the form of sections $\frac{1}{4}$ in. thick, cut from the pinions and gear as shown at *F* in Fig. 6. The form of these sections is shown in Fig. 7. In order to test them in the Frémont machine, certain mechanical changes had to be made in the apparatus, but it was soon learned that specimens of this shape would not give a representative test, because it was impossible to deliver the blow with sufficient accuracy and directness to always break off the tooth at the base and prevent any sidewise or glancing impact. Specimens were therefore finally cut as shown in Fig. 8, and in the modified form of the Frémont machine, proved to be all that had been hoped for as a means of testing the endurance of the material against sudden blows and shocks. Since many gears and pinions are ordinarily subjected to shock strains of this character, the authors hope that some light may be thrown on the problem by the publication of their researches, and that those who have solved it in a different way may be persuaded to give the benefit of their experience for the sake of the practical importance of this subject in mechanical engineering.

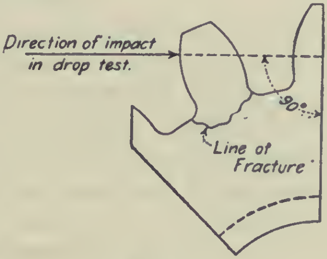

FIG. 7.

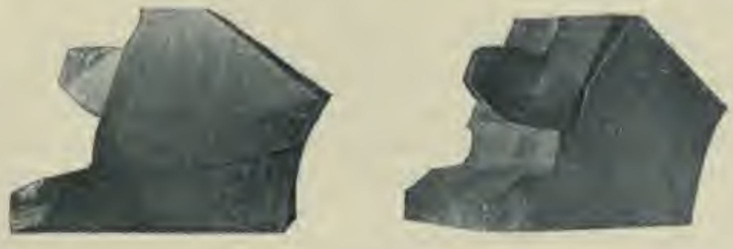

FIG. 8.

DISCUSSION.

Mr. Johnson. MR. C. W. JOHNSON.—This paper is very interesting indeed, but several of us have been waiting to see the comparative results between unhardened and case-hardened material. That is a matter that has been up many times within the last year or two.

Mr. Stoughton. MR. BRADLEY STOUGHTON.—We did not wish to be placed in a position of advertising one form of pinion or the other. Our purpose was rather to describe a method of testing these pinions. I will say, however, that the case-hardened pinion under impact gave greater strength than the pinion of uniform composition. I did not put the figures in this paper, but if they are of sufficient interest I can give them.

The President. THE PRESIDENT.—I think it will add very greatly to the interest of the paper if you will, Mr. Stoughton.

Mr. Stoughton. MR. STOUGHTON.—The registration by the machine in the case of the case-hardened pinion was 509.9, and in the other case 488.2. The first of these specimens was 0.124 in. wide; the other was 0.126 in. wide. Multiplying so as to get the equivalent over the whole width of the tooth gave in one case 20,560 ft. lbs., and in the other 19,370 ft. lbs. Of course it would be very desirable if we could have some form of machine strong enough to break the whole tooth. As you no doubt know, the ordinary method of testing case-hardened pinions now is to hit them with a sledge and see how many blows can be struck before they break. Sometimes a large tup is dropped on the corner of the tooth in order to see whether it breaks off or not. If we had a registering machine big enough to break off the whole tooth at once, I think it would be much better than to be confined to a small specimen; but we did the best we could by taking the small size and multiplying it. The figures given represent only one test on each tooth. The difference between the two tests might be within the limits of error of the machine. I wish to say that in this particular test the case-hardened tooth broke off somewhat slowly, and was somewhat ductile.

In the case of the other pinion the break was much more rapid. **Mr. Stoughton.** It was more brittle, and the whole section was thrown out of the machine by the violence of the shock.

MR. G. L. FOWLER.—I have been particularly interested **Mr. Fowler.** in this matter of broken gears and have done considerable work of investigation with regard to it. Many breakages have occurred along the lines shown in Fig. 1. For two or three years they were apparently inexplicable. Test pieces of metal taken from the point of the tooth down were apparently up to the requirements of the work, and in an inspection of the pinions no defect could be detected. In order to determine the cause the end of the tooth was carefully polished, and then by using a microscope an inkling of the trouble was obtained. There was a zone of bad metal all the way around, about on the line *AA*, Fig. 1. The cracks started at the root of the tooth and came down on the border line of the good and bad metal as shown in the photograph, Fig. 3. On one side the metal was full of blowholes, too small to be seen with the naked eye; while on the other side the material was perfectly sound. However, by a very careful inspection of spots where the bad metal did not seem to exist, I found a fine crack at the root of the tooth on some of the pinions that had been case-hardened. On subjecting this to a magnification of eighteen or twenty diameters, I still felt a little uncertain as to whether it was really a crack, and whether I could show anybody else where it was. I therefore took the specimen to the machine shop and worked it out myself by carefully

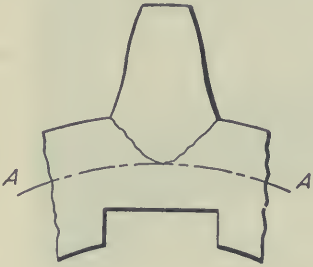

FIG. 1.

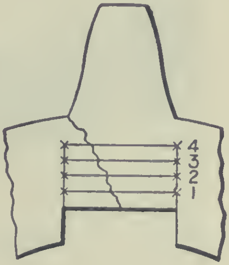

FIG. 2.

Mr. Fowler. cutting out the metal on each side of and beneath the crack. After I had worked it up to where the crack as I supposed actually existed, the metal dropped apart in my hands, and showed the markings of a most beautiful heat crack, evidently the result of bad heat treatment.

There is another thing about actual service that I believe would not show from the test of a pinion tooth in the laboratory. A pinion at the end of the armature shaft has never an equal bearing throughout the whole length of the tooth. The armature shaft will always spring away a little from the gear, and the axle of course will always spring a little due to the weight of the car on the outside and the pressure of the armature shaft against it; with the result that there is a much heavier bearing against the inside end of the pinion than against the outside end. If there is bad metal in there, or if there is a little crack, it will make itself felt until a fracture in detail occurs, and one can guard against such defects only by the most careful heat treatment in every detail.

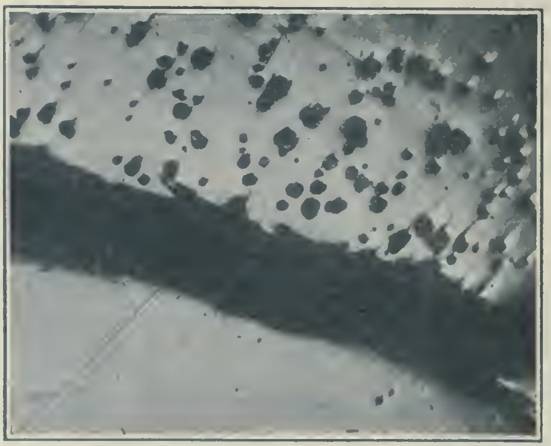

FIG. 3.

There was another interesting case of breakage of pinions. This was not in connection with teeth, yet it shows how careful we have to be in these matters. This fracture is shown

Mr. Fowler. in Fig. 2. The failure occurred through the center of the key-way, never from the corner where there was a very small fillet of about $\frac{1}{16}$-in. radius. The breakages always occur through there, and of course I thought them due to bad metal. But when the metal was examined, which metal, if I understand rightly, was very similar to that which Mr. Stoughton has spoken of, there was nothing to find fault with. An examination into the method of heat treatment found it to be beyond criticism. Then I asked the question, "How did you put those pinions on the shaft?" The answer was, "We slipped them on the shaft, gave them three or four blows with a sledge, tightened up the nut and let them go." I took the pinion and polished the whole surface of the tooth down to the key-way, as shown in Fig. 2, drew some lines as shown and measured them carefully, and then handed the pinion to the man and told him to drive it into position. He hit it six or seven blows with a 12-lb. sledge. Under ordinary conditions the working stress on this metal with the highest rate of acceleration was 19,000 lbs. per sq. in. When he had driven the pinion on I measured the parallel lines, noted the increase in length, and computed the unit stress on the metal. The stress had jumped to about 56,000 lbs. per sq. in.; its elastic limit was 70,000 lbs. per sq. in. The result was that the pinion could not help breaking when put into service. The question was, "What are you going to do about it?" We are not allowed to heat these pinions in a furnace and shrink them on, so we decided to boil them. The pinion was boiled in hot water a a couple of hours, so as to insure uniform heating and then put on and tightened up. We found the stress along these three or four lines to be almost exactly uniform, running about 29,000 to 31,000 lbs. per sq. in. That, with 19,000 lbs. per sq. in. added for acceleration, gives an ample margin of safety. We proceeded to put the pinions on in that way, and had no further trouble; but after I had one shrunk on and measured, I handed it back to our brutalizer, and told him to drive it up to the old place. He did so, and I then found a stress at line (1) of about 52,000 lbs. per sq. in. On line (2) I found a stress of about 48,000 lbs. per sq. in., while on line (4) the stress was about down to the original stress, showing that the brutal effect of driving the pinion on had

Mr. Fowler. stretched the metal immediately around the key-way, and had left the metal on the outside apparently unstretched. By treating the piece of steel as it should be treated, we had no further trouble about breaking.

Mr. Stoughton. MR. STOUGHTON.—I just want to say that those were forged pinions, and that fact no doubt accounts for our not finding any blowholes in them at that point. That might be of interest.

Mr. Fowler. MR. FOWLER.—I wish to say that ours were too.

A number of years ago I was investigating the qualities of different types of steel tires on the market, and I wanted to get some information especially relating to resistance to abrasion. I went to the Carborundum Company, told them what I wanted to do, and they recommended a wheel for the purpose. I had obtained samples of all the tires and all the steel wheels made in this country. The samples used were cylindrical, ½ in. in diameter, taken from the tread down into and through the wheel or tire. I started to grind them with a wheel which was turning at the rate of about 1,200 r. p. m. When I commenced grinding I took note of the number of revolutions required to grind off each ⅛ in. from the tread of the wheel down into the metal. After starting, the number of revolutions required to grind off this ⅛ in. began to increase, and when I got ½ in. down into the section the number of revolutions reached its highest and then began to drop. When I commenced, I had three specimens, taking off ⅛ in. of each in rotation. When I had taken off about ½ in. I put on the fourth specimen, putting it in rotation with the other three. The number of revolutions for the fourth specimen started to increase while the others were dropping. Others were put in afterward and they all increased, giving a curve of the form shown in Fig. 4. This showed that the resistance to abrasion in each case was the greatest at ½ in. below the surface of the wheel, and the method of grinding did away with all suspicion of wheel glazing.

In reference to gears, we had a great deal of trouble a few years ago with some gears wearing out very rapidly. We charged it all to the quality of the metal. Of course, that is usually the first thing done,—to put the blame on the manufacturer. But the metal when tested was found to be all right. **Mr. Fowler.** Then the next thing was to inquire as to the conditions under which the gears were working. A number of years ago Mr. Forney, in speaking before the Master Mechanics' Association, made the statement that if they would allow him to take any

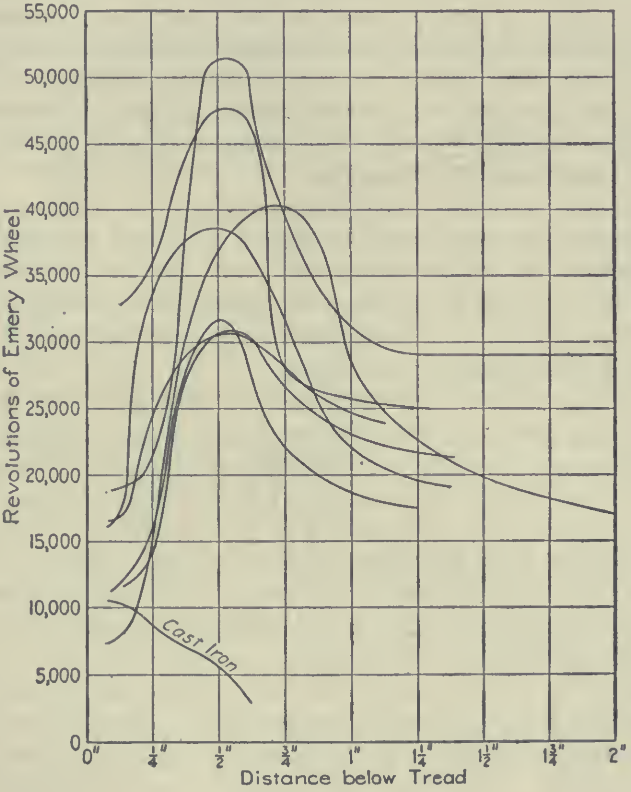

FIG. 4.

locomotive and paint the smokestack sky blue and then let him conduct a test he would show a saving of 10 per cent. in fuel as against any other locomotive. So, I say, if you will give me the gears and let me conduct a test on them I can show that any gear selected will give the best service.

In the case referred to we had four different kinds of metal

Mr. Fowler. in our gears and pinions. I took a sample of each gear and had their surfaces put into identical condition. I then submerged them in water and gave them a constant rate of speed and load and started to rub the several combinations of them together. The result was that the wearing of those gears by the different kinds of metal coming together checked very closely with the wear of the gears that we had in service. I found that when two kinds of metal were put together they would wear very rapidly; but if those same two metals were rubbed against something else the wear would be entirely different. So we shifted our gears around so as not to put those together in which the metal would wear most rapidly.

Another point we discovered was the importance of proper lubrication. Ordinary lubricating oil is of no use whatever; you have to use an oil that is tacky and that will stick, so that when two gears come together the grease is not pushed off and you do not have two bare surfaces coming together, but two surfaces with a film of oil between.

During the past year I have been making some investigations with regard to the abrasion of the surface of brake shoes in actual service on the car. We found that a brake shoe with hard chilled inserts wears out much more rapidly than an ordinary cast-iron shoe, and that a shoe with a steel filler also wears very rapidly. Taking these abrasion tests into consideration, it seems to me therefore that the only way you can get satisfactory results is to make the proper combination of metals; if there is trouble, change the combination. I feel after having gone through this work as though I could take my data and by careful selection of it prove almost anything. I am a good deal like the boy who was put on a horse to ride and whose father was a dealer. He was a little uncertain as to the status of affairs and so leaned over to his father and said: "Dad, do you want me to ride this horse to buy or sell?" That is the way I feel with regard to many abrasion tests.

STANDARD TESTS FOR DRAIN TILE AND SEWER PIPE.

By A. Marston.

Drain tile and sewer pipe, to the value of over twenty million dollars, are produced annually in the United States alone, and are made a vital part of construction work whose cost often totals several times that of the pipe itself. Yet we have no standard tests for these important materials, the determination of their quality usually being left to the varying notions of individual engineers, or even to the whims of ignorant or venial political inspectors. Manifestly there is great need for the immediate adoption of standard tests, which shall develop with certainty the true quality of sewer pipe and drain tile.

Tests of Drain Tile and Sewer Pipe.—Standard tests of drain tile and sewer pipe must develop two cardinal qualities:

(*a*). The bearing strength of the pipe under approximate ditch conditions, for which I shall propose:

1. A direct bearing-strength test.

(*b*). The quality of the material in the shell of the pipe to resist disintegration, which I propose to determine by:

2. The modulus of rupture of the material, calculated from the data of the bearing-strength test, to show the strength of the material;
3. An absorption test of the material, to show its resistance to the penetration and action of destructive agencies.

Failures of Drain Tile and Sewer Pipe.—The need of standard tests is shown by the many instances in which large pipes are found to be cracked when inspected in completed sewers and drains. Reputable engineers, after careful study,* have stated deliberately that present practice in sewer construction "will produce broken pipe in all the larger sizes," and that "it is unquestionably a fact that if careful investigation were made of pipe sewers of the larger sizes, an enormous amount would be found to be cracked."

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* See *Municipal Engineering*, Vol. 30, p. 288, and Vol. 34, p. 294, for articles by A. Potter and J. N. Hazlehurst, respectively.

Recent investigations by the writer* have developed many cases of cracked pipe, both in sewers and in the large tile drains which are now so generally replacing the objectionable open drainage ditch. In several instances in Iowa these large drain tile have cracked during construction, under the weights of comparatively shallow depths of ditch filling, and inspections of completed drains show numerous unsuspected cracked tile.

Theory of the Pressure of Ditch Filling on Drain Tile and Sewer Pipe.—In order to determine by tests whether or not drain tile and sewer pipe will be strong enough to prevent cracking when used in the ditch, it is necessary, first, to develop a correct theory of the pressure of the ditch filling on the pipe. For several years the writer, in directing the tests of the Iowa Engineering Experiment Station, has been giving careful study to the subject, and a theory has resulted, which is still under experimental study by the Station.† A brief preliminary statement is all that we feel justified in giving out at this time.

The prism of ditch filling in a unit length of ditch, above any level, is partly supported by friction against the sides of the ditch, as the material settles, and partly by vertical upward pressure from below. Since there is always some actual movement of the ditch-filling material in settling, relative to the sides of the ditch, no factor corresponding to the cohesion recently discussed by Professor Cain (Trans. Am. Soc. C. E., Vol. 72, 1911) need be considered in calculating the maximum weight, though such a factor undoubtedly reduces the permanent load on the pipe. The mathematical discussion for calculating the average vertical pressure is the same as already published by Janssen for calculating the pressures in grain bins. See, for example, Ketchum's "Retaining-walls, Bins, and Grain Elevators," pages 237–239, for full details.

The intensity of the vertical pressure is less at the sides than at the center of the ditch, and the earth fill at the sides of the pipe is much more compressible than the rigid unyielding pipe. Taking into account imperfect workmanship, the pipe may be called upon to carry practically the entire weight of ditch filling except that supported by friction against the sides of the ditch.

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* See a forthcoming bulletin of the Iowa Engineering Experiment Station, Ames, Iowa, on "Tests of Cement and Clay Drain Tile and Sewer Pipe."

† Prof. A. N. Talbot has given valuable assistance in correspondence on the subject.

Let W = total weight on pipe per unit length;

V = average intensity of vertical pressure in ditch at level of top of pipe, per unit area;

w = weight of ditch filling per unit volume;

B = breadth of ditch at level of top of pipe;

H = height of ditch filling above top of pipe;

R = mean hydraulic radius = area ÷ perimeter;

K = ratio of lateral to vertical earth pressures.

NOTE.—K may be computed approximately by Rankine's formula,

$$K=\frac{1-\sin\theta}{1+\sin\theta},$$

where θ = the angle of internal friction, whose tangent is the coefficient of internal friction.

u' = coefficient of friction of ditch filling against sides of ditch;

e = base of Naperian logarithms;

C = a coefficient of pressure = average vertical pressure in ditch per unit area, in a ditch of unit width, under ditch filling of unit weight per unit volume.

Janssen's formula is as follows:

$$V=\frac{wR}{Ku'}\left(1-e^{-\frac{Ku'H}{R}}\right).$$

For a ditch, $R=\frac{B}{2}$, and $W=BV$. Hence,

$$V=CwB \ldots\ldots\ldots\ldots (1)$$

$$W=CwB^2 \ldots\ldots\ldots\ldots (2)$$

$$C=\frac{1-e^{-2Ku'\frac{H}{B}}}{2Ku'}.$$

Equations (2) and (3) are the ones to be used in calculating the weights of ditch filling which must be carried by drain tile and sewer pipe.

Under the direction of the writer, a series of measurements has been made by the Iowa Engineering Experiment Station of the values of *w*, of *u'*, and of the coefficients of internal friction, for various common ditch-filling materials. We are about to make final check of the theory by a series of actual weighings of pipe in ditches, while carrying filling under service conditions. We do not wish to give out any numerical results until this final test is completed, but our results will be published in an immediately forthcoming bulletin of the Station.

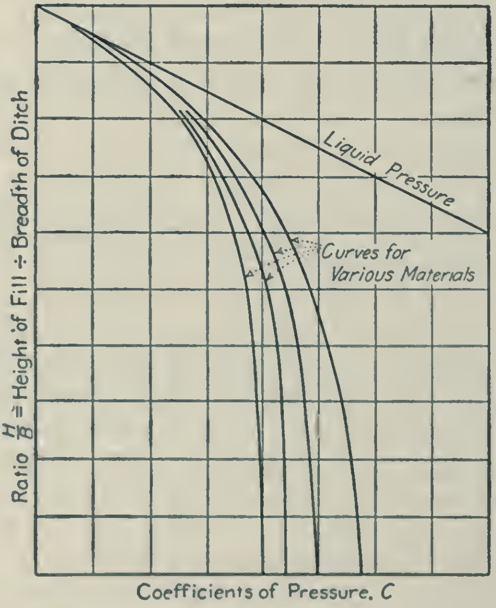

Fig. 1.—Diagram of Approximate Trial Values of "*C*" = Pressure per Unit Area in Ditch of Unit Width due to Ditch Filling weighing Unity per Unit Volume.

In the meantime the general nature of the law of the relation of the weight of ditch filling carried by pipe to the height of the filling can be seen from Fig. 1, which shows, with the numerical scales omitted, some of our values of *C* for different materials. It will be seen that the weight on drain tile and sewer pipe increases very little after such depth is reached that the height of fill is ten times the breadth of the ditch.

Iowa Standard Tests of Drain Tile and Sewer Pipe.—As a result of our study of numerous failures of drain tile and sewer pipe, together with six years experience in testing pipe in the Iowa Engineering Experiment Station, we have devised the specifications for standard tests given below.

These specifications have been adopted as standard for Iowa by The Iowa Cement Users' Association, The Iowa Brick and Tile Manufacturers' Association, The Iowa Drainage Association, and The Iowa Engineering Society.

IOWA STANDARD SPECIFICATIONS FOR DRAIN TILE AND SEWER PIPE.

ABSORPTION TESTS.

1. *Specimens.*—The specimens shall be each approximately three inches square, and shall extend the full thickness of the pipe wall, with the outer skins unbroken.

2. *Number of Test Specimens.*—Five individual tests shall constitute a standard test, the average of the five and the result for each specimen being given in the report of the test.

3. *Drying Specimens.*—Each specimen shall be dried in an oven, or by other application of artificial heat, until it ceases to lose further appreciable amounts of moisture when repeatedly weighed.

4. *Brushing Specimens.*—All surfaces of the specimens shall be brushed with a stiff brush before weighing the first time.

5. *Weighing.*—The specimens shall be weighed immediately before immersion, on a balance or scales capable of accurately indicating the weight within one-tenth of one per cent.

6. *Water for Standard Test.*—The water employed in the standard absorption test shall be pure soft water, at the air temperature of a room which is artificially heated in cold seasons of the year.

7. *Immersion of Specimens.*—The specimens shall be completely immersed in water for a period of 24 hours.

8. *Re-Weighing.*—Immediately upon being removed from the water, the specimens shall be dried by pressing against them a soft cloth or a piece of blotting paper. There shall be no rubbing or brushing of the specimen. The re-weighing shall be done with a balance or scales capable of accurately indicating the weight within one-tenth of one per cent.

9. *Calculation of Result.*—The result of each absorption test shall be calculated by taking the difference between the initial dry weight and final weight, and dividing the remainder by the initial dry weight.

BEARING STRENGTH.

1. *Specimens.*—The specimens shall be unbroken, full-sized samples of the pipe to be tested. They shall be carefully selected so as to represent fairly the quality of the pipe.

2. *Number of Specimens.*—Five individual tests shall constitute a standard test, the average of the five and the result for each specimen being given in the report of the test.

3. *Drying.*—The specimens shall be dried by keeping them in a warm, dry room for a period of at least 2 days prior to the test.

4. *Weighing.*—Each dried specimen shall be weighed on reliable scales just prior to the test.

5. *Bedding of Specimen for Test.*—Each specimen shall be accurately marked, with pencil or crayon lines, in quarters, prior to the test. Specimens shall be carefully bedded above and below in sand for the one-fourth circumference of the pipe, measured on the middle line of the tile wall. The depth of bedding above and below the tile at the thinnest point shall be equal to one-fourth the diameter of the pipe, measured between the middle lines of the tile walls.

6. *Top Bearing.*—The top bearing frame shall not be allowed to come in contact with the tile or with the test load. The upper surface of the sand in the top bearing shall be carefully struck level with a straight edge, and shall be carefully covered with a heavy rigid top bearing, with lower surface a true plane made of heavy timber or other rigid material capable of uniformly distributing the test load without any appreciable bending. The test load shall be applied at the exact center of this top bearing in such a way, either by the use of a spherical bearing or by the use of two rollers at right angles, as to leave the bearing free to move in both directions. In case the test is made without the use of a machine and by piling on weight, the weight may be piled directly on a platform resting on the top bearing, provided, however, that the weight does not touch the top frame holding the sand, and

provided, further, that the weight is piled in such a way as to insure uniform distribution of the load over the top surface of the sand.

7. *Frames for Top and Bottom Bearings.*—The frames for the top and bottom bearings shall be composed of timbers so heavy as to avoid any appreciable bending by the side pressure of the sand. The frames shall be dressed on their interior surfaces. No frame shall come in contact with the tile during the test. A strip of soft cloth may be attached to the inside of the upper frame on each side along the lower edge, to prevent the escape of sand between the frame and tile.

8. *Sand in Bearings.*—The sand used for bedding the tile at the top and bottom shall be washed sand which has passed a No. 8 screen. It shall be dried by keeping it spread out thin in a warm dry room.

9. *Application of Load.*—The test load shall be applied gradually, and without shock or disturbance of the tile. The application of the load shall be carried on continuously, and the tile shall not be allowed to stand any considerable length of time under a load smaller than the breaking load.

10. *Calculation of the Bearing Load.*—The total breaking load shall be taken as equal to the total top load, including the weight of top frame, sand for top bearing, top bearing timbers, etc., plus five-eighths of the weight of the pipe. This total load shall be divided by the length of the pipe in feet so as to give the bearing strength per linear foot of pipe.

COMPUTING THE MODULUS OF RUPTURE.

The modulus of rupture for drain tile and sewer pipe shall be computed from the results of the standard test for bearing strength, according to the following rule:

Divide the bearing strength per linear foot by twelve, multiply the quotient by the radius of the middle line of the tile wall expressed in inches, and divide this product by the square of the minimum thickness of the tile wall at top or bottom, also expressed in inches. This quotient will be the modulus of rupture of the pipe, expressed in pounds per square inch.

The formulas on which the above specifications are based are derived, in the usual manner, for flexible rings subjected to uniform vertical loads, over 90° of the circumference above, and the same distance below. The mathematical details of the derivation will not be given. Use is made in work of both the static and the elastic equations of equilibrium.

Discussion of Iowa Standard Bearing-Strength Test.—The Iowa Standard Bearing-Strength Test, given above, was developed to reproduce so far as practicable in a standard laboratory test the actual conditions under which pipe generally fail in actual use in the ditch.

We have compared the bearing strengths developed in our laboratory with the actual weights of filling under which the same or similar pipe have cracked in the ditches, and have found the results to correspond closely. In many of these cases the height of fill was small in proportion to the breadth of the ditch, so that the uncertainty in the field loadings was small.

The ordinary field conditions under which drain tile and sewer pipe crack from overloads of ditch filling are about as follows:

The contractor rounds off a bearing for the pipe over only a portion of its under side. The ditch filling cannot be compacted on the remainder of the under side of the pipe, and often is not well tamped at the sides. Even when compacted at the sides, it is still so compressible, as compared with the rigidity of sewer pipe, that the pipe cracks before developing much side resistance.* As regards the top pressure, nearly all the weight is carried by the pipe, and mostly on the top 90°, because, first, the friction against the sides of the ditch reduces the side pressure, and, second, the comparatively level top 90° of the pipe furnishes a rigid support as compared with the sloping sides and the compressible side filling.

Hence the specified sand bearing of 90° at top and bottom comes near enough to actual ditch conditions, and at the same time constitutes a simple and inexpensive method of testing.

Results of Iowa Standard Tests.—In Tables I and II we present a very condensed statement to give some idea of the results of about 700 Iowa standard tests of drain tile and sewer pipe. Additional tests of large size sewer pipe are still under way. All these tests will be published in detail in our forthcoming bulletin.

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* At various points in Iowa extra care in compacting the filling has failed to prevent cracking which developed during construction.

TABLE I.—BRIEF SUMMARY OF BEARING-STRENGTH TESTS.—IOWA STANDARD METHOD.

(*a*) TESTS OF CEMENT TILE.*

No. of Tests.	No. of Kinds.	Diameter of Tile, ins.	Bearing Strength, lbs. per lin. ft.			Modulus of Rupture, lbs. per sq. in.		
			Minimum.	Maximum.	Average.	Minimum.	Maximum.	Average.
27	4	4	590 — 980	1,180 — 1,710	790 — 1,280	450 — 870	570 — 2,300	520 — 1,250
33	8	5	550 — 1,740	1,320 — 2,260	990 — 1,920	420 — 1,310	860 — 2,040	600 — 1,560
72	10	6	520 — 1,230	890 — 1,790	810 — 1,390	390 — 970	640 — 1,400	600 — 1,060
24	5	7	790 — 1,770	1,180 — 2,070	1,010 — 1,900	580 — 1,340	810 — 1,960	690 — 1,650
50	10	8	840 — 1,650	900 — 2,330	880 — 1,880	380 — 840	580 — 1,290	460 — 1,050
42	8	10	510 — 1,450	1,240 — 2,030	900 — 1,710	320 — 590	710 — 1,130	560 — 1,000
106	22	12	460 — 2,840	760 — 3,250	650 — 3,130	210 — 850	280 — 2,020	250 — 960
23	6	14	680 — 1,400	940 — 2,510	850 — 1,850	320 — 490	340 — 950	330 — 750
20	7	15	960 — 1,530	1,360 — 2,320	1,060 — 2,060	230 — 570	350 — 730	270 — 640
10	3	16	1,000 — 1,180	1,110 — 1,420	1,060 — 1,280	260 — 500	290 — 710	280 — 620
23	10	18	600 — 3,520	1,350 — 3,850	600 — 3,640	170 — 700	210 — 1,010	170 — 900
31	14	20	890 — 3,340	910 — 3,720	900 — 3,550	120 — 840	210 — 900	170 — 860
4	2	22	1,620 — 2,200	1,680 — 2,880	1,650 — 2,540	400 — 470	420 — 500	410 — 480
36	7	24	600 — 1,820	1,620 — 2,240	1,170 — 2,240	190 — 580	480 — 580	390 — 580
2	1	26	1,360	1,800	1,580	330	430	380
1	1	28	.....	.....	1,570	...	...	550
1	1	30	.....	.....	1,460	...	...	290
7	4	32	1,600 — 2,250	1,650 — 2,700	1,630 — 2,700	...	...	...
1	1	34	.....	.....	1,460	...	...	290
7	3	36	2,230 — 2,740	2,520 — 3,000	2,230 — 2,900	350 — 470	430 — 520	390 — 500

(*b*) TESTS OF CLAY TILE.

7	2	5	640 — 1,340	680 — 1,700	650 — 1,530	400 — 870	430 — 1,110	410 — 1,000
39	5	6	590 — 2,200	1,090 — 2,910	780 — 2,530	420 — 1,400	820 — 2,360	570 — 1,830
3	1	7	810	1,230	960	630	1,080	800
17	5	8	760 — 1,300	1,060 — 2,220	890 — 2,220	570 — 1,900	780 — 1,900	670 — 1,900
6	1	10	1,690	2,150	1,980	1,060	1,440	1,320
53	4	12	870 — 2,080	1,770 — 4,140	1,470 — 2,800	450 — 1,390	1,050 — 3,020	790 — 1,940
5	1	18	1,540	2,010	1,740	650	1,000	830
5	1	24	1,880	2,400	1,990	840	1,420	1,090
4	1	28	1,860	4,170	3,340	580	1,360	1,090
5	1	30	2,740	4,630	3,420	1,000	1,410	1,300

(*c*) TESTS OF CLAY SEWER PIPE.

3	1	6	1,430	2,010	1,770	930	1,460	1,150
8	2	8	1,570 — 1,950	2,060 — 3,320	1,820 — 2,690	1,020 — 1,240	1,170 — 2,320	1,100 — 1,730
4	1	10	1,890	2,840	2,320	1,100	1,660	1,350
3	1	12	1,690	2,500	2,150	930	1,350	1,180
2	1	15	1,630	2,000	1,820	980	1,200	1,090
4	1	24	2,170	5,110	3,520	1,280	2,760	2,080

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* These tile were of different proportions, made by different processes, and were of different ages, from 1 month to 31 years.

TABLE II.—BRIEF SUMMARY OF ABSORPTION TESTS OF DRAIN TILE AND SEWER PIPE—IOWA STANDARD METHOD.

No. of Tests.	No. of Kinds.	Diameter of Tile, ins.	Per Cent. of Absorption.		
			Minimum.	Maximum.	Average.
(a) TESTS OF CEMENT DRAIN TILE.					
1	1	4	....	....	5.9
7	2	5	6.8 — 9.1	7.4 — 11.3	7.1 — 10.3
38	2	6	4.1 — 4.6	7.4 — 11.6	6.0 — 7.5
1	1	7	....	....	8.8
5	1	8	8.9	11.3	10.6
5	2	10	7.0 — 9.5	7.9 — 9.7	7.4 — 9.6
46	5	12	6.2 — 8.8	10.2 — 13.5	9.1 — 10.7
4	2	14	7.9 — 11.2	7.9 — 13.8	7.9 — 12.8
5	2	18	7.8 — 10.3	9.5 — 10.3	8.5 — 10.3
5	2	20	7.2 — 8.9	8.1 — 9.6	7.7 — 9.3
2	1	22	6.0	6.3	6.1
2	1	26	7.1	8.2	7.6
1	1	28	....	....	8.4
1	1	30	....	....	5.9
1	1	34	....	....	7.7
1	1	36	....	....	9.2
(b) TESTS OF CLAY DRAIN TILE.					
4	1	5	15.0	18.5	16.3
30	3	6	2.6 — 4.0	7.3 — 8.2	4.8 — 6.1
2	2	8	....	....	3.6 — 19.0
42	2	12	2.4 — 3.9	7.6	5.0 — 5.8
5	1	18	4.1	8.8	5.5
4	1	28	4.8	6.1	5.4
5	1	30	3.4	5.2	4.3

Examination of Table I may indicate a sufficient amount of data as to bearing strength of the tile used in Iowa to permit, when the complete data are studied carefully, the adoption of tentative test requirements in tile specifications. There are not yet sufficient data to warrant even tentative requirements as to absorption and modulus of rupture.

Moduli of Rupture.—The calculations of the moduli of rupture gave such unexpectedly high results, as briefly summarized in Table I, that we made a special series of tests of concrete and clay tile beams, cut from the broken pieces of the tile tested. The results agree fairly well with those given in Table I. Of course the transverse modulus of rupture is well known to be 1.75 to 2 times as great for concrete beams as the real tensile strength of the material. Even with this allowance some of the values in Table I and in our corresponding beam tests seem so high as to warrant further investigations of the cause. We have had one such investigation made as thesis work, but without conclusive results.

Other Proposed Standard Tests.—A number of other methods of making bearing-strength tests of drain tile and sewer pipe have been advocated or used. Together with the above method, they may be enumerated as follows:

1. *Completely surrounding the pipe with sand in a very strong box.* The author understands that this method was in use for a time in Brooklyn. It has been strongly advocated by Blackmer and Post of St. Louis, in recent correspondence, and they have made a new design for the enclosing box and the bearings.

2. *Bedding the pipe on sand at the bottom, while applying the top load to a narrow bearing strip.* The author would designate this the *Brooklyn Method*, as it is in regular use by the sewerage engineers of that city.

3. *The Iowa Standard Method*, already described herein, beds the pipe in sand for 90° at the top, and the same amount at the bottom.

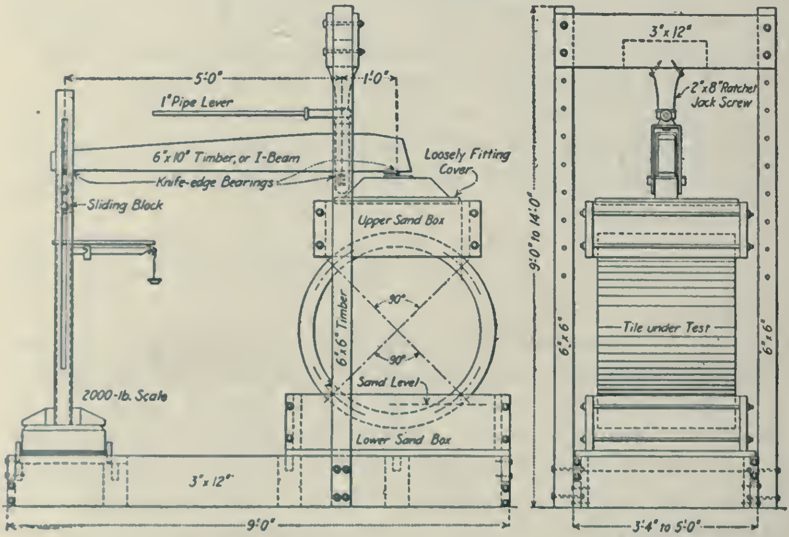
4. In the so-called *Three Point Method*, the pressure is applied along a single narrow bearing strip at the top, while the pipe is supported at the bottom on two similar bearing strips placed parallel, a few inches apart. This method has been developed by Mr. C. W. Boynton, of Chicago, and in the laboratory of Professor A. N. Talbot, of the University of Illinois. The Iowa Engineering Experiment Station has made quite a number of comparative tests with this method.

5. In the method of *Concentrated Loadings*, the top load would be applied to a narrow bearing strip, as in Methods 2 and 4, and the bottom of the pipe would be supported on a similar bearing strip.

All five of the methods outlined above, and any others which may be suggested, should be referred to the Society's Committees on Sewer Pipe and Drain Tile. These Committees should enlist the active cooperation of several prominent laboratories and of the manufacturers of material, and institute an exhaustive series of comparative tests prior to adopting a single standard test. Then an extensive series of standard test data should be accumulated to establish satisfactory strength specifications for general adoption.

The Testing Machines.—If the adoption of standard tests of drain tile and sewer pipe is to result in the maximum benefit it must be possible to conduct the tests readily, accurately and inexpensively in every city whether large or small, in every country where public drainage work is in progress, and in every manufactory of drain tile or sewer pipe.

For this an inexpensive testing machine is absolutely necessary. In Fig. 2 we present an outline of a home-made testing machine



Side View End View

FIG. 2.—Ames Senior Pipe Testing Machine,
Engineering Experiment Station, Iowa State College.

of 10,000-lbs. capacity, capable of testing the largest pipe, and which can be built anywhere for less than twenty-five dollars, in addition to the cost of a platform scale. The Iowa Engineering Experiment Station makes many of its tests on the machine shown, and will send detailed blue-print plans to any one who will build the machine.

DISCUSSION.

Mr. Schuyler.

Mr. Mont Schuyler.—A method for determining the direction and intensity of forces, other than vertical, which may be exerted upon a buried pipe, may be of interest.

The pipe section in Fig. 1(*b*) is to be surrounded with a number of "elements," one of which is shown. This element is

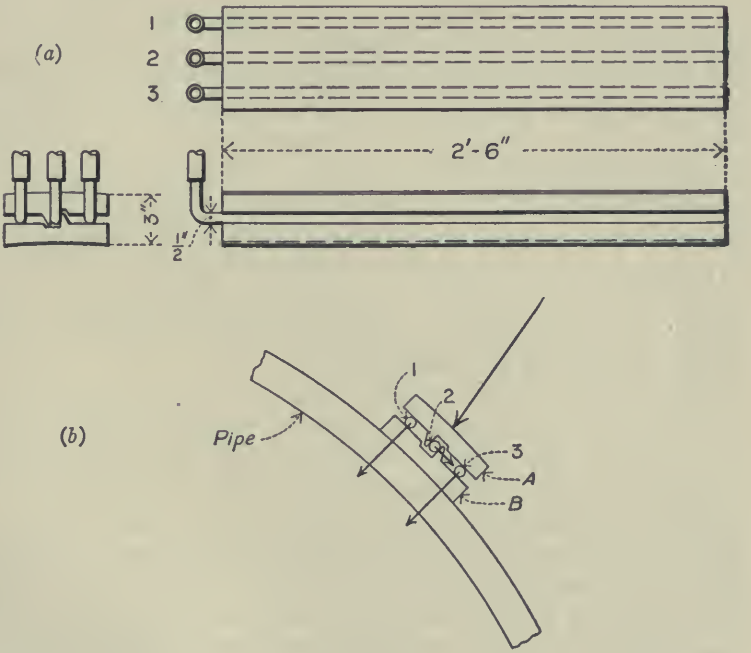


Fig. 1.

composed of two essentially similar blocks, *A* and *B*, made of some rigid material. Between these two blocks are inserted three rubber tubes filled with a fluid. If the pressures are high, the tubes may be stiffened by wrapping with wire. Each tube is connected with a manometer of small bore. Pressure exerted upon block *A* will be transmitted to block *B* through

Mr. Schuyler. the three tubes, distorting or flattening the tubes and forcing a portion of the contained liquid into the manometer. As may be seen from the figure, tubes (1) and (3) will transmit only that component of the pressure on *A* which is parallel to the radius of the pipe, while tube (2) will transmit only the tangential component. Assuming that the three tubes calibrate similarly, and that the force arrows drawn through their centers are some function of their several manometer readings, we can readily compute the original or causative force due to the earth pressure, both as to its intensity and direction.

The same scheme should also be applicable to the solution of the problem of the retaining wall.

Mr. Meriwether. MR. COLEMAN MERIWETHER.—I have just made a number of tests with an apparatus very similar to that Mr. Marston described, using a saddle of 90° around the top of the pipe and with the bottom imbedded in sand. In testing large pipe the wide saddle required a great load to break the pipe, and in one instance when testing a 60-in. pipe, the bottom flange of the 15-in. I-beam lever was sheared. I think it would be much better to use a knife edge on the top of the pipe instead of the wide saddle, thereby reducing the load required to break the pipe. I offer this merely as a suggestion.

Mr. Talbot. MR. A. N. TALBOT.—I have been much interested in Mr. Marston's tests of drain tile and also in his work in determining the pressures which may come upon drain tile in the ditch. It seems to me that the investigation of pressures will be of great value in providing information along lines on which little is known. As to the method of testing pipe, I may say that fifteen years or more ago I made a number of tests of sewer pipe for the Department of Public Works of the City of Chicago. Various sizes of pipe were used. A cushion of sand was used both above and below, the cushion extending over perhaps one-fourth of the circumference of the pipe. This seemed quite satisfactory, and I felt that there was an advantage in having a sand cushion. There was some trouble in obtaining a properly shaped cushion for the different sizes of pipes. I believe Mr. Marston has overcome that difficulty very well in the machine which he has designed. In the series of tests of cast-iron pipe and reinforced-concrete pipe made a few years ago at the University of Illinois,

Mr. Talbot. the pipes were imbedded in sand on all sides, with some lateral restraint, but the purpose of these tests was not only to determine the strength of the pipe but to find the action of the pipe under such conditions. The tests gave the information sought, and the method used was satisfactory for the purpose.

For a standard method of making tests, it strikes me that we should, if necessary, differentiate between tests of the pipe itself and tests to determine the action of a pipe under the load which comes upon it in a trench. It may be that we can find a method of testing the pipe itself that will combine these; but it is not certain that such will be the case. It may be better to have a standard test for the pipe, and then to make separate tests to determine the pressures that will come upon the pipe in the trench and the strength of the pipe to resist them.

In the concentrated-load test referred to by Mr. Marston, we have used double supports at the bottom a short distance apart, say 2 ins. or so, and a single support or line of loading at the top. The double support at the bottom is for convenience; the pipe may be rolled into place and held without further support. The difference in bending moment between a double support at the bottom and a single support may be shown analytically to be very small. If necessary, an allowance can, of course, be made for that difference. In general I should say that that would not be necessary.

A portable testing machine was designed for our laboratory by Mr. Abrams. It was gotten up for the purpose of having a simple machine that could be carried right into the field along the ditch in the event of concrete pipe being made along the ditch. A simple framework of wood was built; a lever having 10 to 1 leverage extends out from this framework, and a load of cast-iron blocks or sand or other material may be gradually applied to the lever until the pipe breaks. The load may then be weighed and the strength of the pipe thus determined. An advantage of the concentrated-load test, it seems to me, lies in the fact that the conditions can be directly duplicated with other pipe, and we can then determine the modulus of rupture by calculation through the known analytical bending moment, and thus make satisfactory comparisons.

Mr. Talbot. I have myself been afraid that in the test where the pipe is bedded, there may be differences in bedding the pipe from time to time and possibly from person to person, and that these differences would make comparisons somewhat uncertain. That, however, is a question to be determined by experimental tests. What is wanted, it seems to me, is to have a simple, portable machine, a test which may be easily made by an unskilled operator in the field; something that will enable the drainage district to determine whether the pipe is strong enough for the purpose and whether it comes up to specifications.

Mr. Boynton. MR. C. W. BOYNTON.—I have watched Mr. Marston's work with a great deal of interest, and certainly he deserves credit for the attention he has given a subject which has been largely ignored by other investigators. I am inclined to believe, however, that a test for drain tile which approximates ditch conditions is not the best. I agree with Mr. Talbot in his opinion that the method which will be found best suited for tile testing will be one which will require only apparatus that is readily portable. A test which will approximate ditch conditions will necessarily confine the testing to laboratories where stationary equipment can be installed. I am of the opinion that the thing needed is a standard apparatus and test which can become a part of every drainage engineer's equipment and can be put into every tile plant in the country. The equipment must be inexpensive, simple and rapid in operation.

With these ideas in mind I have developed a device which consists of a frame carrying a screw which is operated through gears by a hand wheel, the specimen being placed between a horizontal member carried by the lower end of the screw, and a chair resting on a platform scale which is supported by the lower cross-pieces of the frame carrying the screw. The tile is supported in the chair by two half-rounds spaced 2 ins. on centers. The pressure is applied to the tile through the bearing piece, which is also faced with a half-round. These half-rounds are of soft wood measuring 1 in. on the flat side. After the tile is placed in the chair, and before the upper bearing piece is brought in contact with it, the scale beam is balanced. Upon the end of the beam is placed a bucket which is gradually loaded with water drawn by gravity from another vessel supported on the frame. The load is applied through the hand wheel by the operator who at the same time controls the flow of water into the bucket on the scale beam. **Mr. Boynton.**

At first I was of the opinion that the tile should be carried at the 60° point in a chair which would permit of adjustment to suit the size of the tile, but I met with two difficulties: first, the danger of the adjustment not being carefully made for the various sizes and the time required for making the adjustment; and second, to support the larger tile at the 60° point increased the load required to crush them, which would necessitate a heavier and therefore less portable outfit. I would prefer to load the tile along a single element, both top and bottom, but as it is difficult to hold them in the machine when so loaded I selected the support above described, believing that the advantage of having an arrangement which would automatically center all tile regardless of the size was of more importance than having the load applied at identically the same point on all sizes of tile. The test can be applied uniformly by an operator with little experience and will be comparable in every instance regardless of the size of the test pieces.

Mr. Orton. MR. EDWARD ORTON, JR.—I have been very much interested in Mr. Marston's paper, especially in what he said in regard to the absorption test as a mode of disclosing the value of this product. It is rather a sore point with clay workers who are manufacturing drain tiles, that such severe requirements are laid upon their product in this respect in some quarters, when cement tiles are accepted in the same market with no similar requirements being made. A prominent clay worker told me at this meeting that this situation has become somewhat acute in his district, and that the engineer expected him to furnish a product which would show an absorption of not more than 1.5 per cent., whereas the cement drain tiles with which he was competing had an absorption something like 6, 8 or 10 per cent. If a clay worker were permitted to furnish his material with an absorption of 6 or 8 per cent., it would mean the use of less fuel, the production of smaller quantities of distorted pipe, by reason of the lower maximum temperatures in burning, and less loss in the cooling. In short, his product would be less costly. The engineers, having been accustomed to using hard-burned

Mr. Orton. pipe, are very reluctant to accept any other than what they think is the best that the clay can make, but when fixing the requirements for cement pipes, they do not apply the same test, or at least not with the same severity. This the clay workers do not think just and equitable.

Whether the absorption test is equally significant as an index of quality in clay wares and cement wares is a question yet to be settled. It has usually been construed as of importance in judging of the ability of clay wares to stand frost, and it would presumably be of importance for the same purpose in the case of cement wares. But in the case of clay wares, at least, the absorption test has been very greatly over-valued as a mode of comparing different products, or products made from different clays, and it can only be considered a gage of frost insecurity in the roughest way, and where the percentages are large. In the case of wares made from the same clay and burned to various degrees of hardness, the absorption percentage does constitute a reliable index of the changes going on in the vitrification process, and also of the physical quality of the product. In any individual clay, the absorption will start with a comparatively high figure, which will decrease rapidly as the temperature rises, until a certain minimum is obtained, after which the clay will tend to become more absorbent again, due to the development of gas cavities in the vitrifying mass. These cavities are usually penetrable by water or fluids when the mass is submerged; so that the absorption curve, which at first tends to drop to a minimum, afterwards rises again. This minimum point is supposed to be the point of greatest density, the greatest strength and greatest frost resistance.

If the absorption test is used as a mode of determining the best drain tile which can be made from a given clay, I regard it as a very excellent mode of study, but it does not at all follow that the best point of burning in any one clay will show the same percentage of absorption as the best point in some other clay, because the degree of vitrification, the nature and kind of vitrification, and the nature of the bodies that two different clays are capable of producing, are quite distinct and separate. I have become somewhat unwilling, therefore, to see the absorption test set up as a rigid standard for clay products; because wherever the line is drawn, there will be clays whose physical qualities may be perhaps satisfactory as to the product in question, yet whose percentage of absorption will fall either above or below the line assumed as standard. **Mr. Orton.**

MR. BOYNTON.—I should like to ask Mr. Marston what percentage of the weight of earth in the ditch he assumes to be carried by the pipe? **Mr. Boynton.**

MR. A. MARSTON.—The percentage varies with the depth, being less for deep ditches. In some cases where we have made comparisons there is very little uncertainty, as the depths of fill were small. Let me cite a case, for example, which Mr. Boynton will recall as having occurred in Sac County, Iowa, with 36-in. drain tile. The ditch was 4 ft. wide, and the height of fill approximately 5 ft. The weight of the earth coming on the pipe could be measured with very little uncertainty, because the proportion between the height and the breadth of the fill in so shallow a ditch is little over unity. In a deeper ditch there would be more chance for difference of opinion as to the weight. **Mr. Marston.**

MR. G. T. HAMMOND.—I should like to ask Mr. Marston what allowance, if any, he makes in his pipe tests for the effect of the bell end of the pipe? **Mr. Hammond**

MR. MARSTON.—In the tests, as we make them, the entire length of the pipe is subjected to pressure, including the bell. The weights are reduced to pounds per linear foot by dividing the weights by the entire length of the pipe. **Mr. Marston.**

MR. HAMMOND.—In all pipe crushing tests of which I have information, the effect of the bell on the result of the test is an unknown quantity. The bell is subject to more variation in thickness of wall, depth and strength than the barrel of the pipe. This variation may not greatly affect the value of the pipe as laid in a sewer, but it may cause considerable variation in the crushing tests. **Mr. Hammond**

In Brooklyn, we have been testing vitrified pipe, as well as concrete pipe, for a period of about six years. Our tests have been made on pipe ranging in size from 6 to 42 ins. in diameter in vitrified, and from 6 to 24 ins. in concrete tile. We have also tested reinforced concrete pipe up to 48 ins. in diameter.

Our first testing machine, as designed by Mr. E. J. Fort,

Mr. Hammond. Chief Engineer of the Sewer Bureau, consisted of a long lever of timber passing through rigidly placed uprights of timber and fastened at one end, so as to act like a nut-cracker, as shown in Fig. 2. A fulcrum was provided, which applied the crushing load through a steel roller to a movable cover on the testing box. The pipe tested was imbedded in dry sand contained in a strongly braced, iron-bound box, the ends of which were removable, and provided with apertures of the size of the bell-end and of the spigot-end of the various pipes. When the pipe was in place, its interior could be seen constantly. Pressure was applied by means of an iron bucket of about one cubic yard capacity, hung on the outer end of the lever, which was slowly filled with water during the test by means of a hose from a city main. The pressure exerted at the fulcrum was indicated in pounds on a scale placed in the bucket, which showed the depth of water and pressure. The water had, for the most part, to be wasted between tests, and this was a disadvantage. Each of the tests required a comparatively long time; otherwise the machine was satisfactory.

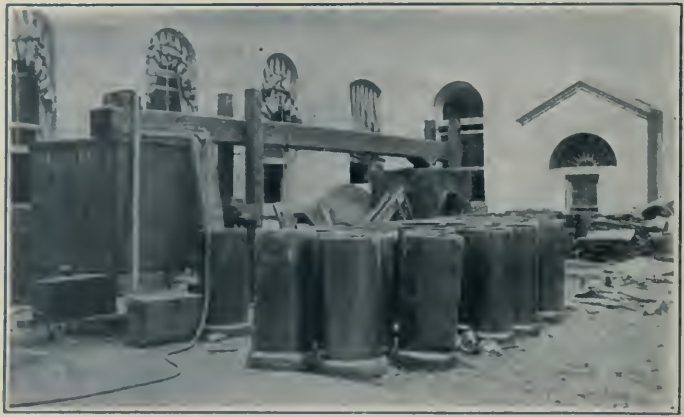


FIG. 2.

Since 1907 we have had a well-equipped testing laboratory, provided with a Riehlé crushing machine, shown in Fig. 3, capable of taking a 48-in. pipe, and of exerting a maximum pressure of 75,000 lbs. The principal purpose of our tests is to

Mr. Hammon

establish and maintain a standard quality of pipe for our contract work, and as we lay about thirty or forty miles of this per year, the tests have to be quickly and readily applied, in order that the work of laying the sewers may not be delayed by the tests. The effect of the plant has been to greatly improve the quality of pipe supplied to us.

The knife-edge crushing test, as employed by us, seems to answer our requirements admirably, and to give excellent comparative results. In this test the pipe is not imbedded in sand and we believe that the effect of the bell on the result is reduced to a minimum. Our results seem to indicate that imbedding the pipe, bell and all, increases the effect of the bell to a maximum, disturbing the result of the test to an appreciable extent, and to an extent which cannot be readily ascertained. The bell adds to the strength of the bell-end of a pipe exposed to a crushing load, and the effect will vary with the length of the barrel of the pipe and also with the various conditions involved in the test. The effect is greatest on short lengths of pipe; thus, a 24-in. length of pipe with a bell will give a higher crushing load per foot than will a 36-in. length with a bell, the barrel portion of which is of equally strong material. Again, a pipe without a bell

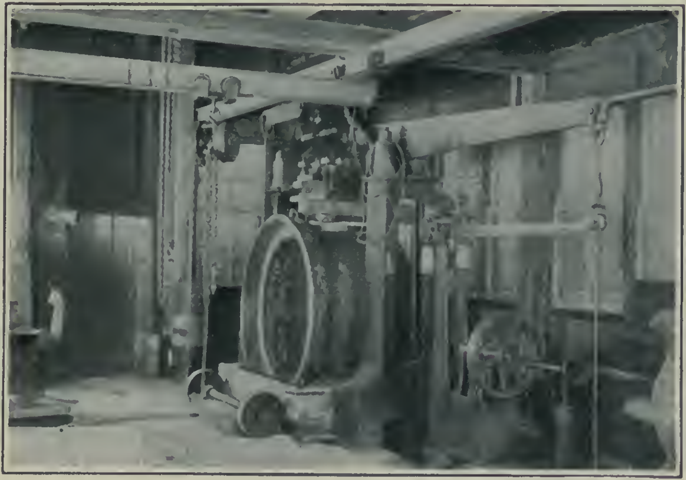


FIG. 3.

Mr. Hammond. will break under a lower crushing load than a pipe of the same size of no better and no stronger material, provided with a bell. The greater the number of bell joints in a given length of sewer the stronger will be its resistance to crushing from the load of covering. Numerous joints are a disadvantage in a sewer for many important reasons, and as few as possible should be placed. It seems obvious that in testing pipe the strength of the barrel should be ascertained with as little interference from the bells as practicable.

In our crushing tests we apply the crushing load, through a strip 1 in. wide, to the length of the barrel of the pipe, excluding the bell-end. This strip, or knife edge, as we call it, is approximated to the pipe surface with plaster of Paris, which is placed on the surface of the pipe between two wooden strips, into which the "knife" fits, and into which it is put while the plaster is soft; after the plaster has set, the strips are removed from the pipe and any excess is scraped off. The pipe for testing is bedded upon sand, carefully prepared. The reaction of the pipe under test appears to be developed over about 10° of the cylinder resting in this prepared bed of sand. The bell is not imbedded. We have used a double knife edge, one edge above and one below the pipe, and we have also tried a double support under the pipe barrel. There is, of course, some variation in the results obtained by these different methods, but this variation is not of sufficient importance to be taken into account for our purpose, which is to make a standard comparative test.

In our opinion, it would undoubtedly be better to use a knife edge on both top and bottom of the pipe, not only because it produces greater uniformity in the test, but also because the unit strength of the material can be more easily and exactly determined from the test. It is our purpose to make this change eventually in our method of testing.

Mr. Wig. MR. R. J. WIG.—In regard to the absorption test as a determination of the quality of concrete, I would say we have made a great number of absorption tests, and have found that the absorption of the concrete is as much dependent upon the character and grading of the aggregate as upon the quantity of cement used.

Mr. Meriwether

Mr. Meriwether.—I should like to say a word about absorption. Mr. Rankin, the engineer in charge of the Newark sewers, made some tests about a year and a half or two years ago on absorption and abrasion of material for sewer inverts. I was present at a number of these tests. After he made the absorption tests he made abrasion tests on the same samples, and the results of the abrasion tests followed almost directly those of the absorption tests. In comparing the clay samples with the concrete samples, the clay samples which absorbed about 2 per cent. did not show as good results in abrasion as the concrete samples which absorbed about 6 per cent. I think it would be unfair, therefore, to judge the abrasion of the two materials according to the degree of absorption. In other words, the concrete pipe may absorb more moisture than the clay pipe and still resist abrasion better.

Mr. Wig.

Mr. Wig.—The resistance to abrasion would be a function of the aggregate in two concretes made in the same proportion. If the grading of the aggregates were the same, the absorption of the two concretes might be identical and yet if one were made with a soft limestone and the other with a hard limestone or granite their abrasive qualities would be very different.

THE FRITZ ENGINEERING LABORATORY OF LEHIGH UNIVERSITY.

By F. P. McKibben.

The Fritz Engineering Laboratory has been presented to Lehigh University by Mr. John Fritz, of Bethlehem, Pa., to enable the students of that institution to study experimentally various problems connected with the materials of construction and hydraulics. There are four main sections in the Laboratory, as follows: (1) a general testing section including the testing machinery and a small machine shop; (2) a room for testing cement; (3) a room for making and storing concrete specimens, such as beams and cylinders; and (4) a hydraulic section.

The Building.

The laboratory building is of the steel mill-building type, 91 ft. wide and 114 ft. long, with a main center aisle between two side aisles of less height and less width. The steel frame carries the roof and traveling crane only, the side and end brick walls being carried on independent foundations. The problem to be solved in planning the building was to secure a structure of sufficient height to accommodate a vertical screw testing machine capable of taking a specimen 24 ft. long, to secure at least two levels for the hydraulic laboratory, and to have concrete and cement rooms, which while in close proximity to the main testing room, could be separated therefrom at any time in order to prevent dust from passing into the main room. At the best, cement and concrete laboratories are dusty and should not be placed where there are testing machines.

As ample light is of prime importance in a testing laboratory, numerous windows are provided in the side walls, end walls and in the clerestory, and a skylight 9 ft. wide and 84 ft. long is placed in the north roof over the main aisle. Steel frames with movable sections are provided for all side- and end-wall windows. The main aisle is 49 ft. 2 ins. center to center of crane columns, and has a clear height of 40 ft. which allows the traveling crane to pass over the 800,000-lb. testing machine. This width affords sufficient clearances at the sides of the large testing machine, which stands practically in the center of the building. A less width for this vertical machine is not advisable. In fact, the larger the proportion of area commanded by the traveling crane the better, but if side aisles are used they should be at least 20 ft. wide. The section set apart in the east end of the building for the hydraulic laboratory occupies, in addition to a depressed area 39 ft. by 54 ft. (10 ft. below the main floor level), an elevated platform of the same area 10 ft. above that level.

Sufficient floor area has been provided for additional equipment, such as a road-materials laboratory, a large torsion machine, and an impact machine; and after these have been installed there will be ample room for future growth. Table I shows the floor areas of the various sections.

TABLE I.—FLOOR AREAS OF SECTIONS IN FRITZ LABORATORY.

Section.	General Testing.	Cement.	Concrete.	Hydraulic.	Office.	Total.
Floor Areas, sq. ft. .	6,772	840	1,320	4,212	250	13,394
Clear Height, ft	40 and 20	12	9.5	20 and 20	12	

THE GENERAL TESTING SECTION.

This division contains all testing machines except the briquette machines, which are in the cement section. It is so arranged that classes can be divided into small groups of three or four students each, and each group can be provided with a machine with which to conduct tests. Occupying the western portion of the main aisle, which is served by the crane and which is connected to the exterior by a doorway 10 ft. wide, this general testing section is well arranged to facilitate the handling of large or heavy test specimens. For example, wagons carrying steel or concrete beams or columns to be tested are drawn into the building through the large doorway and their loads removed by the traveling crane. This crane has a span of 47 ft. 2 ins. center to center of runway beams, is operated by three direct-current motors, and has a safe capacity of 10 tons. In planning a laboratory in which tests on large specimens are to be made, it is very important that rapidity and ease of handling receive proper consideration, and a traveling crane is of the greatest importance.

EQUIPMENT OF GENERAL TESTING SECTION.

Table II shows this equipment so far as the machines are concerned and there are, of course, a great many accessories which need not be mentioned here.

The 800,000-lb. machine is of the two-screw vertical type capable of testing a column of 24 ft., a tension specimen of corresponding length, or a transverse specimen 10 ft. long or less. The clear distance between the screws is 35 ins. Although the weighing table is only 10 ft. 6 ins. long, it is possible to test beams up to 25 or 30 ft. in length by using extension girders on the weighing table. By means of standard weights this machine has been found to be sensitive to 50 lbs.,—that is, 50 lbs. produces an initial movement of the scale beam at a load of 20,000 lbs. or of 800,000 lbs.,—and by means of proving levers its accuracy up to 20,000 lbs. has been found to be within its range of sensitiveness.

TABLE II.—TESTING MACHINES IN GENERAL TESTING SECTION, FRITZ LABORATORY.

Type of Machine.	Capacity, lbs.	Speeds, ins. per min.		Nominal Horse-power of Motor.	Speed of Motor, R. P. M.
		Max.	Min.		
Universal	800,000	8	0.05	20	1,120
"	300,000	3	0.021	7.5	1,120
"	100,000	4	0.04	3	1,700
"	50,000	3	0.38†	2	1,120
"	50,000	3	0.38†	2	1,120
"	50,000	3	0.38†	2	1,120
"	50,000	8	0.05†	2	1,120
"	50,000	8	0.05†	2	1,120
Tension and Compression	20,000	..	....	Hand-power	....
Wire Tester	20,000	..	....	Hand-power	....
Cold Bend	1.5-inch diameter bar	..	....	2	1,700
Torsion	24,000*	..	....	Hand-power	....

The 300,000-lb. machine is very sensitive and accurate, and its slow speed makes it useful for general testing. The extension arms on the weighing table are 21 ft. long, so that a beam up to practically this length can be easily tested under a load not exceeding 300,000 lbs. The top surface of the weighing table and extensions is 2 ft. above the main floor and this height has proved satisfactory. At first thought it might appear desirable to have the machine lower, with the extensions and weighing table below the floor level, so that when not in use the extensions could be covered with steel plates or other removable flooring; but this arrangement would necessitate working in a pit when the machine is in use, and on the whole the position here adopted for the machine is the best possible.

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* Inch pounds.

† Hand-power also available.

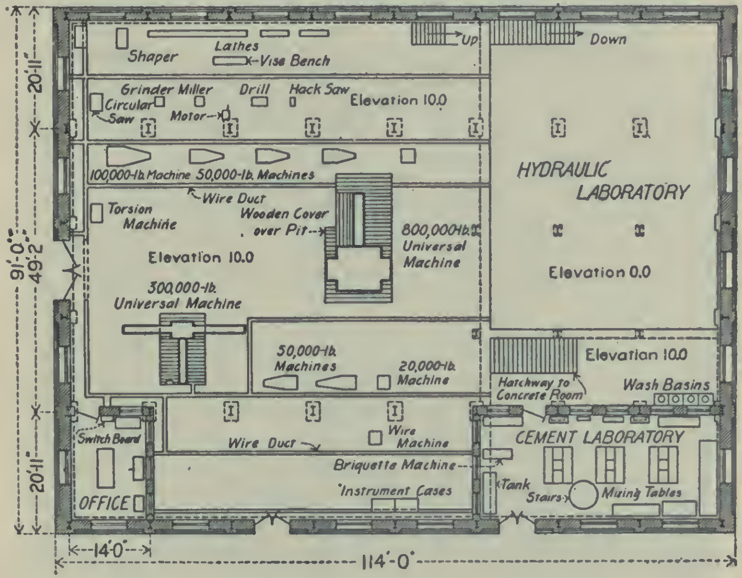

FIG. 1.—Main Floor Plan, Fritz Laboratory.

The smaller universal machines are used not only for ordinary tension, compression, and shearing tests, but also for transverse tests by placing the test specimens on Bethlehem "H" sections laid on the weighing tables, the "H" sections thus serving as extensions to the weighing tables. In this manner the problem of giving transverse tests to several small sections of students simultaneously is solved without having special transverse machines.

The repair shop equipment consists of one drill press, one high-speed lathe, two metal turning lathes, one milling machine, one shaper, one circular saw, one power hack-saw and one grinder.

All of these machines are belt driven, the power being furnished by a 7.5-H.P. motor.

Electric Equipment.

All electricity for lighting the building and for driving the testing machines is 2-phase, 60-cycle, alternating current at 110 and 220 volts respectively, and is supplied from the University power station. Direct current, obtained from a local company, is used for the traveling crane and for this purpose is to be preferred to alternating current, and, except for the tendency to spark, is better for testing machines because of better speed-control. However, if the gearing on the machines is properly arranged to provide for a very slow speed of say 0.05 in. per minute, a rapid speed of 8 ins. per minute, and at least one intermediate slow speed, the alternating current is perfectly satisfactory for ordinary universal machines and in the present instance was the only kind obtainable from the University station without the use of a rotary-converter. To drive centrifugal pumps for a hydraulic-laboratory water supply, direct current is much to be preferred and, although a

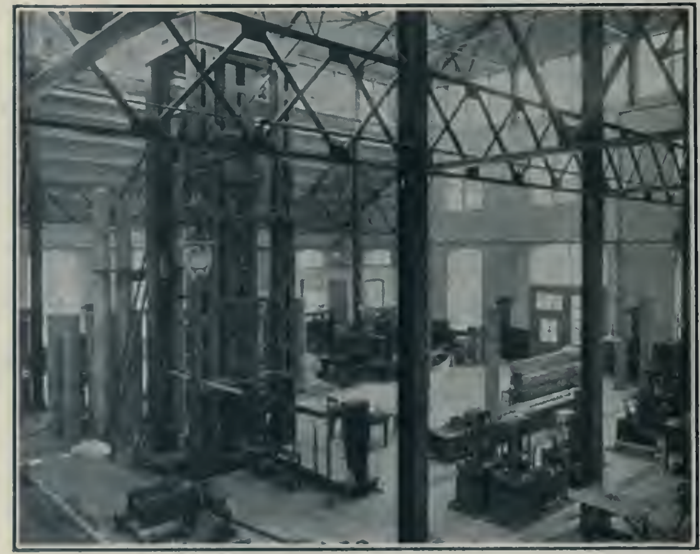

Fig. 2.—General Testing Room, Fritz Laboratory.

variable-speed induction motor can be obtained, yet the possible variations in speed are limited to such a narrow range as to prevent that nicety of speed-control obtainable with a direct-current motor. With proper gearing, therefore, either alternating or direct current can be used for ordinary testing machines, but direct current should be used for hydraulic laboratories and for certain classes of testing machines. The motors of 20-H.P., 7.5-H.P., and 7.5-H.P., for driving the 800,000-lb. machine, the 300,000-lb. machine, and the tools in the repair shop, respectively, are furnished with auto-starters. The motors for the hydraulic section have not yet been installed, but will be of the variable speed alternating-current type.

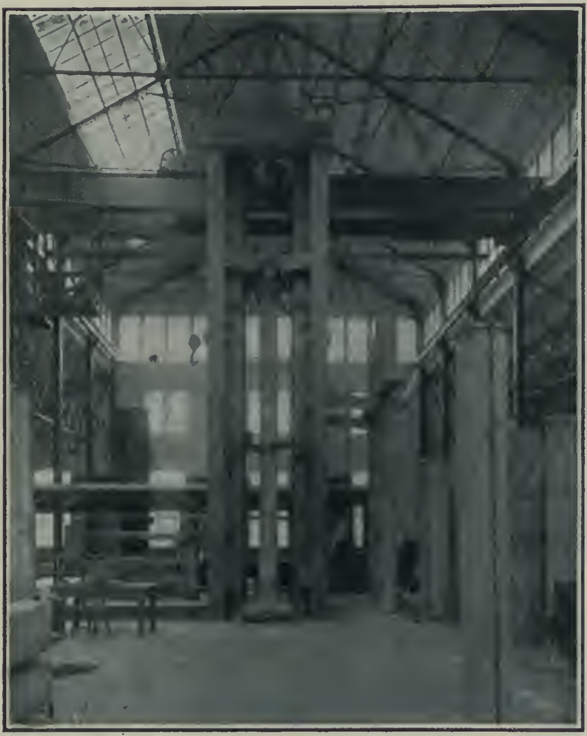

Fig. 3.—General Testing Section, Fritz Laboratory, showing 20-ft. column in 800,000-lb. machine.

THE CEMENT AND CONCRETE SECTIONS.

The cement testing section occupies a separate room on the main floor level and is so planned that twelve students can work at one time. The equipment of this section consists of well-arranged tables for making cement specimens, the ordinary storage tanks, briquette testing machines, and all apparatus necessary for making standard cement tests.

All concrete specimens such as beams, cubes, and cylinders are prepared in the concrete room, which covers not only the entire area under the cement room but also extends beyond this area and underneath a portion of the main testing room to which it is connected by a large hatchway through which the heavy concrete specimens such as beams can be hoisted into the main testing room with the aid of the traveling crane. The equipment in the concrete section consists of a mixer, the necessary bins for sand and stone, and molds for beams, cylinders and cubes.

An important feature in a cement and concrete laboratory is a proper drainage system arranged to prevent pipes from becoming obstructed by cement or sand. This problem has been solved by placing under the concrete room a large concrete sump into which all drainage must flow and in which there is a partition or weir over which the water passes; the sand, cement, or other matter is caught in the sump, from which it is easily removed. This arrangement is satisfactory.

The hydraulic section is now being equipped.

This laboratory has been built and equipped under the personal supervision of Mr. Fritz, by Mr. G. A. Chandler, Associate Professor L. D. Conkling, Mr. S. H. Ingberg, and the writer, and is a part of the Civil Engineering Department of the University.

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